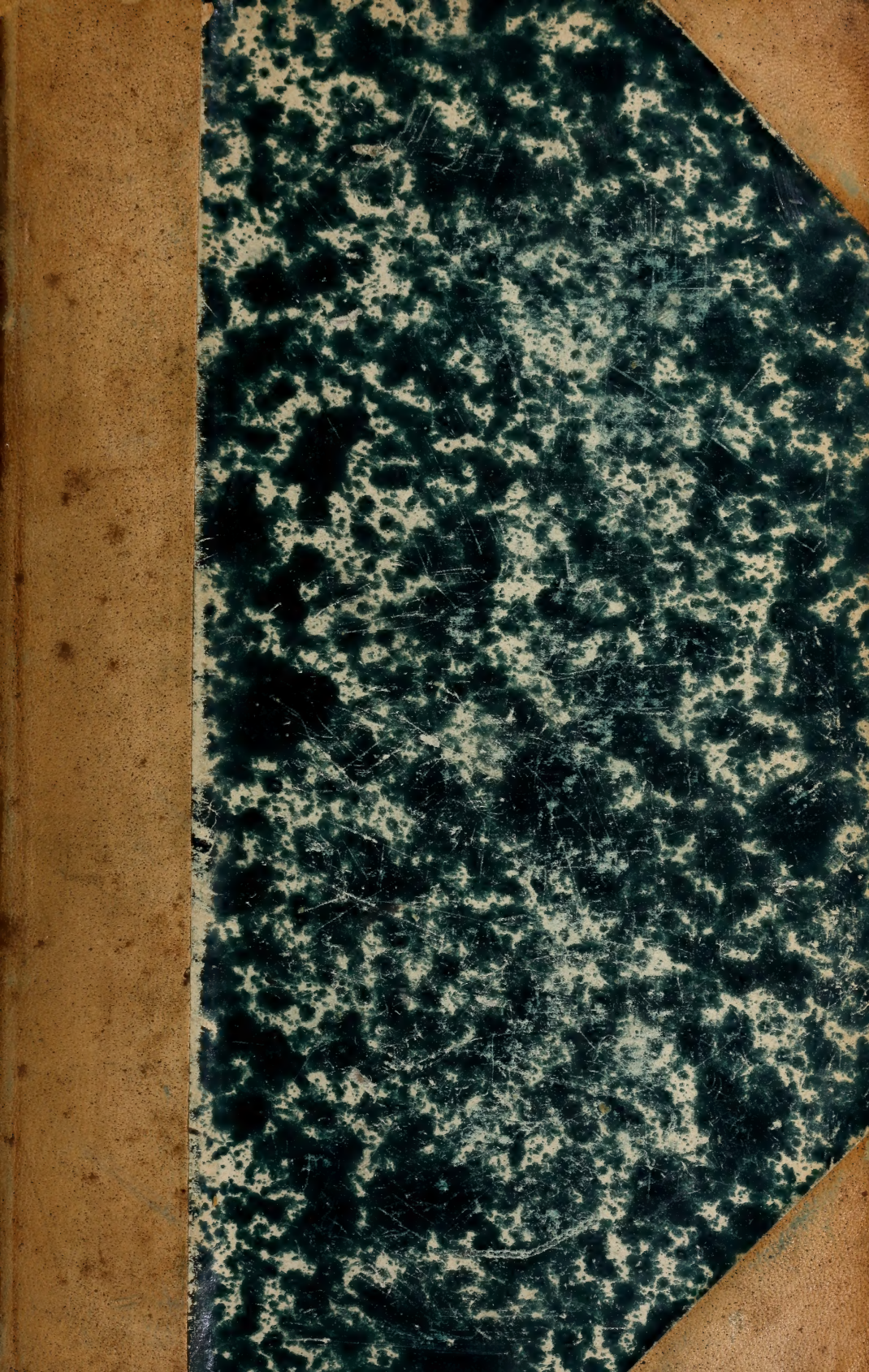






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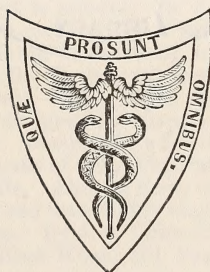
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EDITED BY
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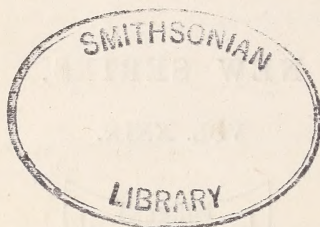
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The following works have been received:—

The Pathology and Treatment of Stricture of the Urethra, both in the Male and Female. Being the Treatise for which the Jacksonian Prize, for the year 1852, was awarded by the College of Surgeons of England. By HENRY THOMPSON, F. R. C. S., M. B., Lond., Surgeon to the St. Marylebone Infirmary, Fellow of the Medical and Chirurgical and of the Pathological Societies, etc. London: John Churchill, Princess Street, Soho, 1854. (From the Author.)

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Le Moniteur des Hôpitaux. Rédacteur en Chef, M. H. DE CASTELNAU. Sept., Oct., Nov. 1854.

Revue de Thérapeutique Médico-Chirurgicale. Par A. MARTIN-LAUZER. Sept., Oct., Nov. 1854.

Archives d'Ophthalmologie Comprenant les travaux, les plus importants sur l'Anatomie, la Physiologie, la Pathologie, l'Hygiène, et la Thérapeutique de l'Appareil de la Vision. Par M. A. JAMAIN. June, July, Aug. 1854.

Annales Médico-Psychologiques. Par MM. les Docteurs BAILLARGER, BRIERRE DE BOISMONT, et CERISE. Oct. 1854.

The Dublin Quarterly Journal of Medical Science. Aug., Nov. 1854.

Monthly Journal of Medical Science. Aug., Sept., Oct., Nov. 1854.

Medical Times and Gazette. Sept., Oct., Nov. 1854.

Dublin Medical Press. Sept., Oct., Nov. 1854.

The British and Foreign Medical-Chirurgical Review. Oct. 1854.

The Journal of Psychological Medicine. Edited by FORBES WINSLOW, M. D., D. C. L. Oct. 1854.

The Dublin Hospital Gazette. Sept., Oct., Nov. 1854.

The Medical Chronicle, or Montreal Monthly Journal of Medicine and Surgery. Edited by W. WRIGHT, M. D., and D. C. MCCALLUM, M. D. Oct., Nov., Dec. 1854.

The Buffalo Medical Journal. Edited by A. FLINT, M. D., and S. B. HUNT, M. D. Oct., Nov., Dec. 1854.

Memphis Medical Recorder. Edited by N. P. MERRILL, M. D., and C. T. QUINTARD, M. D. Sept., Nov. 1854.

Charleston Medical Journal. Edited by D. J. CAIN, M. D., and F. PEYRE PORCHER, M. D. Nov. 1854.

The American Journal of Science and Arts. Conducted by Profs. B. SILLIMAN, B. SILLIMAN, JR., and J. D. DANA, in connection with Profs. GRAY, AGASSIZ, and GIBBS. Nov. 1854.

The New York Journal of Medicine. Edited by SAMUEL S. PURPLE and STEPHEN SMITH, M. D. Nov. 1854.

The Ohio Medical and Surgical Journal. Edited by JOHN DAWSON, M. D. Nov. 1854.

The North-Western Medical and Surgical Journal. Edited by N. S. DAVIS, M. D., and H. A. JOHNSON, M. D. Oct., Nov. 1854.

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The Western Lancet. Edited by L. M. LAWSON, M. D., and T. WOOD, M. D. Oct. 1854.

The Virginia Medical and Surgical Journal. Edited by J. B. McCRAW, M. D., J. F. PEEBLES, M. D., and G. A. OTIS, M. D. No. XVIII. 1854.

The Southern Journal of the Medical and Physical Sciences. Edited by Drs. KING, JONES, CURREY, and WOOD. Sept., Nov. 1854.

Nashville Journal of Medicine and Surgery. Edited by W. R. BOWLING, M. D., and PAUL F. EVE, M. D. Sept., Nov. 1854.

New Orleans Medical News and Hospital Gazette. Edited by Drs. CHOPPIN, BEARD, and SCHLATER. Sept., Oct., Nov., Dec. 1854.

The New York Medical Gazette and Journal of Health. Edited by D. M. REESE, M. D. Oct., Nov., Dec. 1854.

The Medical Examiner. Edited by SAMUEL L. HOLLINGSWORTH, M. D. Oct., Nov., Dec. 1854.

New York Medical Times. Edited by H. D. BULKLEY, M. D., and J. G. ADAMS, M. D. Oct., Nov., Dec. 1854.

St. Louis Medical and Surgical Journal. Edited by Drs. LINTON, MCPHEETERS, MOORE, and DEMING. Sept. 1854.

The New Jersey Medical Reporter. Edited by S. W. BUTLER, M. D., and JOS. PARRISH, M. D. Oct., Nov., Dec. 1854.

The Boston Medical and Surgical Journal. Edited by J. V. C. SMITH, M. D., and GEO. S. JONES, M. D. Oct., Nov., Dec. 1854.

The American Journal of Insanity. Edited by the Officers of the New York State Lunatic Asylum. Oct. 1854.

The Stethoscope. Edited by a Committee of the Medical Society of Virginia. Oct., Nov., Dec. 1854.

Southern Medical and Surgical Journal. Edited by L. A. DUGAS, M. D. Oct., Nov., Dec. 1854.

The New Hampshire Journal of Medicine. Edited by GEO. H. HUBBARD, M. D. Oct., Nov., Dec. 1854.

The American Medical Monthly. Edited by E. H. PARKER, M. D. Oct., Nov. 1854.

The Peninsular Journal of Medicine. Edited by A. B. PALMER, M. D., and E. ANDREWS, M. D. Sept., Oct., Nov., Dec. 1854.

The Western Journal of Medicine and Surgery. Edited by LUNS福德 P. YANDELL, M. D. Oct., Nov. 1854.

Iowa Medical Journal. Conducted by the Faculty of the Medical Department of Iowa University. Oct., Nov. 1854.

American Journal of Pharmacy. Edited by WILLIAM PROCTER, JR. Nov. 1854.

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Engraved by J. H. Smith from the original portrait by the artist J. H. Smith

NATHANIEL CHAPMAN, M.D.

N Chapman, M.D.

Portrait of Nathaniel Chapman, M.D., by J. H. Smith, 1850.

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ART. I.—*Biographical Sketch of Nathaniel Chapman, M. D., late Professor of the Theory and Practice of Medicine and of Clinical Medicine—A Discourse delivered, October 6, 1854, before the Trustees, Medical Faculty, and Students of the University of Pennsylvania.* By SAMUEL JACKSON, M. D., Professor of the Institutes of Medicine. [With a Portrait.]

OUR duties to the dead do not cease, nor are our obligations to them cancelled, when the grave closes on their mortal remains. A deep, a weighty debt is due by the living to the illustrious dead, by the present to the time past.

All that we possess truly valuable; our civilization, our social and political organizations, our knowledge, the arts, letters, our science, philosophy, and religion, are rich and inestimable inheritances that have passed to us in lineal descent. We have no title to them as our productions; they are not the fruits of our labour; they have not germinated and been developed under our cultivation. They are the accumulated treasures, the creations of the intellectual faculties with which man is endowed by his Creator—laboriously cultivating and exploring the world of thought and nature through successive ages. They descend as heir-looms of humanity, inalienable possessions of our race, from generation to generation. Each receives them in trust, to preserve, to perpetuate, and to complete; and is bound to transmit them with augmented value, from higher opportunities of cultivation, until the perfectness of knowledge and the universality of truth shall dispel ignorance, error, and the false—the sources of misery, vice, and crime.

This vast amount of human knowledge is collected, condensed, and improved by those devoted to its cultivation and teachings. They gradually

eliminate from it all that time and experience have proved doubtful in authority, or untrue as fact. They embody, in a compact form, the thoughts, the ideas, the ascertained truths of all preceding time and investigations.

In them are incarnated thought, the established ideas and governing beliefs that constitute the essentialities of human existence, and control human actions and destinies. Abstract from man his inherited knowledge, and these radical established idealities of his intellect, and what is he but a gross animal, a mere barbarian? Those whose lives are devoted in this manner to those great offices, are the most illustrious of benefactors. They win for man new capabilities; enlarge his intellectual possessions by new discoveries; augment his power over nature, by compelling her to yield up, for his daily uses and enjoyments, the forces by which she works her own wonderful operations. By these scientific conquests, the material advantages of our existence daily expand; while humanizing influences are not less active in various ways, purifying man's moral nature.

Such men are the really great, the nobles of nature, heroes that lead the van of the struggling ranks of intellectual progress, of human amelioration, of the extinguishment of the still existing barbarism of society, and hasten the onward march of civilization.

It cannot be justly said that mankind have underrated the inestimable advantages they have received from these benefactors of the race, or have ungratefully treated them. In unenlightened times they were regarded as possessed with more than mortal endowments. They have been deified after death; temples have been erected for their worship, and religious rites instituted to celebrate their attributes.

In later times, and in our day, every method that love, affection, and grateful remembrances could devise, has been invoked to perpetuate their resemblances and forms, to record their actions, to acknowledge their benefits, to blazon their names, to celebrate their praise, and award to them "the honourable meed of fame."

There is one, whose name within these walls has long been, and will be, "familiar as household words," to whom this justice is to be rendered; to whom this public tribute is due.

The late Professor Nathaniel Chapman was long the ornament, the boast, and the pride of this school. He shed over it, in its brightest days, its brightest lustre. He will ever be ranked amongst its most illustrious teachers, as he was acknowledged the most eminent of the physicians of his time.

Unhappily, Professor Chapman, by premature infirmities, was separated from his professional and professorial duties before his earthly course was run. The setting sun was obscured, and its lustre dimmed by a darkened horizon. His death did not fall as an unexpected calamity; yet it called forth deep feelings, and awakened the sympathies of his professional associates, and a large circle of personal friends. The medical schools, the College of Physicians, the Philosophical, and other societies of this city, expressed their

esteem for his memory, and eulogized the varied endowments of his character.

The professional merits and services of Professor Chapman require a more extended notice and fuller attestation. This duty I might claim as peculiarly belonging to myself, from the relations so long subsisting between us, and the lasting obligations I have been under to him. These I have always felt were most honourable to myself, which I look back on with grateful and pleasing recollections.

He was my early patron, benefactor, and friend. He sought me out in the day of adversity, when friends fall away. I had never crossed his threshold; he had heard of difficulties and troubles that involved me; and, with generous impulse, he called on me unsought, urged me to enter the profession which I had left some years previous, promised me success, and encouraged me by the example of Boerhaave, who commenced, at a late period of life, his professional career.

I felt grateful for the interest shown towards me, but was compelled to decline his advice. At that time it was impossible. I was bound by ties and duties that compelled me to struggle on with my entanglements, whatever might be the result.

The casualties of time and events at length left me free to act, wrecked in fortune, heavily encumbered, with no other resources than my professional attainments and individual exertions. But with no field of action open to me, how could they be made available in this emergency? I had given two courses of lectures in the College of Pharmacy, an institution I had assisted in establishing. It occurred to me that a course of lectures on *Materia Medica*, with which I was perfectly familiar, connected with *Therapeutics*, could be made useful to students and profitable to myself.

Unknown to them, in no communication with them, how could I compass my object. I felt that the former kindness and interest shown by Professor Chapman justified me in seeking his aid. I called on him, laid before him my scheme, and asked him if he felt free to recommend my course to the students. "Yes, and at once," was his reply. "Doctors Dewees, Horner, and myself have just commenced a summer course. Come and join us." I had no knowledge of this arrangement, so opportune for my success. That day week was appointed for my opening lecture.

I returned to my solitary home. I was alone in my native city, without relative or connection, without kith or kin. But a weight was off my heart; my spirits were buoyant; the darkest hour had passed, and light was breaking on a long night of adversity.

The opportunity for instant exertion, the only favour I could have asked or accepted, had been nobly afforded me. My success must depend wholly on myself.

I wrote my lecture. Though I had left the practice, I had not abandoned medicine as a science. My principles were not those then prevalent in our

medical schools. They were drawn from the ancient sources of our art, rich in imperishable practical facts, drawn by profound observers from a study of disease in nature, and not in books. For fifteen years, a portion of my leisure time had been devoted to this study in the Logonian Library, where are to be found every authority of the Greek and Latin medical writers, translations of the principal Arabian, and the most eminent of the Italian, French, and German medical authors, from the revival of letters to the seventeenth century. I had imbibed the doctrines of the school of Montpellier, from the works of Bordeu and Barthez, which had ever proved faithful to the fundamental principles of the Hippocratic writings, and had not deviated from the route, the only true one, enlightened observation and intelligent experience, on which they had placed medicine. I was familiarized with the physiological views of Haller, Hunter, and Bichat—the last then little known in this country. From these sources I had formed my method of viewing the facts of medicine; they governed my principles, and, though old, they had the quality of being novel.

My lecture was delivered, Professor Chapman sitting beside me. The principles announced were in direct conflict with his most cherished medical opinions.

This was not intentional. My mind filled with the subject, no personal considerations had intruded on my thoughts.

After the lecture, some students remarked to me the singularity of my opposing the Doctor's principles in his own lecture-room, in his presence; and this recalled me to the fact. With most persons a mistake like this would have marred my fortune; but Professor Chapman's cordial congratulations and desire for me to continue the course, assured me that he had not misunderstood me, or misconstrued my intentions and feelings.

From imperfect health, in almost daily suffering, with broken-down forces, and unintermitting work on hand, I was compelled to economize strength and time. I could make no visits, and our meetings were merely casual or professional. I had no opportunities, nor were they thought of, to conciliate his favour through personal influence.

Two years subsequently, on entering the lecture-room, I was surprised by the students warmly greeting me on my appointment as Professor Chapman's assistant in the University. He had announced it to the class that morning. It was the first intimation I had received of such an intention. I called on him soon after, at his request. He congratulated me on my appointment, and mentioned that "he had kept his intended application to the trustees to himself, as he would not excite expectations that might not be fulfilled." He continued, "you and myself agree in some opinions. We differ in others. Teach what you believe to be true."

The egotism of this narrative will, I hope, be pardoned. It is grateful to acknowledge generosity so noble; and a trait like this gives a deeper insight into the real character, into the inner man, than the most elaborate eulogy.

Toleration of differences of opinion is a lofty virtue. It is rare to find it even where it should most abound; in the disciples of religion, of science, and of philosophy. Bitter feelings, deep animosities, personal hatreds, violence, and persecution, too often usurp the place of benevolence, forbearance, and philanthropy. Yet these ought to be the productions of those elevated regions of intellectual thought, in the calm of which the passions should be quelled, and the turbulence of contentious thoughts and struggling interests be stilled. "The fruit of the spirit is goodness and truth."

Professor Chapman, from his reading, experience, and the past history of Medical Science, was decidedly skeptical of the truth and permanency of medical doctrines. They are convenient for a time, and necessary, he thought, as the rallying-points of facts that would be otherwise scattered and disjointed. While he believed the time for truthful conclusions had not arrived, he was not opposed to the investigations of others more sanguine and enthusiastic; or offended by a confidence in theoretical principles differing from his own. He did not acknowledge their conclusions, but remained firm in his own opinions, in which he had full confidence as safe guides in his practice.

In undertaking, at the request of the Medical Faculty, to deliver an eulogium on Professor Chapman, the task would be to me most grateful could I feel confident of my ability to do justice to the subject. I most sincerely regret my incompetence to perform this duty in a style and manner I could regard as worthy of his character, and befitting his merits. Neither time nor my talents permit me to attempt to portray the varied attributes of character that gave life and joyous animation to the social circle; that made him the beloved physician, inspiring confidence in illness, and dispelling the gloom of the sick chamber; that rendered him the most popular and honoured of teachers by annual classes of students gathered from all parts of this wide-spread land, and called forth the universal respect and reverence of the whole American Medical Profession.

The point of view I shall select as the most appropriate on this occasion, in which to present to you our late venerated colleague, is in his professional character, and chiefly in his connection with our science, as the guide and guardian of its theory and practice, in his position as Professor in this school. He held in this University, for more than thirty years, the most important chair in a medical faculty. The duties devolving on its incumbent are deeply responsible. They demand a gifted intellect, high qualities of character, and extensive knowledge, accurate, solid, and practical. In reviewing this phase of his life, it will be necessary, in order that you may fully understand the important services he performed for our science, to exhibit it to you in its theory and practice at the time when he was called to undertake the office of recreating medicine into a practical method, of new moulding the medical mind of a large mass of the profession, and training them to the responsible duties of practitioners.

NATHANIEL CHAPMAN was a Virginian by birth, a Philadelphian by adoption.

From the time he made this city his permanent home, engaged earnestly in his multiplied pursuits, he never left it for recreation or relaxation. His visits abroad were professional.

His ancestry was a highly respectable family of Welsh origin. One branch was amongst the earliest colonists that settled Virginia. The family, his immediate progenitors, migrated from Maryland, where the original estate on the banks of the Potomac, opposite Mount Vernon, has continued in possession of the family for a century and a half.

Of his boyhood days little is known. His principal education was obtained at the Classical Academy in Alexandria, established by General Washington. He never received the advantages of a regular collegiate course of study.

With an educational training so imperfect, he must have been for the most part self-educated.

Dr. Chapman commenced his medical studies with Professor Rush, in 1797, and graduated in 1800, in the University. While a student, he became the associate and intimate companion of the literary characters of the day, at the head of whom was Mr. Denny, a highly polished writer, and editor of the *Portfolio*, a periodical, at the time possessing the highest repute. Chapman was a contributor to its pages, which, at that period, was regarded as a mark of distinction.

The year subsequent he visited Great Britain for the completion of his medical education. He remained abroad during four years in pursuit of this object. The first year was spent in London, as a private pupil of the celebrated Abernethy. The Edinburgh Medical School was the attraction that carried him abroad. At that time, the celebrity it had acquired from its Munros, Cullen, Brown, and Gregory, had not been eclipsed by the Paris or German schools, or rivalled by those of London and Dublin. The medical school of the Scotch metropolis was the cynosure of American physicians during the colonial period, and continued to be so until within the last twenty-five years. Most of the eminent medical men of this city, New York, and Boston, of the latter part of the last century, were its alumni. I doubt whether, at that time, more was known of the European continental schools than the mere existence of some two or three of repute. All of the medical doctrines, ideas, principles, and practice of this country were derived from the Edinburgh school, or from English writers. Our knowledge of the works, contributions to science, doctrines, theories, and practice of the French, German, and Italian medical schools and profession, with some very limited individual exceptions, does not date beyond twenty-five or thirty years. This circumstance must not be overlooked.

The great accession to medical science derived from those sources, commenced only towards the closing period of Professor Chapman's professional labours. No small portion of the new and important developments, that are

changing all our medical views, occurred a short time previous to, and after his retirement from his professorial chair.

While residing in Edinburgh, Dr. Chapman's admirable social talents brought him into intimate relation with some of the distinguished literati and eminent individuals of that day, a class for which that city has been long famed, and which has won for it the title of the Modern Athens. An interesting anecdote, in this connection, is related in a well-written and graphic biographical sketch of Professor Chapman, in the fifth volume of the *Medical Examiner*, new series.

In 1804, Professor Chapman returned to the United States, and shortly after contracted a very happy matrimonial alliance with one of the most distinguished and influential families of this city and State. At this epoch commenced his professional life, which continued for forty years in an uninterrupted course, brilliant and successful beyond ordinary examples. I do not mean by success what so often it is intended to imply—accumulation of a large fortune. Had that been his object he could easily have accomplished it. It is within the power of any sordid mind. He was “not covetous of gold;” he had a higher estimate of professional services and relations than to regard them as the means of exacting a high percentage of gain. He never made an extortionate charge. He erred in estimating his services at too low a money remuneration. This proceeded from his social and generous nature. He forgot the patient in the friend, warmed by the familiar, animating, and lively converse, which so often made the sick-room the most agreeable place of resort. Often, under these influences, he has refused to make a charge, preferring the gratification of a friendly action, and valuing more the affectionate regards of those he esteemed, than any pecuniary compensation.

“ Thanks, to men
Of noble minds, is honourable meed.”

The success of Dr. Chapman was an exalted reputation and a wide-spread fame; a world of kind and loving friends, whose hearty welcome and joyous expressions testified to the pleasure his presence ever imparted; a deep confidence in his professional skill and knowledge, that brought strangers from a distance to seek his advice and assistance; an unexampled popularity amongst the classes of the school, founded on esteem and reverence for the man, and admiration for his abilities as a teacher and lecturer; and the high appreciation of the Medical Profession throughout our country, for his eminent qualities as a physician.

How few successful capitalists, accumulating millions, can boast of a wealth like this!

True riches do not consist of hoards of money, or acres of broad land. They are the sum of permanent happiness, not of short-lived joys, that man can concentrate in his transient existence. Power and rank can do no more than gratify ambitious hopes, and large rentals flatter acquisitiveness. Can

those gratifications exceed the delights that ever flow from the conscious approbation of the moral sentiments, proceeding from the daily performance of benevolent actions; from a life spent in relieving human suffering, and the honours and praises of loving and grateful hearts, for daily acts of beneficence?

Such were the worldly capital and income of Dr. Chapman. This was his great success in life.

Professor Chapman, soon after his return, opened his career as a teacher with a course on obstetrics. At that time, this department in the University was combined with Anatomy and Surgery, as the subjects of one chair, held then by Dr. Shippen, with Dr. Wistar as an adjunct. The Midwifery course was given by Dr. Shippen, and consisted of some ten or twelve lectures. They comprised the simplest rudiments of the art, interspersed with ludicrous anecdotes and jokes, not always remarkable for refinement or propriety, which kept the class in an almost constant roar of laughter.

The real obstetrical instruction of that period was by private teachers. This department was occupied by two very eminent and popular teachers—Drs. James and Dewees.

The winter succeeding his return, Dr. Chapman commenced a private course on the same branch, which proved successful. In 1808, he became the associate of Dr. James, in conjunction with whom he continued to lecture to considerable classes until 1810.

Dr. James was elected, in June of that year, Professor of Midwifery, which had been separated from Anatomy, and made the subject of a distinct chair, at the request of Dr. Wistar, who had been promoted to the chair of Anatomy and Midwifery, on the death of Dr. Shippen, in 1808.

The course of Dr. Chapman was, however, not interrupted by this circumstance. He continued, from the popularity he had acquired, to command very respectable classes.

The event did not disturb the connection between Drs. James and Chapman. The professorship was merely nominal. So low was Midwifery, at that period, that attendance on the lectures of Obstetrics was not necessary to a degree.

In April, 1813, the death of Dr. Rush produced a revolution in the medical school of the University. He was succeeded by Professor Barton, who was transferred from the chair of *Materia Medica* to that of the Theory and Practice of Medicine, and of Institutes.

In the following month, Dr. Chapman was elected to the vacant chair of *Materia Medica*.

It appeared a bold step in one so young in the profession as Dr. Chapman to follow the veteran teacher, Professor Barton, in the *Materia Medica*, who had distinguished himself in that branch, to which, in connection with Botany, he had particularly applied his attention. It was a matter of surprise to many that Dr. Barton should have relinquished a position which he had filled with

honour and reputation, to accept another for which he was unprepared, and with health too imperfect and vacillating to admit the application and labour it would require.

Dr. Chapman lost nothing by the comparison. He added greatly to his reputation.

It is my impression that the courses delivered by him, during the two years he occupied the chair of *Materia Medica*, were an advance on those of his predecessor.

The work he subsequently published, *The Elements of Therapeutics and Materia Medica*, embraces all the material portion of his lectures. That work, I may safely assert, was the best treatise in the English language on those subjects at the time of its publication, and maintained that rank until within a few years. The rapid advances in all the departments of the medical sciences render any medical work obsolete in a few years, unless, by successive editions, it is kept current with the changes and improvements incessantly taking place, from the great activity of scientific investigation now in progress.

The plan of this work, as of his lectures, was, at the period of publication, entirely novel.

Materia Medica presents itself in two aspects: In the one, it regards, and is in close relation with pharmaceutical chemistry and botany, zoology, and even mineralogy.

In the other, it faces, and is intimately connected with physiology and pathology.

In the first relation, the articles of the *Materia Medica* are treated as subjects of botany and zoology, and in their characters and properties as members of physics and chemistry. Those subjects constitute *Materia Medica*, the matter of investigation and study of the apothecary and the pharmacist.

In the opposite aspect, the articles of the *Materia Medica* are studied in their character of remedial agents. As such, they must be gifted with active qualities, capable of acting on the living structure, of influencing and modifying the vital and functional actions, generally or locally, in a manner to be employed in combating pathological or diseased states of the organism, and remedially in diseases.

This knowledge, the discussions and the determinations of the subjects they involve based on physiology, constitutes *Therapeutics*, "a province of our science," as Dr. Chapman remarks in the preface to his book, "exceedingly interesting, and which has been hitherto strangely neglected." It is in this relation that *Materia Medica* incorporates with the *Practice of Medicine*, and is indispensable to a rational treatment of disease.

Therapeutics in medicine are what strategy is in war. They are the science, the principles that impart effectiveness to skill, and by a knowledge of which, genius can accomplish those combined and calculated operations on which depend success and triumph.

Materia Medica was treated by Professor Chapman in his lectures, and in his *Elements of Therapeutics and Materia Medica*, as merely subsidiary to therapeutics, indispensable to the physician. Since then, the *Materia Medica* of Barbier of Amiens—a very elaborate work, in four volumes—and the later admirable treatise of Trousseau and Pidoux on the same subject, have been published, in which this plan has been adopted. Professor Chapman claims it as original, though I am induced to believe it was pursued in the *Apparatus Medicinæ* of Murray, a work now very rare, and which he had not met with.

The opposite system is that which has generally been followed by the English writers, and has been adopted by the late Dr. Pereira, in his *Materia Medica*, a standard work in that department.

Like most human concerns, each method has its advantages and disadvantages, and one or the other will be preferred, according to the particular views and interests entertained and felt by individuals.

In 1816, by the demise of Professor Barton, the chair of the Theory and Practice of Medicine and Clinical Medicine, in the University of Pennsylvania, again became vacant. Dr. Chapman was translated to it from the chair he occupied. He had not yet reached his fortieth year. He had not been in the practice of medicine over thirteen years, and he had attained to the highest position of honour and trust then known in the medical profession.

In undertaking the duties of this chair, difficulties were to be encountered that do not beset it in ordinary circumstances. His abilities as a teacher, his knowledge and acquirements as a sound and practical physician, were now to be severely tested.

In forming a just estimate of any individual, he must be judged by the lights and standard of the times in which he lived, and what he then accomplished.

To apply the measure of an after period, and one of great progress, would be manifestly unjust.

At this most important juncture of his professional life, it is, then, necessary to bring into view what was the state of medical science, and of medical teaching when he was called on to undertake that duty.

His chair had recently been filled by Dr. Rush, who had acquired the most brilliant medical reputation of this country. Dr. Rush possessed undoubted genius, a fervid imagination, an ardent temperament, and most implicit faith in his opinions. He had imbibed in Edinburgh the doctrine of Brown, then in vogue.

For a century previous, and succeeding to the fall of the Galenical humoralism, numerous medical doctrines had been promulgated, founded on some one class of phenomena existing in the animal economy. Those doctrines were based on chemical, mechanical, physical, spiritual or vital, and nervous considerations and views, according as these phenomena had respectively occu-

pied the attention of their authors. Brown repudiated the whole as worthless. His doctrine was essentially a union of solidism and vitalism.

All living actions, he laid down, proceeded from the solids in consequence of an inherent property he named irritability. This property is called into action by excitants, and the action resulting is life. Hence *life* was said to be a forced state. Health and disease were depending on the normal or abnormal degree of excitation, and proceeded from different states of excitability. From this it follows that all diseases form but two classes, those of debility, or asthenic, and those of excess of excitement, or sthenic diseases. The greater number of diseases, 97 in the 100, are asthenic, or results of debility. Such are the fundamental dogmas of the Brunonian theory.

Prior to 1793, most of the physicians of this city appear to have been Brunonians, and it is most probable that the doctrine of Brown owed its origin and rapid propagation to a typhus constitution existing throughout Europe, and extending to this country. The same condition seems to have prevailed during the last ten or fifteen years.

At the above period, a sudden change must have occurred.

The yellow fever broke out. At first, it was treated as a putrid fever, or asthenic affection, and very unsuccessfully. Dr. Rush was the first to detect the difference, and resorted to depletory treatment by bloodletting and evacuants. All other febrile affections exhibited the same change, and this system of treatment became general. That such change in the character of diseases occurred is evident, as many practitioners, most decidedly opposed to Dr. Rush, were compelled to follow him in his system of treatment.

The doctrine of Brown was here completely at fault. The diseases he had classed as asthenic had assumed a sthenic character.

Dr. Rush did not, as others, abandon wholly the doctrine; he modified it, and adapted it to meet the exigencies of this change, which in reality moulded it into a new doctrine. He retained Brown's principles of solidism and excitability, but erased *debility* from the category of disease. Debility, in Rush's theory, was not disease; it was no more than a predisposing cause of disease.

All diseases were sthenic, or of excitement. The essential nature of disease, "morbid excitement," was made to consist in a convulsive action of the bloodvessels. Thus, there existed but one disease; "disease was a unit." The only difference, however diversified the symptoms, consisted in states of excitability and degrees of excitement. All this apparent theorizing, or reasoning, consists of mere vague phraseology, without a jot of evidence.

The etiology of diseases was made as simple as their pathology. Morbific causes acted only as excitants in an abnormal degree. An enthusiastic disciple, exaggerating the ideas of the master, asserted his belief that the poison of yellow fever might be so diluted that, when bottled and drunk, it would only produce the exhilaration of champagne.

In the doctrine of Brown, the human organism is regarded as a whole; in health and disease, each portion is in a corresponding state. He claims credit

for this view. Dr. Rush adhered to this proposition, which vitiated materially his theory.

The incorrectness of this proposition is well established by observation and experiment, as well as by general anatomy and special pathology and special therapeutics.

The names and classes of diseases, Brown asserted, were useless in practice. The only point to be determined was the degree of excitement.

Dr. Rush held the same opinion. Nosology he discarded wholly. In his lectures, he arraigned, tried, and condemned it to execution by the hangman.

The therapeutics and treatment of disease were as simple as the pathology and etiology. Special names and remedies were to be disregarded; a limited number of medicines, chiefly evacuants, with depletion and revulsions, formed the resources of the practitioner.

The simplicity which this doctrine imparted to medicine made it popular with the students. It required no depth of thought, extended reading, or length of time, to master it. Well might its author boast, like Themison, in Rome, that medicine had been brought down to the capacity of every intellect.

The above sweeping generalities of Dr. Rush, it is obvious, were little adapted to young practitioners entering on practice without experience; yet they accomplished a good purpose. They broke up the adherence to a blind routine of remedies prescribed from names; they taught independence of thinking, and the habit of examining into the condition of patients.

The first duty devolving on Dr. Chapman, on assuming his new chair, was to settle the plan of his course.

A large body of our physicians had been educated in the doctrines of Dr. Rush, and they were popular. The old fabric of methodical medicine had been razed to the ground by the assaults of Brown and Rush; while the views and doctrines they had attempted to establish, Dr. Chapman had been compelled to abandon as unreal, from the result of his own experience and researches.

The nosological systems that had been devised to systematize the facts of medicine were so imperfect he could adopt none of them fully.

Medicine at that time was at a halt. All the facts that could be known by the then available means of research and investigation were exhausted; nothing new could be expected from them, and all the attempts to work them into a persistent theory had proved miserable abortions.

Dr. Chapman had no pretensions to be a reformer; that he could change the character of medicine; or that, by the means at his command, as a practising physician, he could elevate it from its position of a highly cultivated art to a lofty science. At this time, general anatomy was unknown; pathological anatomy had revealed only the grosser alterations of organs; physiology shed no illuminating ray on pathology or practice; pathology was almost entirely conjectural; chemistry was incapable of solving the chemical actions of

living beings, and the attempts made were deceptions; while the microscope had not poured forth its revelations of minute and elementary structure.

What could be done, under these circumstances, but to collect together the most perfect of the fragments of the wreck of the methodical system, which, in reality, were the embodied experience and tested facts of centuries of practical observation, and to rearrange and reconstruct them into systematic order? By this plan he could, in the most effective manner, accomplish the main object of his chair—the teaching of the best practical methods of treating and curing diseases, and of educating for society sound medical practitioners.

Such were the dictates of common sense, of prudence, judgment, and wisdom. Such were the leading characters of Dr. Chapman's mind. Such was the course he adopted; and such the results he accomplished.

In 1818, 1819, 1820, a remarkable revolution suddenly broke out. From 1806–7 the low forms of fever and diseases of the typhous type had prevailed. Yellow fever had disappeared; bilious fevers, and their congeners, were rare; the evacuant and depletory system of treatment had been nearly abandoned. In 1818, a cycle, similar to that prevailing from 1793 to 1806, recommenced. Yellow fever prevailed at Baltimore, in 1819, and some cases, in scattered localities, presented themselves in this city.

In 1820, it reappeared in Philadelphia, and seemed to threaten to become an epidemic, but was checked, apparently, by the promptness of the measures adopted to arrest it. It prevailed in 1821, in New York, as an epidemic.

The treatment extolled by Dr. Rush, and by Dr. James Johnson, bleeding and calomel, and which was relied on generally in the yellow fever of 1793, proved an entire failure. The mortality under that course was over ninety per cent.; and it was abandoned.

The City Hospital, for yellow fever patients, was opened by the Board of Health, and Professor Chapman and Dr. Thomas Hewson were appointed attending physicians.

In this institution, Dr. Chapman employed ol. terebinth., and with more favourable results.

It is to be observed that hospital cases most usually have passed through the first and only paroxysm so characteristic of that special disease, and in the collapse that ensues, excitants will often rally patients in danger merely from prostration. In the first stage of the disease, with high reaction, turpentine was unfavourable in the cases in which I saw it tried.

The same general observation is equally applicable to the bilious and remittent fevers of this cycle, which, neither before nor since, have prevailed to the same extent. They would not bear, as in the former analogous period, the depletory, purging, and calomel treatment.

The diseases of the two cycles were the same, and had the same symptoms, and the same general characters; but they were not identical. The methods of treatment applicable in the one were most disastrous in the other.

This difference I have always attributed to a reigning typhus element, and the intercurrent, in the last cycle, of typhoid fever. The first cases I can recall of that affection were as early as 1818.

In 1822-23, and in subsequent years, the wards of the old Arms Hospital were crowded with cases of that affection.

Dr. Chapman, myself, and others, found the active and perturbing treatment, as emetics, active cathartics, and general depletion, before then adopted in our fevers, unsuccessful. A mild and partially expectant treatment, iced drinks, and diluents and diet, had become the received practice of the establishment, with mercurials, used by some in small doses.

Dr. Lawrence, who fell a victim to the disease in 1822, had also fairly made out all the prominent anatomical lesions, from daily dissections, four or five years before M. Louis's celebrated work on that disease had reached this country. The ulceration of Peyer's glands, the enlarged and often ulcerated state of the solitary glands and congestions of the alimentary mucous membrane, were attributed to inflammation, and were looked on as evidences of that pathological condition.

The general medical theory held by Professor Chapman, as well as the general theory of fever that had been the doctrine of the University for many years, favoured this view, and led to this conclusion. All fevers were regarded as sympathetic disturbances excited by local inflammations. Idiopathic fevers were abolished in medical doctrine.

Professor Chapman, according with nearly the whole medical profession of that period, was a solidist and vitalist. But he recognized the differences in the vital endowments of the tissues and organs, and the diversities of pathological conditions. This was a capital and redeeming proposition in his doctrine, in which he differed from Brown and Rush, in whose theories its rejection was a fatal error. The various organs and systems, he taught, were associated in health, and in disease, acted and reacted on each other. The agent of these associated actions he named Sympathy. Of the nature of this "principle of sympathy," he professed to have no knowledge. He says: "It must be confessed, at present, we have no very distinct intelligence relative to its nature." He employs the term to denote, "like chemical affinity, caloric, and many other such expressions, a principle or power of which we know nothing, except from the experience of its effects, the precise essence or nature being occult, and concealed." He acknowledged that no accurate idea could be formed as to the mode of its action, or to account for the more intimate consent of parts.

In neither case, he says, could the phenomena be referred exclusively to the nerves, as was commonly supposed. But it must be remembered that the discovery of nerve-centres, and their controlling power over sections, organs, and apparatus, of the organism, had not yet appeared above the horizon of medical science, pouring new light into its day-beam, and dissipating the

obscurities that enveloped so many vital phenomena like dense mists, impenetrable to vision.

Though, in this theory, "the Principle of Sympathy is made to pervade the body, every portion of which is susceptible of associative actions, that link the several parts into one whole or unity of system; though it is the general medium for the reception and propagation of impressions, yet all portions are not equally susceptible to the action of remedies, or, perhaps, to the causes of disease. Of the organs that hold this relation, the stomach is possessed of infinitely the quickest sensibility to action, and the most intimate and multiplied relations. No viscus or organ, not even the brain itself, can be compared to it in this respect, or which occupies so important a station in the animal economy." He observes, further: "The stomach is probably the throne of the vital principle from which would seem to emanate an influence that, diffused over the system, preserves the order of the parts, and sustains the vigour, tone, and well-being of the whole animal economy. *Languido ventriculo omnia languent*. Assailed by impressions it cannot resist, this organ, as the centre of association, becomes the seat of the first link in the chain of most diseases, and is always the chief medium of the operation of our remedies in the correction of morbid derangement."

This sketch of the fundamental medical principles and ideas of Dr. Chapman, incorporated in his theory of sympathy, was indispensable in a just estimate of his medical teaching, and the services he rendered to the profession and to medical practice. The term he adopted has led to great misapprehensions and erroneous opinions respecting his principles and doctrines. In its common acceptance, it has a different and more restricted meaning than he employed it to express.

Dr. Chapman's theory of sympathy was founded on great and permanent physiological and pathological truths, that have been recognized in successive ages of our science, though designated under different modes of phraseology. It reproduces, modified and condensed, some of the soundest ideas and most positive facts of the great authorities of past times. Any one, with a moderate familiarity with the history of medical doctrines, will recognize in it the animism of Stahl, the solidism of Baglivi, the irritability of Glisson of Cambridge, as announced in his admirable work, *De Ventriculo et Intestinis* (not the irritability of Haller, who "curtailed it of its fair proportions"). The doctrine of Sympathy has been the fruitful source of all succeeding medical theories; of the vitalism of Barthez, the great luminary of the Montpellier school; the irritability of Brown and Rush; and the physiological doctrine of Broussais. At the same time it is seen to embrace the great physiological truth, first enunciated by Van Helmont in a strange jargon, of the independent vitality of the separate organs, yet united in action and subjected to the supreme power of the stomach or epigastric centre.

This important physiological fact, then first promulgated, was mystified by a personification of the vital properties of the organs, converting them into

dramatis personæ, enacting life, under the name of Archæus. The great Archæus was enthroned in the stomach or epigastric centre, the capitol or centralized seat of vital power; while a subordinate Archæus, subjected to its control, ruled each separate organ.

Change the term "Archæus" into *sympathy*, and this phantasmagorial representation of a fact of wide import, presents the theory of Dr. Chapman, in its simple expressions of the laws of sympathy. Change it into the term *irritability*, and its correlative irritation, and we have all the essential elements of the more presumptuous doctrine of Broussais. In this manner, no little of what is regarded as new is often no more than olden ideas, facts, and opinions, regenerated in a new phraseology.

Sympathy, or consent of parts, is a perennial fact in medicine. It is found in the Hippocratic writings; it is treated of by Galen; it was the basis of the doctrines of Bordeu and Barthez; its facts are found in almost every work of great practical authority. The medical theory of Professor Chapman, of which it is the radical principle, was the soundest and best theory the times admitted. It was the co-ordination of great truths, long recognized in medicine, free of any rash conjectures or hypotheses, and fully answering the object for which it was intended—to guide the practitioner in the treatment of disease. It had no great *éclat*, for it was plain matter-of-fact. It made no pretension beyond the demonstration of observation. There was about it no meteor glare that dazzled, but bewilders. Its light was as of the dawn, not bright, but steady, showing the path to be trod clear and certain, and its direction sure.

If the medical theory of Dr. Chapman be compared to that of Boerhaave, Hoffman, Cullen, Brown, Darwin, or Rush, though it had none of the pretensions or celebrity of those short-lived systems, it possesses from its modest adherence to the available facts of observation and nature, as then established, more of the true character of a theory than can be claimed for any of them. They cannot be regarded, for the most part, as other than brilliant hypotheses, and not in the just acceptance of the term, as theories.

The doctrine of Broussais was an attempt to systematize completely the facts of sympathy, and to adjust the theory of irritability and excitability of Brown and Rush to the facts of general anatomy. Unless this could be accomplished, those doctrines were annihilated. He failed by attempting too much; "his ambition overleaped itself." He professed to have completed medicine. It is not surprising that Dr. Chapman could discover nothing new in its dogmas, except some rash speculations.

The theory of Broussais, in fact, differed from that of Dr. Chapman only in two important points:—

1st. It undertakes to assign inflammatory irritation as the most general pathological lesion. The surgical ideas of inflammation, from the importance they acquired by the publication of John Hunter's classical work on that subject, had become predominant in medical ideas. Dr. Chapman, with wise

reserve, did not attempt to resolve the obscure phenomena comprised in the nature of diseases.

2d. By the absurd exaggeration given to the ascendancy of the stomach in determining the pathological states of the organism—sanguine irritation of the stomach, immediately induced or sympathetically communicated, having been made by Broussais an absolute primary condition for the production and existence not only of fever, but of all diseases, and impressions that were felt or produced action in the economy—a plea may readily be proffered to give countenance to this dogma, but it cannot stand a rigid scrutiny.

It is somewhat curious that the most strenuous opponents, in this country, of the Broussaian views were supporters of the principles of Dr. Rush. Yet, indubitably, Broussais's theory may be looked on as the adaptation of Rush's system to the requirements and exigencies of general anatomy. It was the completion and perfecting of that system, as far as that was possible.

The doctrine of Broussais was, I have no doubt, suggested to him by the pathological lesions he discovered in the autopsies of typhoid fever patients. That disease had reappeared in Europe after an interval of many years, and the prevalence of it gave that doctrine rapid currency. Fever and structural lesions were regarded as absolute evidences of inflammation. According to the knowledge and the ideas of that day, this inference was legitimate.

The same error was committed at our Almshouse Hospital, when the same lesions were observed, and received a similar interpretation.

At that period, the absolute distinctions now known discriminating fevers and inflammations had not been brought to light. The most logical conclusion from the facts, as they then stood, was that fever was always a result of inflammation.

Dr. Chapman was a most uncompromising vitalist and solidist. He would not admit the slightest concurrence of the fluids in the pathological state, or therapeutic operations. In this respect, he was in agreement with nearly the whole profession of the time. Bichat, a great authority, was not a less strenuous opponent of humoralism. He laid it down as a principle, "that every medicine, even a cataplasm, modifies the vital properties, and that we never act but by and through the sensibility of the organs."

On one occasion, Dr. Chapman engaged Drs. Coates, Lawrence, and Harlan to repeat, at his expence, the experiments of Magendie on the absorption of medicines, to test their accuracy. They were confirmed; but the Doctor did not change his opinion. This was a grave error. A scientific fact fairly demonstrated, should never be rejected because it clashes with our beliefs.

Dr. Chapman constantly improved his lectures by additions of a practical character in pathology and therapeutics. Other novelties he looked on coldly and with distrust. So many theories and systems of great promise had proved mere fallacies; so much, at various times, had been vaunted as great advances, yet ended in failures, that he had become incredulous. Besides, therapeutics, or the art of cure, was to him the great end of all medical research. What

did not tend decidedly to this result did not interest him. Hence, he undervalued the physical signs of disease. He saw in them correctly only diagnostic means; they proved the existence of disease and placed its locality. From his practical perspicacity and tact, he had seldom found the rational or constitutional signs to deceive him. Physical explorations did no more than confirm his previous diagnosis. They were not suggestive of the true and most important subjects of medical inquiry; as, for instance, the causes, nature, and remedial treatment of tubercles.

In the same manner, he did not anticipate very important practical results from microscopic or minute anatomy. He often pointed to the example of the material Organic Medicine of the Paris school. It promised to unravel all pathology with the scalpel; it has merely demonstrated pathological anatomy, which it still confounds with disease itself. It has not approached a solution of the great pathological problem, and has led to no therapeutic advancement.

Dr. Chapman disbelieved that a knowledge of minute structure would yield any better fruit. It could do no more than carry our knowledge of altered structure a step further. This criticism is correct; but, without a knowledge of the primary elementary forms of organized structure, the analysis of the organic or nutritive action, in the alteration of which lies the pathological problem of organic changes, could not be effected. Minute anatomy prepares the ground for this important work.

As a lecturer, Dr. Chapman was self-possessed, deliberate, and emphatic. Whenever warmed with his subject, his animation became oratorical. Often, the tedium of dry matter would be enlivened by some stroke of wit, a happy pun, an anecdote, or quotation. He was furnished with stores of facts and cases, drawn from his own large experience and observation, illustrating principles, diseases, or treatment under discussion. His bearing was dignified, manner easy, and gestures graceful. He had a thorough command over the attention of his class, with whom he always possessed an unbounded popularity. His voice had a peculiar intonation, depending on some defect in the conformation of the palate that rendered the articulation of some sounds an effort. The first time he was heard, the ear experienced difficulty in distinguishing his words. This was of short duration; for, once accustomed to the tone, his enunciation was remarkable for its distinctness. Students would often take notes of his lectures nearly verbatim.

Nature had cast him in so plastic a mould that he would have been distinguished in any position of life. From the character of his intellectual faculties, and the tendency of their operations, the natural bias of his mind seemed to fit him for the bar or the legislative hall, rather than the plodding occupation of the schools. He possessed every requisite to form an orator of the highest order, had not the defect mentioned been an insuperable obstacle at the bar with ever-changing juries.

From the solid yet brilliant qualities of his mind, united to great quickness

and natural rhetorical manners, he would have ranked in political life with Clay, Webster, Calhoun, and other distinguished public men of the Republic.

In society, Dr. Chapman was a most agreeable companion. He was an admirable talker. Wit and joke, "for many a joke had he," and repartee, and anecdote happily told, abounded and sparkled in his conversation. His manners were courteous and gentlemanly, uniting dignity, without repelling, to a social familiarity that never invited liberties. He had his faults and his failings, as what mortal has not; but this I can aver, that

"E'en his failings leaned to virtue's side."

It has been my object to claim for our late colleague and friend the justice due to him for the services he rendered to our science, and to his professional merits. They are not sufficiently known, and are far greater than what are attributed to him.

He rescued medicine from the sway of hypothetical systems, and restored it to its legitimate rule of common sense, observation, experiment, and nature. This great change was effected in a manner so unobtrusive and gentle, it scarcely attracted attention; and the benefit thus conferred on science has never been properly appreciated. From that time onwards, the legitimate science of medicine has pursued, without deviation, its proper course and its true direction. It has not, during the last three centuries, been so long undisturbed by the intrusions of speculative systems.

The important mission imposed on him he carried out successfully. The duties he was charged with he conscientiously fulfilled; the part assigned to him in this world was ably performed; and the dark curtain of the last act has fallen.

His name is inscribed on the pages of the medical history of our country, with those of the distinguished and memorable men whose cultivation and labours have advanced and illustrated our science. It is enrolled in the annals of this University, always foremost in promoting medical instruction; that has from its commencement uniformly maintained an honourable fame and a high position; in which he was pre-eminent in reputation in its most flourishing period and its highest renown. It is embalmed, cherished, and revered in the grateful bosoms of the thousands who loved him as a man, valued him as an instructor, and blessed him as a physician.

ART. II.—*On the Medical Topography of Astoria, Oregon Territory, from a Report to the Surgeon General U. S. Army, by I. MOSES, M. D., Assistant Surgeon U. S. Army.*

THE Columbia River is formed by the confluence of its two branches. The northern, or Clark's River, arises in the Rocky Mountains, about latitude 52° north, near some high peaks called Hooker and Brown; the southern, called also Lewis, Saptin, or Snaker River, having its source in the Wind River chain, in latitude 43° , and following an irregular serpentine northwest course, unites its waters with those of the north branch to form the largest river on this Pacific coast, and pour their immense volume into the ocean in latitude $46^{\circ} 30'$. Cape Disappointment, on the north side of the mouth of this river, is a high, narrow, pine-covered promontory, united to the main land by a low neck, over which there is a portage for canoes and small boats to Shoal-water Bay. Within the mouth of the river the shore sweeps round to Chinook Point, forming a deep bay (Baker's), six miles wide. Chinook Point—off which is the chief fishing-ground of the natives of this region—is a bare hill, gradually sloping towards the water, and sending a long sand-spit far into the bay. From this point the banks stretch along with numerous indentations, and are interrupted by several small streams navigable for canoes.

Point Adams, a low sandy spit stretching some two miles out, forms the southern promontory. This is the terminating point of an extensive sand plain, more than twenty miles long and four wide, thrown up by the waves into three distinct parallel ridges, and left by the gradual retrocession of the ocean. The original and true southern promontory is Kilamook Head, thirty miles below the present mouth of the river. The entrances from the sea are two narrow channels, leading the one into Baker's Bay, the other along the southern promontory, between sand-spits and shoals, over which, in stormy weather, the waves foam and boil in so tumultuous a manner as to have rendered the entrance of the Columbia a terror to mariners. Wild and awful too is the picture to the stranger who approaches this notorious bar, for the first time, during the winter season.

Within a comparatively short period, the main current of the river seems to have changed towards the southern shore; and, not only has the channel deepened and become more direct, but the banks have yielded: and, should the change continue, time will still further diminish the risk as the commerce of the country attracts greater numbers of vessels. From Point Adams to Shark Point, the shore makes a deep bend, forming Young's Bay, about four and a half miles wide, into which flow several small streams from the south. From Sharp Point to Tongue Point, a high, narrow peninsula, the shore forms a gentle curve, interrupted by a projecting shelf of small extent, where is the site of the original Astoria, and which is now occupied as a military post. The

place commemorates by its name the originator of one of those bold strokes of genius which great commercial minds often conceive for the extension of trade; and which not only brings pecuniary gains to the individual, but opens new and extended sources of national wealth. What then failed of success through the infidelity of private agents and the breaking out of national hostilities, the unalterable course of destiny has brought about; not, however, until after a rival and foreign company has for years held undisputed sway, and reaped a golden harvest.

Strangers arriving in the country are universally disappointed on seeing Astoria. Their minds, filled with the entertaining narrative of the early settlement of this place, written by Washington Irving, a pleasing picture of an old settlement fills their imagination—and with eager look, as they enter the river, their eyes seek this familiar spot; but their dream is soon dispelled by the view of a line of rough bank, thickly grown with firs almost to the water's edge, and rising in irregular and broken background.

On our arrival at this place, but little of the original establishment could be traced. The knoll upon which the buildings stand contains about twenty acres of cleared land, backed by impenetrable forests of pine. All the structures erected by the first occupants were destroyed by fire some years ago; and the only memorials are the decaying ends of pickets, or heaps of crumbling chimneys. The Hudson's Bay Company erected several buildings, and made improvements as long as the trade with the Indians made its occupancy desirable; but, for several years previous to our arrival, the post had become of little trading value, and the buildings had been allowed to fall into decay.

After the treaty of 1846, when the provisional government of the territory enacted laws for the benefit of settlers, the lands between Shark and Tongue Points were taken up as claims by squatters, who had built themselves small log-houses, and cultivated garden patches. The site occupied and recognized as Astoria, was included in these claims, without respect to its occupancy by the Hudson's Bay Company. On the arrival of the first collector, General John Adair, at the post of Astoria, he located the custom-house and post-office one mile above where he established himself, and purchased the *good-will* of the claim—and this spot is now called Astoria by some, while below is distinguished as Fort George; but this is contested by the rival occupants. Since the establishment of the military at this post, the citizens still occupy it in common with the troops; and no reserves in this territory having been confirmed at Washington, they have continued to erect buildings and make improvements.

Immediately upon our arrival (June 28, 1850), we went into camp, and set to work busily to repair the buildings abandoned by the Company, and erect such others as would afford us convenient shelter for the approaching winter. This was accomplished by the first of November.

The country generally in the vicinity of Astoria is extremely rough and rugged, broken by hills and gulches, covered with a dense forest of hemlock,

spruce, cotton-wood, ash, and willows, with an undergrowth of shrubs, bushes, and vines, which render it impossible for man or animal to penetrate. A few localities are less densely wooded, and entice the settler to pitch his tent and commence the toil of opening a farm. Clatsop Plains, about ten miles long, and four wide, is the only farming district which, as yet, has yielded crops. It is a series of three sand ridges, successively thrown up by the ocean waves, and left by its receding waters. The more recent formation is covered with a thin stratum of decayed vegetable matter, which increases so as to afford a foot or more depth of soil in the older formation. A narrow and shallow rivulet winds through, fringed with cotton-wood and willows. A narrow belt of forest separates these sands from the richer soil. At Astoria, the soil is for the most part a heavy red and black clay, mixed with some gravel, which becomes, during the rainy season, soft and sticky, and, in the summer, dry and fissured. The beach is covered with pebbles, and conglomerate of clay and lime, enclosing shells and petrified marine animals; broken masses of scoriaceous rock lie along the water's edge, and crop out from the banks.

Prominent among the forest trees are those of the pine tribe—spruce, hemlock, and a few cedars attaining incredible height and circumference—cotton-wood, maple, and willows. The oak is not found below Tongue Point; there are innumerable varieties of shrubs and creeping vines. Clearings soon become overgrown with fern—which is difficult to destroy—the wild-rose, gooseberry, a species of raspberry called *salmon-berry*, which is yellow or red, large and coarse—the strawberry, currant, blackberry, cranberry, black and red whortleberry, jal-lal (*Gaultheria shallon*), and service-berry (*Amalencha ovatis*). These are all wild, and inferior in size and flavour to the cultivated. The potato, turnip, cabbage, and beet, are largely cultivated, and attain an enormous size and great perfection; wheat and corn will not ripen except in warm, sheltered spots; oats are well adapted to the soil and climate. The small garden vegetables only succeed with great care, and are apt to be destroyed by the cold nights and frosts, which are often very late. The larger fruits, peaches, apples, pears, &c., do not ripen. The grasses growing on the tide lands are tender, and afford a nutritious food to animals who are able to keep in good condition throughout the year by grazing. Farmers rarely make any winter provision for their stock; the mildness of the winter not requiring them to be housed or fed. Deer, antelope, and black bear, are found immediately in the vicinity; the cougar inhabits the woods with the rabbit and squirrel; beaver and otter are found on the small streams, and along the coast. The waters abound in the various species of wild fowl, swan, sandhill crane, ducks, and snipe, which are brought in plentiful supply by the Indians; there are also pelicans, gulls, eagles, crows, ravens, woodpeckers, swallows, and robins. Flocks of pigeons (*Columba zanaida*) arrive early in the spring, and the trees are often broken by their weight; they are inferior in flavour to the same bird on the other side of the mountains. The grouse and pheasant do not inhabit this locality. There is a remarkable scarcity of singing

birds. Of the reptiles, none of the poisonous variety are found; a green snake, with longitudinal stripes from two to three feet long, the lizard, and snail are seen. There are comparatively few flies; and no mosquitoes, or other annoying insects. The sand-fly is occasionally troublesome towards sunset on warm and still evenings.

The principal food of the natives, and, until within a very few years, of the early settlers in this vicinity, consisted of fish, which, from its abundance, affords plentiful provision throughout the year. Soon after the rainy season, and early in spring, the fishing season commences, and the natives awake from their winter torpor to prepare for the busy time which approaches; canoes are launched and refitted, the nets are mended, and gay trappings put on. This is the season of mirth and plenty. In April, the sturgeon (*Acipenser*) makes its appearance; its flesh is more delicate and palatable than the same fish on the Atlantic coast; it attains a very large size. Towards the end of April, the salmon begin to come in, and increase so as to make the bay merry with their sport; they swim near the surface, and continually leap into the air, often several feet above the surface; they run in vast shoals until the middle of summer. Sunrise and sunset are the most favourable periods for taking them, and the best fishing-grounds are where the sand-spits run far out into the bays. Chinook Point is the favourite resort of the tribes at the mouth of the river. Their nets are long and narrow, and made of grass-twine; the sinkers are of stone, and the floats of wood. An ancient superstition deters them from selling to the whites until the salmon-berries are ripe, and in no case will they part with a salmon until the heart is taken out; upon the strict observance of these points they rely for the plentifulness of the season. To prepare for the winter supply, the salmon is cut into thin slices or strips, and dried on frames hung over slow fires, or along the rafters of the lodges; or, pounded fine, is put up in bundles after being dried. The autumn or "*masache*" salmon are an inferior variety, which runs in September and October, lean, of a pink colour, and having long, sharp teeth. In seasons of scarcity, they are put up for winter use, but generally are not much esteemed. Cod is taken at the entrance of the bay, outside of the bar; of the smaller fish, there are flounders, perch, suckers, and occasionally the "*ulicon*." The last variety, from being very abundant, suddenly disappeared from this locality, and is now only found in the harbours and inlets to the north. It is a delicate fish, resembling, in size and flavour, our *smelt*; and so fat, that, when dried, it is often burned by the natives for the purpose of giving light. The fisheries have been carried on, to a limited extent, by the Hudson's Bay Company as a matter of commerce; and, lately, by the Americans. The salmon is salted and barrelled, and has met a good market in the islands and California. In experienced hands, it would prove a source of great profit. Oysters are found in the bays and inlets north of Cape Disappointment, small in size, but of good flavour; also, the various kinds of soft and hard shell clams, terrapins, and crawfish. The clams are large, of fine flavour, and

superior to the oysters; but these latter are much improved by cultivation, and, contemptible as they would appear to our gourmands on the other coast, are readily sold in San Francisco at the enormous price of ten dollars the bushel. A gentleman from Virginia is now engaged in planting and feeding them, and, from his success thus far, there is every reason to believe they will attain a size and flavour equal to the finest of Maryland and Virginia.

From the above notice of the natural productions, I turn to a brief account of the Indian tribes residing near the mouth of the Columbia. These consist of the few and wretched representatives of three once powerful tribes, the Chinooks, Clatsops, and Cathlamets. The *Chinooks* reside on the north bank of the river, along Baker's Bay, Chinook Point, and Shoal-water Bay, and are about one hundred and fifty in number. They once boasted of two or three hundred warriors, and the famous Concomly as their chief. They are fast disappearing before the white man. Their fishing-grounds are desirable for future value, and even their burial places are eagerly sought after as *good claims*. The *Clatsops* reside, as their name imports, on Clatsop Plains; and this tribe, which, at the time of the settlement of Astoria, numbered one hundred and eighty warriors, is now reduced to about twenty, who can hardly be said to have a local habitation. They wander about, pitching their tents, and lounging about the residences of the whites, awaiting the no distant period of their entire extermination. The *Cathlamets*, who are found on the islands and along the banks of the river, less exposed to the intercourse of the whites, have not disappeared as rapidly; they are scattered, few in number, and will ere long become extinct. Among them resides an old Indian, called *Scamarqua*, who is looked upon as a sort of chief of all these scattered bands, and exerts considerable influence. I have heard him relate, in poetic strain, the past glories of his race, of the martial deeds of his sires, their trophies of victory, and triumphs after return from battle. Yet this bard of military deeds, and last representative of the shadowy glory of his race, does not scruple to get beastly drunk, and has lately sold his granddaughter as a mistress for one hundred and fifty dollars, payable in blankets. All feud has ceased among these various tribes; neither has anything to excite the cupidity of the others, and but few individuals are possessed of any weapons. They rarely quarrel, except when maddened by intoxicating drinks, and then one or two are not unfrequently shot or stabbed. He who possesses a canoe, and a few blankets, is esteemed wealthy; rarely, an individual accumulates about him a large number of articles of finery. A few horses are found among them, but their chief mode of conveyance is by canoe; and it excites one's surprise and admiration to witness the skill with which they manage these beautiful models of naval architecture. They are shaped from cedar trees, and vary greatly in size. Those used by the natives along the coast, and in the neighbourhood of Puget's Sound, are often from forty to sixty feet in length, and twelve to fifteen beam, capable of accommodating sixty persons. They are rapidly propelled through the water by short paddles, skil-

fully handled by both males and females, who kneel on rushes. Their shape is the model, in many points, of our new clipper ships—long, sharp bows, with little curve from gunwale to keel; painted or stained black on the outside, and red inside, ornamented with small white pebbles or shells. A hatchet and knife are the only tools employed in shaping them; and we cannot refrain from admiring the ingenuity of a people who can achieve such perfection of finish with instruments so rude. What might they not accomplish if supplied with and taught the use of the tools employed in our mechanical arts? In rough weather they carry an immense sail, and the canoe is impelled through the waters with the speed of a fish, rising on the top of the highest waves without danger of upsetting. These coast Indians are as much at home on the waters as the Apache on the plains, or the Arab in the desert.

These several tribes speak the same language, follow the same customs, and resemble each other in manners, dress, and person. The majority of both sexes are short, with heavy, stout bodies and limbs; generally from five to five and a half feet in height, with low foreheads, rendered still lower in appearance by the flattening of the head; long, straight, coarse black hair, which is preserved with great care. Greased with fish-oil, they comb, plait, braid, and decorate it with bright ribbons. All traces of beard on the face are carefully eradicated, as well as the hair on the pubes of either sex. They have short, flat noses; eyes dark, and set far apart; mouth wide; teeth small, and often worn to the gums; lips short, thick, and everted. The females especially often become grossly fat. They have short, bowed, fat legs and thighs, large ankles, but small feet. The females have no attraction of face or form; personal beauty is rarely seen. They are filthy in their habits and persons. Where they have been taken as wives by the earlier settlers, they easily learn the customs, and accommodate themselves to cleanly and domestic habits. The half-breeds are more comely, and occasionally handsome; these, as soon as they reach the age of puberty, and sometimes even before, become the wives or mistresses of the whites. These Indians have, generally, assumed our dress, and a few are well and genteelly clothed. Frequently they present a fantastic arrangement of the articles; thus, the shirt is worn outside the pantaloons. Many of the females have learned to make their own dresses. In their primitive condition, their only mantle in summer was a fringe of grass, or fibres of cedar-bark covering the pelvis; in winter, they wrapped themselves in mats or skins of animals, and burying themselves in holes dug in the ground, or small lodges built of mats and branches of cedar, there remained during the rainy season, with a small fire in the centre filling the place with smoke, a small aperture in the roof being the only means for its escape. Within a few years they have erected larger lodges of board and plank, which have been floated off from the saw-mills by the spring freshet, and which they pick up along the river-bank. Since the discovery of gold in California, the Indians have occasionally accumulated quite large

sums; but, as their only idea of the object of money is to satisfy their wants, they rarely hoard it. When the mines of California almost entirely deprived Oregon of its male population, the Indians became useful as labourers; and, when canoe travelling was the only means of transportation, they reaped a rich harvest. Like all savage tribes, they are inconstant, and, having obtained sufficient to satisfy their immediate desires, no inducements will prevail upon them to continue any useful occupation. In morals, they resemble most of the Indian tribes. As soon as a man is able to support a wife, he makes his selection, informs the parents and friends, who, in conclave, determine upon the amount to be paid by the suitor. This being arranged satisfactorily, a day is appointed when a mutual exchange is made; the bride being turned over to the groom, the blankets, horses, canoes, or slaves, are paid to her relations. A dance, a feast, an interchange of presents, and not unfrequently a bloody quarrel, end the ceremony. A chief may possess as many wives as he can support; but, as among less barbarous nations, he has one favourite. Being thus a merchantable article, their women may be procured by any respectable native or white man who can afford the price. Chastity is unknown among the single, and constancy rarely practised among the married; occasional exceptions may be found among those who have white husbands. Their religion, if it may be so called, is the belief in a good and evil spirit; the former dwells above, and is called *Sahale Tihee* (a name also given to any high official personage), and who, when pleased, sends mild winters, abundance of salmon, and fruitful summers; his wrath is displayed by a hard winter, epidemic diseases, or scarcity. There are some curious and interesting myths among them; they have a faint tradition of a deluge, and earthquakes; particular localities are supposed to be the favourite abodes of the good spirit.

The decay of the Indian tribes along the Columbia has been fearfully rapid. A robust and numerous people have disappeared almost as by the wand of a magician. A severely fatal epidemic of measles carried off nearly half the tribes in 1829-30; this was followed by a congestive form of intermittent fever, which has reappeared at various times and created vast havoc; but the scourge of these tribes has been syphilis, and scrofula in its most fatal forms. At the period of Captains Lewis and Clark's visit to the mouth of the Columbia, syphilis was scarcely known; but, as soon as vessels began to enter the river, the disease appeared and rapidly spread. Ignorant of any curative means, vast numbers have died from the primary disease; while, in its secondary and transmitted forms, generations have perished unborn. Glandular and eruptive diseases have carried off the infants, tubercular phthisis blighted their youths, and brought their young men and maidens to premature old age and an early grave. It is remarkable that very few can be found among the men who have not lost one eye by ophthalmia (I presume syphilitic or gonorrhœal). Many are absolutely deformed by the enlargement of the cervical glands, frequently suppurating, discharging, and forming

frightful cicatrices. Harelip and cleft palate are often seen. Abortion is common, and not unfrequently brought about intentionally. Childbirth is a no more easy or less dangerous process than among other females in the same circumstances of life. The older females of a neighbourhood are the midwives, and are quite as good and useful as our more fashionable *monthly nurses*. Where nature is not interfered with, and no malposition or malformation presents, the infant safely enters the world; should any abnormal circumstance arise, the child or mother, or most frequently both, are sacrificed. The attending midwife calls in consultation other *sage-femmes*; these failing to afford relief, the woman is left to die. But, deformity of the pelvis being rare, and the children generally small, accidents rarely occur. The infant, immediately after ablution, is straightened out, tightly swathed, with arms included, and placed on a board, to be submitted to the operation of flattening of the head. This is effected by pads, suitably placed over the frontal bones, inclining from the superciliary ridge to the vertex, counter-pressure being made by a pad under the occipital bone. This pressure is maintained constantly for a year, when the bones having sufficiently hardened to retain the shape, the pads are removed. Infants do not appear to suffer by this pressure, which is kept up night and day; they nurse well, and sleep comfortably. Among certain tribes, side-pads are used, so as to render the head pointed; but this is not followed by the Chinooks. In ordinary cases of sickness, they call in the aid of the "medicine man." This individual is held in high estimation, and demands large fees for his advice and services. These are given at a vast personal risk, and somewhat upon the system, but more just, of their advertising professional brethren in large cities. Upon visiting the patient, and receiving his fees, the doctor goes actively to work to drive out the evil spirit from the suffering body, where it has assumed the form of a wolf, a snake, a beaver, or large stone. The friends having formed a circle, a low and solemn incantation is commenced, accompanied by the regular beating of small sticks of wood, and gradually swelling in tone and rapidity of utterance, until it becomes a howling, yelling, and frightful succession of sounds. The *doctor*, sitting at the bedside, swaying his body to and fro, keeping time to his song of invocation, begins to press and knead the breast and abdomen. As he becomes excited, he jumps up and dances about the lodge with constant and most fatiguing gesticulation of head, arms, legs, and body, until he either becomes frantic by excitement, or falls exhausted. Having, by this time, arrived at a just appreciation of the shape of the disease, he retires from the lodge, and, after a suitable interval, returns. In a most dignified manner he resumes his seat at the bedside, and his song. When, again, a second time the necessary pitch of excitement is attained, he suddenly thrusts his hands beneath the blankets, and, to the surprise and admiration of the assembled friends, jerks out and casts among them a dead wolf, a snake, beaver, or stone; having thus successfully combated the dis-

ease. Should the unhappy victim of Esculapian art fortunately get well, the doctor remains in peaceful enjoyment of his professional gains. Should Father Time, however, have knocked at the door of the lodge during these mockeries, as he invariably does in severe cases, the doctor not only has expended his time and labour for nothing, but now has forfeited his life by failing to restore his patient to health. If he can compromise the matter with the relations and friends of the deceased by paying his value, estimated in horses, blankets, canoes, or slaves, he redeems his own life; but, failing to satisfy the demands of the afflicted, who are usually very exacting, he may expect to be killed before he sees the sun set many times.

The burial-grounds in the neighbourhood, and along the river, attest the numbers of the people who have passed away. They are sacred spots near the banks, and often on naked or isolated rocks. The corpse, if of a chief, or "big man," attired in his best habiliments and gaudiest ornaments, is wrapped in blankets, and placed in his canoe, his fishing-net and spears by his side. Around are hung cooking utensils, such as plates and tin dishes; and thus equipped, he is suspended from the branches of a tree, or raised on a platform, if buried on land, or secured on the top of some island rock, to enjoy in his heaven the fulness of happiness, and to await the solemn day of resurrection, when he shall be prepared to launch his canoe upon the glad-some waters, and the sporting salmon shall yield him abundance.

The most noticeable feature in the climate of Astoria, is its equability; the summers are cool and healthy, the winters stormy, rainy, and disagreeable, but mild. The troops arriving late in June (28th), encamped, and remained in tents until late in November, by which time suitable buildings had been prepared for their accommodation. Observations were taken with the thermometer (no barometer having been supplied) regularly from August 5, 1850, until October 1, 1851, when the post was broken up, and the troops returned to Columbia Barracks, embracing a period of fourteen months. The medical officer of the surveying schooner Ewing, kindly furnished me with the results of his observations for one hundred and one days, commencing April 20, 1850, of which I avail myself:—

Observations during 101 days, commencing April 20, and ending July 31, 1850, on board of U. S. Surveying Schooner Ewing, by Dr. Mitchell, U. S. N.

Thermometer: Maximum, 82°	Barometer: Highest, 30.42 in.
Minimum, 40°	Lowest, 29.80
Mean, 58°	Mean, 30.10
Greatest variation, 30°	

Wind constant from W. S. W. to N. N. W.; light in the morning, and after sunset; commencing to blow stiffly at about 10 A. M. Of these 101 days, 45 were clear, 10 foggy, 16 cloudy, and 30 rainy.

Commencing with my own observations, lat. $46^{\circ} 30'$, long. $123^{\circ} 30'$:—

1850, August. Sunrise. 9 A. M. 3 P. M. 9 P. M. Daily mean.
Thermom.: Monthly mean, 54.07 66.46 70.25 58.09 62.22

Minimum, 51° . Maximum, 79° . Greatest variation, 29° .

Wind from S. W. to N. W.; twelve days were clear, the remainder being cloudy or hazy, with two or three light showers. Early in the day, before the sea-breeze sets in, the sun is hot, and at times oppressive; but, from $9\frac{1}{2}$ to 10 A. M., the wind increases so as to temper the sun's rays. After sunset, the air becomes generally so cold as to make a small fire very comfortable.

September. Sunrise. 9 A. M. 3 P. M. 9 P. M. Daily mean.
Thermom.: Monthly mean, 52.5 60.36 68.80 55.88 60.45

Minimum, 44° . Maximum, 94° . Greatest variation, 33° .

Wind, W. to S. W. Clear days, 10; rainy, 4; cloudy and foggy, 16; 1.8 inch of rain fell. Early part of the month foggy and cloudy; the third being the warmest day of the year.

October. Sunrise. 9 A. M. 3 P. M. 9 P. M. Daily mean.
Thermom.: Monthly mean, 48.48 55.64 66.45 52.22 57

Minimum, 41° . Maximum, 82° . Greatest variation, 30° .

Wind, variable. Westerly winds, especially from S. W., being cold and disagreeable, accompanied by rain. N. E. and E. winds prevailing during clear and mild days. From the 6th to 23d, rainy and foggy, with occasional short intervals of clear, warm weather; the earlier and latter part of the month clear and mild; 6.7 inches of rain fell.

November. Sunrise. 9 A. M. 3 P. M. 9 P. M. Daily mean.
Thermom.: Monthly mean, 41.08 44.3 57.03 43.36 49

Minimum, 33° . Maximum, 78° . Greatest variation, 37.5° .

The month began with cold and frosty nights, and clear days; rain commencing early on the morning of the 6th, continued almost incessantly to the end of the month; prevailing winds from the east; thunder and lightning frequent from the 18th to 23d; 13.65 inches of rain fell.

December. Sunrise. 9 A. M. 3 P. M. 9 P. M. Daily mean.
Thermom.: Monthly mean, 37.16 38.93 47.51 39.04 42.37

Minimum, 22° . Maximum, 59° . Greatest variation, 27° .

Snow fell during the first week, remaining for a short time on the forest trees, but melting on the ground; cloudy weather and light showers until the 22d; ice formed on the 3d; clear weather from 23d to the end of the month; 6.20 inches of rain fell.

1851, January. Sunrise. 9 A. M. 3 P. M. 9 P. M. Daily mean.
Thermom.: Monthly mean, 41.16 42.38 46.74 41.67 43.95

Minimum, 28° . Maximum, 56° . Greatest variation, 16° .

Westerly winds generally prevailing; rain fell day after day incessantly

during the whole month; 27 inches of snow fell on the 15th and 16th, but soon turned to rain.

February.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.
Thermom.: Monthly mean,	40.07	42.64	57.85	39.93	46.14

Minimum, 31°. Maximum, 69°. Greatest variation, 35°.

Wind very variable, E. and W., accompanied by rains which were constant in light showers until the 24th, when the weather became cold and clear; 10.95 inches of rain fell.

March.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.
Thermom.: Monthly mean,	41.54	45.29	52.54	43.54	46.96

Minimum, 32°. Maximum, 69°. Greatest variation, 29°.

Prevailing winds, easterly; constant light showers; mornings frosty from 3d to 9th; 6.1 inches of rain fell.

April.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.
Thermom.: Monthly mean,	48.06	53.36	58.23	51.4	53.15

Minimum, 42°. Maximum, 82°. Greatest variation, 27°.

Westerly winds prevailing; mild showers until the 27th; 4.38 inches of rain fell.

May.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.
Thermom.: Monthly mean,	49.83	57.29	60.74	52.09	55.29

Minimum, 46°. Maximum, 78°. Greatest variation, 28°.

Prevailing winds, S. W. and W.; constant warm showers during the month; 5.95 inches of rain fell.

June.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.
Thermom.: Monthly mean,	54.06	61.01	66.43	56.6	60.25

Minimum, 51°. Maximum, 74°. Greatest variation, 21°.

Westerly winds constantly prevailing; weather, generally, clear and mild; rained four times during the month; 2.55 inches of rain fell.

July.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.
Thermom.: Monthly mean,	56.58	62.61	68.29	58.74	62.43

Minimum, 54°. Maximum, 76°. Greatest variation, 19°.

Westerly winds constantly prevailing; weather clear and bright, except three foggy and misty days; *no rain fell.*

August.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.
Thermom.: Monthly mean,	54.74	67.84	72.16	60.29	65.06

Minimum, 54°. Maximum, 80°. Greatest variation, 22°.

Westerly winds nearly constant; weather clear until 20th, after which light showers until the end of the month; 2.3 inches of rain fell.

September.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.
Thermom.: Monthly mean,	53	59.63	64.87	55.09	59.07

Minimum, 48°. Maximum, 69°. Greatest variation, 18°.

Westerly winds mostly prevailing, though often from east; light showers during the early and latter few days of the month; 1.9 inches of rain fell.

These observations, taken with care, may be considered the average measure of the temperature of seasons, year after year, at this place. An occasional "hard winter" occurs, when snow falls in greater quantity, and remains longer. Thus, during the last winter (1849-50), snow fell frequently to the depth of six or eight inches, and remained for a week or more covering the ground, during a period of six weeks; yet, the thermometer did not range lower. When we consider the high latitude of Astoria, being about the same as Houlton, Maine, and the Sault St. Marie, the uniformity and mildness of the climate seem remarkable, and form the peculiar feature of localities on the Pacific coast. The coldest day was the fifth of December, when the thermometer stood, at the hours of observation, 22° , 24° , 30° , 28° , the mean being 26° . Snow began to fall at 6 P. M., changing to sleet during the night, and the temperature moderating. On no other day during the winter did the mercury remain constantly, and only on four days did it fall below the freezing point. The warmest day was on the third of September, 1850, when the mercury stood at 94° at one o'clock; the temperature, at the hours of observation, was respectively 62° , 77° , 82° , 52° , the mean being 72° . Taking the average of the daily mean, we find the monthly mean to be:—

1850. August	62.22	1851. January	43.95
September	60.45	February	46.14
October	57	March	46.96
November	49	April	53.15
December	42.37	May	55.29
		June	60.25
		July	62.43
		August	65.06
		September	59.07

Hence we find, from August 1, 1850, to July 31, 1851, the mean annual temperature to be 53.267° .

I do not deem it necessary to go into a more detailed analysis of these observations, believing them sufficient for useful application; and I do not have the required data to make a comparative reference to the climate of other localities, so as to draw *isothermal* lines accurately.

From the above report, it will be seen that, during the first month after our arrival, five cases of malarial fever occurred, and two in each of the next two succeeding months. These were relapses caused by exposure and lying on the ground, in men who had suffered from the disease at Vancouver. No other case occurred subsequently among the troops; and the disease, when it attacked either citizens or Indians, could be distinctly and directly traced to a visit up the river. I did not see a single case of any idiopathic fever. Five cases of erysipelas occurred, one in October, two in November, and two in December, 1850, and may be accounted for by the cold, rainy, and foggy weather which prevailed, the men being in camp until the middle of November. The disease was mild, and yielded readily to the external application of a strong solution of nit. argenti, and the administration of full doses of quinia; a plan of treatment which I found highly successful in Toluca, Mexico, where the disease arose from the same exciting causes. Affections of the bowels prevailed to a considerable extent during the summer and autumn of 1850, although the cases among the troops were mild, and of short duration. I met with many severe, and three fatal cases among citizens. The cholera which prevailed in California seemed to extend its influence, in some degree, to this place. One well-marked case occurred about a mile above Astoria. A large proportion of the residents of Pacific City, a small village under Cape Disappointment, were attacked with diarrhoea and dysentery. One fatal case occurred, to which I was called a few hours before death; the patient had frequent and copious dejections of clotted blood, mingled with feces. Several passengers died on their voyage, and others arrived at this post ill with bowel complaints. Of those who were landed and placed under my care, two died with the same but less urgent symptoms, as the fatal case at Pacific City. One family, consisting of father, mother, and three children, were seized within a few hours of each other. Blisters to the abdomen, opium, and mineral astringents, with the most careful diet, were the principal therapeutical means employed.

One-eighth of all the cases reported were catarrhs, mostly of a mild character. In December, 1850, an influenza prevailed, and many complained; but, the symptoms not being sufficiently urgent, the cases are not reported sick. This *influence* was felt in the interior of the territory among the inhabitants generally. Two of the cases reported were attended with active febrile movement, headache, pains in the limbs, anorexia, and painful cough. Antimonials and low diet relieved the symptoms, and convalescence was rapid. Seven cases of catarrh occurred in April, 1851, among a detachment of rifles *en route* for California; they had just left their comfortable quarters at Columbia Barracks, and were exposed in tents at Astoria during wet weather. Occasional cases occurred during other months. More than one-third of all the cases were syphilitic, which, being taken early in hospital, readily yielded to the usual remedies. The frequency of this disease is explained by my previous remarks concerning the Indians. Rheumatism was confined to two or

three old soldiers, who, after any unusual exposure, or before any extraordinary detail, were certain to have an attack. Twenty-six, or one-eighth of all reported, were cases of injuries or wounds; two burns among the cooks, from scalding water; fourteen contusions among artificers and working parties; one fracture of the finger; one luxation of the shoulder-joint; one subluxation of the ankle-joint; and seven cuts inflicted with an axe, in the act of chopping, or in the carpenter's shop by sharp tools.

From the above analysis of the sick report, it will appear that the troops at this post (excepting venereal diseases) enjoyed an unusual degree of good health. Only one man died of syphilitic cachexia, who had been ill a long time.

While the equableness of temperature throughout the year, and the mildness of the climate insure perfect healthfulness, there are few who would select this as an agreeable place of permanent residence. The winters are cheerless, rain falling day after day for five months (from September to March), and a dense, foggy atmosphere, with rarely an interval of clear sunshine. The summers short and dry; atmosphere frequently foggy and smoky; warm mornings, succeeded about 10 A. M. by a stiff sea-breeze, and cool evenings. Late in the spring, a cold, frosty, northwest wind will frequently blow suddenly, accompanied by hail, blighting the young leaves, and killing tender plants which have struggled into a feeble existence. I found the temperature so unpleasantly cool during the evening while in camp, soon after our arrival (though in July), that I invariably had a fire kindled in front of my tent, or passed the time in our mess-tent, where a large stove was put up; and, during the whole period of my residence at Astoria, I can confidently say, there were not ten days that I was without fire in my quarters. During the warmest hours of the warmest days, we could not complain of heat; summer vestments were never needed; if a few hours early in the day seduced one into assuming a lighter material, the cool sea-breeze of the afternoon soon warned him that comfort required woollen garments. I would not be understood, however, that there is *no* pleasant weather; on the contrary, there are days when one may enjoy the brightest rays of the sun, the clearest and deepest of blue skies, the balmiest breathings of the south, and nights when the heavens seem to have unfolded the brightest page of their mystic scroll. The *aurora* is frequent during the spring, and intensely brilliant. Thunder-storms are not frequent, or severe.

Astoria affords a pleasant place of resort from the hot, dusty, and malarial atmosphere of Portland and its vicinity; and, in the future growth of the territory, Clatsop Plains will be, on these western shores, what Newport and Cape May are on the Atlantic. The fashionable belle, paled by her winter's dissipation, and the gouty and over-fed man of business, will flee the city to enjoy a few weeks of pure air, and here renew their vigour in the sportive waves of the Pacific Ocean.

ART. III.—*Extracts from the Records of the Boston Society for Medical Improvement.* By WM. W. MORLAND, M. D., Secretary.

August 14, 1854. *Operations for Removal of Opacities of Cornea.*—Dr. WILLIAMS mentioned the case of a young man, who, while in California, had an inflammation of both eyes, accompanied by indistinctness of vision. After the use of a solution of acetate of lead, as a collyrium, he noticed dense white opacities in the centre of both corneæ. When seen by Dr. W., the left eye presented a dull white opacity in the centre of cornea, apparently a deposit upon its surface, with slight cloudiness of the epithelial layer, extending for some distance around it. From the right cornea most of the incrustated deposit had fallen off, but a ring of epithelial opacity remained, similar to that observed in the left eye.

Ether having been administered, the opaque deposit was removed, and the surrounding cloudy portions of epithelium shaved off, little by little, by means of cataract knives. He was allowed to leave town immediately after the operations, and suffered very slight inconvenience from their effects. Four days after, there was no injection of the eyes; the right cornea was clear, and that of the left eye much improved, though not yet entirely recovered from the loss of its superficial substance removed by the operation. To-day, a fortnight after the operation, his eyes seem perfect. The mass from the left cornea was tested by Dr. B. S. Shaw, and exhibited unequivocal evidence of the presence of lead. The original affection was doubtless catarrhal conjunctivitis (which appears to be of frequent occurrence in California), with ulceration of the cornea; and the case affords another example of the propriety of avoiding the use of collyria containing lead whenever there is any abrasion of the surface of this important structure.

Sinus Perforating the Skull; Discharge of Pus after the use of the Trephine; Recovery.—Dr. S. D. TOWNSEND reported the case. B. N. D., æt. 47, clergyman, entered the Hospital August 4, 1854. For the past three years patient has had a discharge from a sinus an inch from the top of the left ear, during which time he has had some small pieces of bone come away. Latterly he has suffered from headache and loss of memory, with occasional aberration of mind. A probe, introduced into the opening, penetrated an inch and three quarters. The only injury he remembers to have received on the head, was at twelve years of age, when he was struck on the head with an umbrella handle.

12th. Patient being etherized, a crucial incision was made over the sinus, the flaps turned up, and a portion of bone removed with a small trephine, which was followed by a copious discharge of pus. Much relief followed the operation, until the 5th of September, when he complained of severe pain in the head, and of dizziness, accompanied with a profuse discharge from the opening. In a few days this passed off, and he was discharged on the 21st, entirely relieved of all his bad symptoms.

Dr. J. B. S. JACKSON mentioned, in this connection, the case of a boy at the Hospital some years since, in whom, on entrance, only very slight cerebral trouble was manifest; he had, however, a fistulous opening on the back of the head. On the morning after entrance, having been bright as usual the day previous, he suddenly became unconscious, and died in about five minutes.

On *post-mortem* examination, a small opening was discovered through the integuments and bone of the skull, and also a depression into the cerebral substance, containing about three ounces of pus; the membranes were healthy; the walls of the abscess thick.

Epithelial Tumour of the Cornea.—Dr. BETHUNE reported the following cases: J. H., æt. 40, watchman, was first seen July 8, 1851, when he was described as follows: "Health generally good; says that, two years and four months ago, he received a blow from a piece of wood below right eye; three months after, what he calls a little blister appeared at the inner corner, which gradually grew over the sight. Has had slight occasional pain, and the eye waters. On examination, conjunctiva appears thick, with dilated vessels at inner and lower half of right eye. About half the inner cornea is covered with a whitish growth, somewhat vascular. Sight is much affected, and is fast becoming more so, and is affecting the left eye. A vascular pteryx overlaps cornea at inner edge of left eye. At outer edge is seen a slightly thickened growth. Operation on right eye: a large piece of the thickened growth is removed from the inside of right cornea. 12th. Comfortable. 25th. Right eye healing, but upper lid adheres somewhat at outer point of incision. Pteryx removed from left eye. Aug. 2. Discharged."

This case was not again seen till the present time. Says the existing disease commenced two years ago; he has occasional darting pain in left eye. On examination: right eye, no remains of former disease; left eye, conjunctiva and sclerotic somewhat injected; a white and red fungus shoots up from inner angle, covers half of the cornea, to which it seems firmly attached by a neck, and spreads on the surface like a mushroom; sight gone, except perception of light. Health good, except anorexia for two months past.

Aug. 4. Operation: Left eyeball removed; on external examination of globe, the parts adjoining the growth appear normal. On section, the internal structure seems healthy.

Microscopic Examination by Dr. B. S. SHAW.—Small fragments of the tumour were removed from the circumference, and broken to pieces only with considerable difficulty, and after soaking for some time in water. The tumour was found to be composed wholly of *epithelial cells*, principally of the large scaly variety, with comparatively small nuclei. A few young and small epithelial cells and free nuclei were mingled with the others, and the whole arranged concentrically around common centres. From its minute structure the tumour should be called epithelial. No trace of cancer, or other cell, to be found, and no fibres.

Dr. B. had seen a number of cases of this disease; or, at least, of one closely resembling it. The last he reported was in 1849. The microscopic examination rather led to the conclusion that its structure was cancerous. He had heard from the patient within a few months, and there had been no return of the disease. It is quite remarkable that while the more important growth, in the present patient, was entirely removed from the right eye, without any return, that the left eye, from which the pteryx had been removed, should be the seat of this tumour. The apparently innocent character of this growth might lead one to regret that a portion of the eye had not been preserved. It will be observed, however, that the eye was of no use as an organ of vision; that the position of the patient would make the wearing of an artificial eye of less consequence; and, lastly, that the formation of this tumour, after the removal of the pterygium, showed a strong tendency to the renewal of such growths. This would very likely prove a source of irritation sufficient to prevent his earning his living.

Sudden Spontaneous Perforation of Cornea.—John L., æt. 26. First seen, August 1, 1854. This man looks sufficiently healthy, but has had occasional oppression after food, and diarrhœa, since an attack of variola in the spring. No previous affection of eyes till this morning. After rising, while washing his face, felt a sudden pain in left eye. The sight, he thinks, is not affected. On examination: right eye well; left eye, conjunctiva somewhat injected; cornea pierced at lower edge by an ulcer, through which the iris slightly protrudes, leaving the pupil a long, irregular oval. He was ordered a purgative, one leech to the temple, cold applications to the eye, and rest.

3d. Stramonium around orbit, with nitrate of silver to ulcer. 8th. Improved—continue same. 12th. Eye stronger; pupil somewhat more round; omit shade; continue nitrate and stramonium. 19th. Eye still improved in strength, but appears much the same.

This is only the second case which Dr. B. remembers to have seen of this severe lesion of the eye without some previous warning. The other was in a feeble little girl, of nine or ten years old. In that case, the perforation was a very small one, and a general course of tonic treatment, with stimulant local applications, and dilatation of the pupil, was followed by entire recovery. The iris retracted, and the eye resumed its natural appearance, without even irregular pupil, or scar on the cornea.

Effect of Position in the Treatment of certain Gastric and Enteric Affections.

—Dr. COALE remarked, that the late frequency of cholera morbus and other similar affections, had given him an opportunity of testing, to a considerable extent, the efficacy of a certain practice of his, based upon observation made some time since, but which he felt wanted confirmation before suggesting it generally. He is convinced, from actual experiment, that persons affected with irritability of the stomach are much less liable to vomit if they lie on the right side than when they recline in any other position—particularly on the left side. The explanation is evident. While lying on the right side, any contraction of the stomach need not much affect its solid contents; but, when lying on the left side, the contents are in the neighbourhood of the cardiac orifice, and any contraction of the organ will force them more or less through this opening into the œsophagus; thus, the difference between the two cases will be a simple eructation in the first, and vomiting in the second. This, Dr. C. has now tested in very many cases; and by many experiments in some of them, varying the position to the increase or diminution of the nausea and vomiting. It may be urged in objection to the explanation, that a contraction of the stomach that would force the contents through the cardiac orifice, would produce vomiting at any rate. But the difference is this: the same amount of contraction which, when the patient lies on the right side, throws off gas merely, when he is on the other may force a small portion of solid or fluid matter into the œsophagus, when reflex action is at once excited, and the whole stomach stimulated into action.

In treatment of cases of flatulence, and of what is commonly called “cramp colic,” Dr. C. has found reclining on the right side beneficial. It lessens the vomiting—as first said—a frequent attendant in these cases; but, besides this, it gives a more ready escape to gas contained in the transverse colon. For example, suppose the trouble is a spasm, confining gas in the transverse or ascending colon, were the patient on the left side, and a relaxation of the spasm to occur, the gas is still kept behind the affected spot, for the distended intestine is not liable to take upon itself sufficient action to expel it. But, if the patient be on the right side, the gas then ascends and passes on to an

unaffected part of the intestine, by which its escape is facilitated. At any rate, whatever may be the true explanation, Dr. C. is very confident of the correctness of his observations, and of the benefits resulting from this peculiarity of his treatment. He has not found any suggestion of this sort in treatises on the treatment of the diseases mentioned.

Ruptured Uterus.—Dr. PUTNAM reported two cases. The first, a healthy woman, æt. 40, second gestation. Was slightly sick on the 7th of August, but had no positive labour-pains until the afternoon of the 9th, when they became quite strong, and so continued till midnight, when she had one unusually severe, which was followed by a chill, and then ceased altogether. During the succeeding three hours had sharp pain in right side, but no uterine effort. At three o'clock, the female who had been in attendance sent for her physician, who found her cold and nearly pulseless; and, at nine o'clock, when Dr. Putnam was called, she was moribund. The abdomen, just above the level of the umbilicus, on each side, presented two firm, resisting prominences, having a sulcus, three inches wide, between them. The head of the child remained down nearly to the external organs, but was not immovable.

On examination after death, the prominence in the right hypochondrium proved to be the breech of the child, protruding four inches from the rupture; that on the left was the uterus, contracted to a diameter of six inches. The placenta, detached, laid loose in the cavity of the abdomen. Having withdrawn the fœtus from the cavity of the pelvis, the laceration was found to extend from the os uteri, laterally, to within an inch of the fundus. The thickness of the parietes at the lacerated edge was half an inch; at the opposite side it was one inch and a quarter. Both ovaries pale, and dotted with numerous red points. Ten ounces of blood in the cavity of the abdomen.

CASE II.—The mother of several children, after a labour of twelve hours, had sudden excruciating pain, followed by distress at the scrobiculus cordis; there ensued vomiting, with a rapid, feeble pulse. The delivery was effected by the forceps; the placenta was removed without difficulty. After delivery, she was more comfortable, but the abdomen became distended, and she sank within forty hours from the time of the rupture. In this, as in the former case, there was no retrocession of the head, nor external hemorrhage.

Colloid Disease of the Right Ovary.—Dr. PERRY reported the case. The patient was 33 years old, and of a feeble constitution, although she never had any severe illness till the present. She was married when she was twenty years of age; had one child eleven years since; the labour was severe; and, since that time, she has had more or less of uterine trouble—leucorrhœa, menorrhagia, &c. A year ago last June, she had an attack of enteritis, which lasted several weeks, and was followed by ascites. This was cured, as she said, by drastic cathartics. In the following September, she began to have some of the symptoms of pregnancy, such as nausea in the morning, enlarged mammae, and some fulness in the pelvic region. This fulness continued gradually to increase, and, fearing there was something “wrong,” as she expressed herself, she sent for Dr. P. in November. He made an examination, and found the body of the uterus of the natural size, and having no connection, apparently, with the tumour. She was informed of this, but was unwilling to do anything until the full period of gestation had passed, as she fully believed herself to be pregnant. In March last, finding herself uncommonly large, she consented to see a surgeon; and, as distinct fluctuation was felt over the whole abdomen, and as she was very uncomfortable on account of her size,

she was tapped. Only a little gelatinous fluid followed the withdrawal of the trocar; and, from this time till her death, which took place in July, she continued to increase in size, so that, at that time, she was enormously large. She was obliged to remain in a semi-recumbent position, with her feet drawn up, for some weeks before her death.

The *post-mortem* examination was made by Drs. Jackson and Ellis. On dissection, the diseased mass was found to consist, throughout, of a gelatiniform substance; the transparency, the colour, and the consistence being such that many parts of it could not have been distinguished from common calves-foot jelly. To some extent, however, there was a clouded, whitish, opaque appearance, which was found to be owing to the presence of fat. The investing membrane was not defined like that of an ovarian cyst, and was, apparently, thin and loose in structure where it could be traced; no attempt being made to trace it for the most part, as the adhesions were very general. The interior was, to a considerable extent, and perhaps generally, traversed by septa; but these were thin and delicate as compared with those of a common ovarian cyst, and many of them would have been overlooked if the attention had not been particularly directed to them; none of them were too firm to prevent the jelly-like substance being readily dipped out with a cup. On separating the adhesions, a considerable quantity of sero-purulent fluid was found in the peritoneal cavity. The right Fallopian tube was stretched over the tumour, and, being traced to the extent of twelve inches, was then lost. The uterus was somewhat misshapen by being elongated towards the origin of this tube, but was otherwise healthy. The right ovarian ligament was soon lost in the diseased mass. Left ovary and tube healthy. The anterior parietes of the abdomen were spread out thinly over the tumour, and the organs were much displaced; but no other disease was found than what has been described. There was, however, very extensive cadaveric softening of the stomach.

Dr. H. J. Bigelow kindly furnished the following microscopical and chemical examination of the tumour:—

Externally, the membrane investing the mass, and containing the jelly-like material, is of a delicate pearly blue, and semi-translucent, irregularly lobulated by the outline of the numerous cysts into which the mass is subdivided. Upon cutting through this delicate structure, the contained jelly is at once exposed, imprisoned in cysts, which vary in size from a filbert to a hen's-egg; their intermediate septa being sometimes only a transparent pellicle. These septa are highly vascular, and, upon raising them with forceps, the contents of the cavity can be turned out as clean as calves-foot jelly from a tumbler; in fact, the colloid matter bears much resemblance to this material both in colour and density, presenting various shades of amber, occasionally reddish, or streaked with opaque white.

Under the microscope, this opacity is due to the presence of abundant oval, granulated, fatty agglomerations deposited at tolerably regular intervals; but the mass of the colloid is hyaline and structureless. Here and there it exhibits a faint tendency to attenuated fibre, and, from the walls of these cysts, abundant spindle-shaped, and other singularly elongated and tortuous cells can be scraped; the walls themselves being composed of supple and nucleated fibre. A very few cells, irregular in shape, varying in size from a quarter to half that of an epithelial scale, might pass for cancer-cells were there other evidence of cancerous disease; but the jelly is structureless. In this point, it differs materially from the microscopic standard of colloid cancer, which exhibits, generally, an abundant areolar meshwork of a peculiarly fine fibre, im-

prisoning assembled bodies like cells. It is true, that this same jelly-like and structureless material has occurred in the enlarged ovaries where the peritoneum was affected with the usual and areolated colloid, and might, therefore, be supposed to be identical with it; nevertheless, the structureless form occurs, as in the present case, without any other indication whatever of cancer; and it yet remains to be settled how far this "glue-like" matter is indicative of cancerous disease. It certainly is not so when it occurs in the thyroid cysts, and also in some other independent cysts. Neither does chemistry always throw light upon the nature of the varieties of this jelly. In this instance, it has been examined by boiling nitric acid, acetic acid, ferrocyanide of potassium, and other tests. It presents the main characters of the albuminous group of bodies, but it is far from being well characterized albumen.

Uterine Polypus and Ovarian Dropsy.—Dr. SHATTUCK reported the case. E. D., a pale, thin, and rather tall woman, 40 years of age, entered the Massachusetts General Hospital on the 28th of April, 1853. The menses were established at the age of sixteen, had been regular, and she had been always strong and well when she was married, at the age of seventeen. In the course of that year, in the sixth month of pregnancy, she miscarried, and flowed very much. She had insufficient care, was exposed to cold, got up very soon, and worked hard both before and after the miscarriage. She miscarried a second time during the course of the next year; got up very soon, and went to work, but with health and strength much impaired. Catamenia regular, but profuse. She continued to work hard, and came from Ireland to this country thirteen years ago. Between three and four years ago, there was uterine hemorrhage, for six months, at intervals; she was obliged to give up all work, and to keep still. She was under medical care for a year, and was so much better at the end of that time as to be able to go to work again; but, in six months' time, she was obliged to give up again on account of the exhaustion from frequent hemorrhages. She has had much of pain in the back, and hardly a week has passed, during the last year, without hemorrhage. She looks as if she had been a stout, healthy woman, but is pale, weak, anemic; appetite small; bowels costive; pulse small and frequent; sleep scanty and disturbed. A large soft body was felt and seen in the upper part of the vagina by several physicians, but it was a question whether this body was a polypus or an inverted uterus. There was no variation in the tumour on change of posture. There was no sensitiveness, but some flowing followed the examination. She was put upon tonics, chalybeates, laxatives, and as generous a diet as she could take; but she continued to flow at intervals, and the general health was but little improved. She complained at times of abdominal soreness. The abdomen was rather full, but no tumour was detected there on palpation. On the 10th of June, she was thoroughly examined, a uterine sound being used, and a ligature was put round the tumour by Dr. Warren, the patient having been etherized before the examination, and being kept under its influence during the operation. It was followed by a thin, bloody discharge, and soft black clots, to the amount of six or eight ounces.

On the next day, abdomen full, resonant, with some pain; not very tender on pressure; the pulse 108; no flowing. The ligature was tightened for the second time on the 13th. She got up from her bed, and went to the water-closet, in the absence of the nurse, on the 12th, and was not as well afterward. Abdominal pain and tenderness increased; offensive discharge from the vagina; appetite small; able to take but little nourishment. The ligature came away

on the 20th, firmly attached to a consistent tumour, two inches long and four or five lines thick, a half inch or more broad in the broadest part. On the 21st, abdomen full, particularly below umbilicus, where it was flat; resonant superiorly; tenderness on pressure. Fever, abdominal pain and tenderness were noted on the 24th, and she had vomited twice. She was supposed to have peritonitis and effusion. She continued to suffer from pain and tenderness of the abdomen; stomach irritable; breathing short, and at times hurried. She took laxative and diuretic medicines, according to the state of the stomach, opiates to alleviate pain and procure sleep, and such nourishing drinks as were retained. She continued to sink, and died on the 2d of July.

The abdominal cavity only was opened at the autopsy, and a cyst came at once in sight, extending from the pubis to the ensiform cartilage, attached to the left ovary, in common with several small cysts—from the size of a walnut to that of an egg—some of them containing pus. The fluid in the large cyst was turbid, amber-colored, neutral, six quarts in amount; density, 1024; containing much cholesterin, in shining scales, floating on the surface, found, under the microscope, to be groups of thin, transparent, rhomboidal plates. Oil-globules and blood-corpuscles were found, and a large amount of albumen.

Uterus normal; a slight red cicatrix on posterior face.

This was quite an interesting case to those who observed it, and witnessed the patient from day to day. The diagnosis of polypus was made out, and proved to be correct. Had the operation anything to do with this large secretion of albuminous fluid in one cyst and of pus in others? The abdomen was carefully examined on the entrance of the patient into the Hospital. There probably was ovarian disease at that time, but it was not until about the time of the operation that the active morbid secretion was set up under which she sank, and which was mistaken for peritonitis.

Typhoid Fever; remarkable prevalence in one family. Remarks on certain Elements in the Causation and Propagation of Epidemic Diseases.—Dr. CHAS. E. WARE stated that he had recently seen several cases of typhoid fever, occurring in the same family, apparently depending upon some local cause. The house was situated in one of the healthiest parts of the city, and had everything within and about it to render it comfortable and healthful. A young girl of fourteen and one of the servants were taken ill, at about the same time, with the disease. Both died. A young boy was attacked with all the early symptoms, but was removed to the country, and, after a few days' illness, convalesced. Another—a servant, who came into the house during the sickness of the first—was only a few days in the house when she became sick and had typhoid. A young man of sixteen, cousin of the girl first attacked, and who was frequently, and for a long time, in the house visiting his cousin, had the disease. Immediately on the death of the first girl, the family shut up the house, and retired to the sea-side. There was not much typhoid about at the time, and there was none in the neighbourhood.

Dr. Ware said that he had seen two or three instances, in the course of his practice, of typhoid having this local character. In one instance, seven boys, under twenty, the whole of a family except the mother, were sick together with well-marked typhoid. There were no other cases in the immediate neighbourhood. It was an old wooden house which they occupied, but one which was clean and well-aired. In another instance, in a brick block of two houses, standing rather isolated, but near a stable, and not particularly well ventilated, there were eighteen cases of typhoid in the course of about three months, in the autumn. There was no unusual amount of the disease

about at the time. In another instance, he had seen five cases in one family in the course of a season.

Dr. BIGELOW, Sen., remarked that there seemed a probability that the acting cause, in Dr. Ware's cases, was a *local* one; it rarely happens that typhoid fever is taken by contact with persons who are removed to healthy localities. During twelve years at the Massachusetts General Hospital, several of the attendants and nurses were ill with typhoid fever; it would seem as if they took it by contact with the other patients. The degree of contagious influence, said Dr. B., must be very inconsiderable, but is effective when the exposed individuals are, from any cause, peculiarly susceptible; the liability to communication of the disease in this way is not sufficient to deter persons from doing their duty to those ill with the fever, but is enough to lead to the exclusion of unnecessary visitors.

Dr. MINOT asked of Dr. Ware if the condition of the drains were known, in the locality referred to by him?

Dr. WARE said there was nothing apparent to any of the senses, and the occupants of the house were unaware of anything out of order as to drainage.

[The house, as mentioned by Dr. Ware, is in an airy, dry situation; is on one of the hill-streets, the declivity of which, although slight, is doubtless effective in favouring drainage; and there has never been any effluvium perceptible; during the previous summer, a thorough repair of the house was made, particularly of the cellar and lower story; the cellar was enlarged, deepened slightly, cemented in parts, freed from any dampness, and no chance left for imperfection in drains, cesspools, or sinks. No illness of any sort occurred in the houses on each side of the one in which Dr. W.'s patients were; nor, to our knowledge, any cases of typhoid fever in the street, other than those reported.—SECRETARY.]

Dr. J. B. S. JACKSON referred to a number of cases taking place, some years since, in one house, and that an entirely new one. Dr. JAMES JACKSON had charge of them.

Dr. Bigelow, Sen., spoke of the reported influence of filth in the causation and aggravation of epidemic disease; he thinks that it receives the blame thereof, in six cases out of seven, unjustly; it is well known that people who live in airy, healthy situations, yet have the same diseases; *density of population, in low-lying lands*, where occupation is cheap, is undoubtedly often an element in the causation of these affections, and in their increase. It is, added Dr. B., a fact that about as much filth exists in cities in healthy, as in sickly seasons.

Dr. Minot referred to the striking immunity from cholera in well-drained cities; he compared Boston, in this respect, favourably with others known as ill-drained.

Dr. J. O. STONE, of New York, who was present at the meeting, said that, in many of the side-streets of that city, the filth, during this season, had been, and still is, extreme; so much so, that it is impossible to drive close to the side-walks by reason of the accumulation; yet, in those very streets there was not a single case of cholera; filth had not seemed at all productive of the disease, and Dr. S. added that the city inspector thought it had had no effect in increasing the disease; dampness, said Dr. S., has an undoubted and decided influence in the production and continuation of cholera.

[May there not be supposed to be a great difference as to the possible action of accumulations of filth, arising from their *position*? A vast amount thereof might be innocuous, or nearly so, when lying in the *open and comparatively broad* streets; but were the same, or even a far less quantity, disposed in yards,

narrow lanes, and closes; or, what is worse, in cellars, and near to or even in the rooms inhabited, would not there be an effect therefrom, and very palpable, also, upon those exposed? If disease has often, as none can doubt, been *generated* by these means, is it unlikely that existing epidemics are often aggravated by them? It is doubtless unwise to stir up all the hidden and long accumulated filth of a great city upon the first alarm of an approaching, or just appearing epidemic; for, perhaps, the very disturbance of these collections may then only develop disease; "prevention being better than cure," the true policy, and indeed duty, of municipal and sanitary authorities is to *permit no accumulations likely*, even, to aid in producing or aggravating disease.—
SECRETARY.]

Dr. PUTNAM referred to the remarkable exemption of the city of Lyons, France, and which is surrounded by a river, from cholera.

Dr. J. B. S. Jackson spoke of the case with which the city of St. Louis might be drained, from its position, &c.; he also alluded to the severity with which the city of Bangor, Maine, had been visited by cholera. Its largest part is high in situation.

Dr. BETHUNE remarked that there was, however, a large level space in both these latter cities, and if dampness be effective in causing choleraic disease, as it undoubtedly is, the fact of the vicinity of rivers to both cities may be of consequence.

Dr. BLAKE alluded to the very high situation of Quebec, yet cholera has there been very destructive.

Dr. Bigelow, Sen., spoke of the former course of cholera, and also, in reference to Dr. Minot's suggestion as to the influence of efficient drainage, he said that Boston doubtless owed very much of her immunity from cholera to the arid, rocky soil; and drainage was much facilitated by the amount and frequency of declivity within the city.

Dr. PERRY said that, in 1849, there were five fatal cases of cholera on St. James Street, in Roxbury, a high, airy, well-drained and dry situation; in 1832, he saw seven cases of cholera in the rear of Eliot Street, Boston; on examination of the premises, it was ascertained that a vault *was emptying itself into the well* used by the family.

In the cases at Roxbury, Dr. P. thought there was evidence in favour of the theory of communication of the disease by contagion; no imprudence in diet or otherwise was traceable, except in the *first* case, that of a servant, who, from some imprudence, had diarrhoea, which was neglected; the different persons afterwards affected had been in close contact with, or had nursed each other; a child slept with one of the patients; a sister who attended the funeral of one patient died of the disease soon afterward; a woman who came from a distance and washed some clothes in the house, soon sickened and died.

Dr. C. E. Ware referred to the moving of the troops from Avignon to Arles, in France; at the former place there had been cholera, at the latter, none; it broke out, however, immediately on the arrival of the troops; the latter were then removed to Marseilles, and there the cholera raged. Either the disease moved along in the course of the troops, or else they carried it.

Dr. Bigelow spoke of the custom of moving troops from place to place, until they get free from epidemic disease. The plan is successful.

Dr. HOMANS, Sen., remarked that the course of *epidemic fever* had been very striking in the town in which he formerly practised. Typhoid fever prevails. There is a river with a fall of only four feet in six miles, and the adjacent grounds are, at times, overflowed by it. In July, "gastric fever" occurs in the latter part of August, and in the first part of September, typhoid fever

begins to manifest itself. Dr. H. described the course and frequency of the fever during one year. Of eighteen persons in two houses, twelve had gastric affection, with fever; by the last of July, one person in nearly every house in the track of the fever had been affected; the disease followed the course of the river, then branched off around a large pond. In one house, there were seven ill in the time between September 1st and the middle of October; after that time, the disease went into houses situated upon hills. Sixty-eight or sixty-nine persons were attacked, in all. Brookfield, Massachusetts, is the town referred to. In Oakham, which is the adjoining town, a physician, thirty years in practice, *knew of no case of typhoid fever.*

Mumps in a Pregnant Woman. Premature Labour, followed by the appearance of the same Disease in the Infant, twenty-four hours after its birth.—Dr. HOMANS, Sen., reported the case. Mrs. S—, about 25 years of age, of robust constitution and in good health, had reached the end of the eighth month of her first pregnancy. On Sunday afternoon, July 30, she was seized with severe pain in the jaws, attended by stiffness of the neck and slight febrile symptoms. She passed a restless night, and on Monday complained much of pain in her face and neck, which was in some degree constant, with periodical exacerbations. Neck was generally much swollen and tender, especially on right side under the ear; deglutition somewhat difficult and painful; voice hoarse; appetite not much impaired. These symptoms continued increasing in intensity until 10 P. M. of Tuesday, when the pain in neck subsided, and labour pains commenced. Labour went on regularly, and a healthy infant was born at noon on Wednesday. Up to this time, the swelling about patient's neck had steadily increased, attended by tenderness internally and externally, preventing the opening of the mouth widely. From the birth of the child this gradually subsided, and in two weeks from the commencement of the attack had disappeared from the left side, and nearly so from the right. Convalescence progressed uninterruptedly. Translation of the mumps to the mammaræ, in this novel complication, did not occur. On the day succeeding its birth, the infant was observed to cry, as if in pain, while gaping. On examination of its throat, the left parotid gland was found to be swollen; this increased for two days, with the usual phenomena, and then subsided, the right side of the neck not being at all affected.

August 28, 1854. Phlebolites.—Dr. J. B. S. JACKSON showed a number of phlebolites taken from the veins behind the urinary bladder, their usual situation. They were of very large size.

Dr. ABBOT had seen two cases in which phlebolites were found in the veins below the knee.

Dr. Jackson said he had only thrice seen them in other situations than the one he had mentioned; in one of these three instances the splenic veins were the seat; in each instance, the phlebolites were not of a round shape, as were those of the pelvic veins.

Apoplexy of Placenta; Premature Birth of an Enfeebled Child; Convulsions of the Mother.—Dr. COALE reported the following case: Mrs. C. B., æt. 22, of nervo-sanguine temperament, full figure, though small, was delivered of her first child, a large, hearty boy, at the age of twenty and a half years. She took ether during labour, which was prolonged to about twelve hours. Some few hours after delivery, was seized with a violent headache, and at eighteen hours after delivery, with convulsions, of which she had three or four,

rendering her unconscious for twenty-four hours. This account Dr. C. had from the patient, as she was confined at the south, and her husband absent. Last spring she was again pregnant, having in the meantime, and up to Tuesday, August 8, enjoyed excellent health. On the day mentioned, while riding out with her husband and child, an accident happened by which, for the moment, she was very much frightened. From that moment she could not feel any motion of the child in utero, though previously it had been very lively. Dr. C. had frequent opportunities of watching her, and investigating the symptoms; could ascertain no signs of life, though he did not examine very thoroughly with the stethoscope. The breasts became flaccid and small; the abdomen settled down. No effect was perceived on her general health.

Aug. 25. Seventeen days after the fright, she was taken with slight pain and flow at noon; at 8 P. M. active labour; at 8 $\frac{1}{4}$ delivered of a living, emaciated female infant, fourteen inches long, and weighing forty ounces. The after-birth (a portion of which was exhibited) was unnaturally solid, and contained a clot of effused blood, the size of a hazel-nut, and several of the size of a pea, distributed throughout its substance. Mrs. B. flowed very profusely, making her pulse slightly fluttering for a very short time. At 10 P. M. a violent headache commenced, which lessened in an hour or so. The next morning the pulse was 66, soft, perfectly regular; head ached but slightly; left her at 8, having watched her carefully for more than twelve hours. Returned at 6 P. M.; the case being at some distance from the city preventing her being seen sooner. Headache at times through the day, and sometimes very severe; pulse 80, and possibly not quite so soft as in the morning; flows freely, "twice as much as she did at her former labour;" has slept several times during the day; sleep tranquil, and without dreams.

Called at daylight; ascertained that, towards midnight, sleep had become disturbed; great jactitation; at 12 $\frac{1}{2}$ A. M. a convulsion. Dr. Cotting saw her in half an hour, when she soon had another. Bled her to one and a half pints; soon after, while in the act of taking a Dover's powder, had a slight one. The pain of the head had always been referred to the top of the head, and to the back or base. The Dover's powder seemed speedily to tranquillize her, and she remained under the influence of it for six hours; convalescence was speedy and continuous. The infant lived but two days and a half.

Foreign Bodies in the Bladder of a Female.—Dr. H. J. BIGELOW exhibited a number of hair-pins, and of common pins, which he had at different times, during the last few months, extracted from the bladder of a young woman. The patient had been affected with strangury, and had originally endeavoured to relieve herself by dilating the urethra with a hair-pin, which, according to her statement, escaped into the bladder. Her attention once turned in this direction, the subsequent introduction of pins and hair-pins seemed to have become a sort of mania with her. Strangury has been in this case singularly obstinate and persistent. Dr. Bigelow described a very simple manœuvre, by which these foreign bodies were readily extracted, and which was, as far as he knew, undescribed. A pair of common dressing-forceps were lightly introduced, closed, into the bladder. The foreign body being felt, was easily seized and held. By two fingers of the left hand now passed into the vagina, the operator can feel with precision the position of the pin in the forceps, if firmly held, and which, as it probably lies more or less transversely, can be either rotated into the axis of the forceps by relaxing the grasp, or, what is better, urged to one side or the other so as to be held only by one extremity. The forceps being then lightly held and withdrawn, the pin follows. In the

case of a common pin, the point, when held short, is apt to catch in the urethra; so that it is better to pass it back into the bladder, turn it cross-wise, slide it through the forceps so as to be held only by its head, and then extract it.

In this connection, Dr. J. B. S. JACKSON mentioned Dr. L. V. BELL's procedure for extraction of a pin from the male urethra. The pin was in head first; Dr. B. seized the penis, and thrust the pin through it; then turned it, and easily brought it out.

Dr. COALE referred to a similar case, in which the same operation was done, and which was related in an English journal.

Dr. GOULD advised the pushing down of a piece of soft wood, bougie-shaped, upon the pin's point; and so engaging the pin, to withdraw it. Certain remarks were made upon the removal of foreign bodies from various localities.

Dr. BIGELOW, Jr., mentioned having lately extracted a piece of smooth glass from one of the nostrils, by forceps. Dr. B. spoke of the method related, some time since, by Dr. HOMANS, Sen., for dislodging foreign bodies from the nostrils, viz: close the free nostril, press upon the trachea with a finger, to compress the oesophagus, then blow strongly into the mouth of the patient—the foreign body flies out.

Dr. HOMANS said he had often found this method successful in cases in which a bean, or similar body was lodged in the nostril; when fixed in the passage, the full force of the breath being felt by it, generally acts with the effect to drive it out.

Foreign Body retained for two weeks in the Alimentary Canal; Discharge per Anum.—Dr. ABBOT mentioned the discovery of a pin in the alvine discharges of a child, upon whose face the mother had accidentally dropped it, two weeks previously; the pin entered the child's mouth, and passed down, being seen in the pharynx by the mother.

Dr. GRAY referred to an instance observed by him, of the reappearance of a pin *per anum*, four days after it was swallowed.

Dr. PARKMAN had pushed a button backwards from a patient's nostril, into the pharynx, it having been found impossible to seize and extract it by the anterior nasal passage; in thirty-six hours it had passed from the bowels.

Twin-Birth; Double Breech Presentation.—Dr. W. E. TOWNSEND reported attending a case of labour on the night preceding the fourth of July. It was a twin-birth; both the children were boys, the first weighing seven, the second six and a half pounds, and the breech presented in both cases—a foot accompanying the breech of the first child. The placenta was single, and very large, the cords arising from it at some distance apart.

[At the next meeting of the Society, Dr. COALE, referring to Dr. Townsend's case, remarked that the packing of the infants in the uterus, was not, perhaps, so difficult or unusual as might be imagined from the mere fact of both the breeches presenting. In five twin-births, attended in one year by Dr. C., the presentation of each child was the head; yet the heads did not, necessarily, lie in apposition *in utero*—a process of *version* well enough explaining the case; and the like process was doubtless the fact in respect to the double breech-presentation mentioned by Dr. Townsend.]

September 11, 1854. *Disease of the Cerebellum.*—The case was reported by Dr. PERRY.—B. D., æt. 39, single, a German, and fuller by trade, entered

the Massachusetts General Hospital, June 30, 1854. The persons who brought him stated that he seemed occasionally out of his head, and was in the habit of getting up in the night, and disturbing the inmates of the house. Patient had been in this country nine months; said he had always been well till about three months previously; since then, had been subject to a violent headache, mostly in the daytime, extending from the front to the back of the head; when quiet, did not notice it much, but it pained him during exertion, particularly in lifting. His father died of paralysis, æt. 53. Patient himself had no paralysis, but occasional double vision. Had done no work for four weeks. Complained of no other symptoms; stomach and bowels well; slept well; skin of good temperature.

July 1. In bed; makes no complaint, except when he moves—then has dizziness, and some pain in head; appetite good; no intolerance of light; eyes look natural.

2d. Complains of some pain in head; one dejection; vomited this morning, once.

5th. Feels comfortable, except pain in head, which he thinks is increased; pain still in front and back, and not at base.

8th. Although not complaining as much, his memory seems to be affected; walks like a partially intoxicated person.

9th. Yesterday afternoon had a slight convulsion, with apparent loss of consciousness, lasting a very short time; slight diarrhoea; one dejection this morning—involuntary; lies in bed quietly; says he has no relief from leeches; skin natural; pulse 64, soft, distinct; tongue moist; lobes covered with a whitish coat; breath very offensive; answers questions readily; eyes look quite natural; pupils contract readily.

11th. No better; pain and dizziness the same; pain now referred to forehead principally; no nausea; no loss of power over sphincters.

Examination of the Urine, by Dr. BACON.—"Night. Pale; turbid; faintly acid; density, 1.014; a small deposit of foliaceous crystals, and rosettes of phosphate of lime, with epithelium and vibrios. Morning. Pale; turbid; faintly acid; density, 1.016; a small epithelial deposit, with a few rosettes of crystallized phosphate of lime, and vibrios; urea in about the normal proportion; no albumen."

20th. Appearance of patient rather improved; bowels more regular; sleeps less than he did; memory about the same; walking the same; complains still of pain and dizziness in head; no paralysis; pulse 68, feeble.

24th. Patient, during last two days, more restless, wakeful, and irritable; inclined to get out of bed frequently; does not answer questions as readily, and appears to be more confused; vision more impaired; bowels regular.

27th. Passed a more comfortable night; quite drowsy; not easily disturbed; took an enema last night, and had involuntary dejections; appetite not as good for the last few days.

28th. Not as well; extremities inclined to be cold; pulse 72, feeble, regular; loss of appetite; answers questions imperfectly; pupils not much dilated, but do not act readily under influence of light; passes water involuntarily; no dejection.

29th. Three dejections in bed this morning.

31st. Quiet; says he has constant headache, and no sleep; no dejection since 29th; pulse 108, rather weak; pupils largely dilated, but contracting slightly under influence of light; says his sight is not affected.

Aug. 2. Dilatation of pupils continues; says he has less pain; free, involuntary dejections in bed; voids urine involuntarily; passed a restless night; can walk by taking hold of bed.

4*th*. Grows weaker; entire loss of power over sphincters; bowels quite open; more wakeful; complains of no pain, except in head; pulse 84; fec-ble, regular.

8*th*. Growing more insensible; pupils more dilated; no difficulty of deglutition.

11*th*. About the same, growing, perhaps, more insensible every day; no paralysis of limbs; no difficulty of deglutition; answers questions.

16*th*. Lies quite stupid; almost entire loss of consciousness; pulse very feeble; respiration fuller and deeper; no paralysis of limbs; slight cough in swallowing; nurse reports that on passing her hand under his head to lift it, he has an expression, and utters a sound, as if in pain.

20*th*. Quite insensible; breathing more decidedly stertorous; deglutition considerably more difficult; ward-tender reports, that for last three weeks he has had frequent priapisms, and is in the constant habit of self-pollution.

23*d*. Patient continued in an unconscious state till this evening, when breathing became more difficult, and he died quietly about half-past nine.

Autopsy, fourteen hours after death.—*Thoracic viscera*, healthy. Some old, pretty firm, reticulated adhesions at back of upper third of right lung, passing round horizontally so as to make a sort of diaphragm, dividing right thoracic cavity into two portions, the upper containing the upper third of the lung.

Abdominal viscera, on superficial examination, healthy. Liver and spleen rather dark in colour. Kidneys very much engorged, and red, but healthy. Urine in bladder somewhat bloody in appearance.

On opening *cranium*, no unusual congestion of meningeal vessels observed. Cavity of arachnoid unusually dry. Cerebral convolutions perceptibly flattened, and rather paler than usual. A distinct sensation was given to the hand, as of a fluid at some depth beneath the surface. On cutting away the hemispheres, the cerebral substance was found quite firm, the medullary matter showing no red points. On reaching lateral ventricles, they were found largely distended with limpid serum, from four to five ounces escaping. Water of ventricles, pale. In fact, the whole substance of the cerebral hemispheres had a very healthy appearance.

Cerebellum.—The upper third of the right lobe was converted into a soft whitish substance, of the consistence of soft soap, presenting no traces of organization, and without any odour of decomposition. Remaining portion of this lobe, and the whole of the left, presenting no abnormal appearance.

Diabetes Mellitus.—Dr. PERRY reported the case. Mrs. —, aged 52, was seen by Dr. P. in October last, on account of a severe pruritus of the labia, with which she had suffered for more than a year. It was, at the time of Dr. P.'s visit, so severe that she was deprived of rest at night, and was hardly able to perform her duties during the day. She informed Dr. P. that she passed a large quantity of urine, had great thirst, had lost flesh, and recently she had not had much appetite. She had not menstruated for several years. Dr. P. suspected she had diabetes, and requested her to save some of the urine for analysis.

Dr. BACON examined it, and found it contained about seven and one-half per cent. of sugar; specific gravity 1.032. At a subsequent visit, Dr. P. examined her, and found the labia covered with a dry scurf, which, on examination, was found to be crystallized sugar. She passed about four quarts of water in twenty-four hours. She was directed to use a wash of borate of soda, mixed with sweet oil and water, which she was to apply several times a day to the labia, and to take four grains of iodide of potass three times a day, and half a

grain of extract of opium at bedtime. Her diet was to be of meat, milk and bread, with porter at dinner. In a few days after commencing this course, the irritation subsided, and she was able to get her accustomed rest. In four weeks, Dr. Bacon again examined her urine, and found it contained a little more than three per cent. of sugar; specific gravity same as at first examination. She passed less in quantity, and her general health was rapidly improving. In six weeks more, Dr. B. again examined the urine, and was not able to detect any sugar; the patient, at this time, appeared quite well, and had regained her usual flesh and strength; the amount of water passed was no larger than in health. The treatment was continued for several weeks, and from that to the present time she has considered herself well.

Dr. P. said he was induced to try the diet prescribed, in consequence of the great unwillingness of the patients to confine themselves rigidly to animal food. The case is related, not on account of any peculiarity in it, unless it might be the *pruritus* (Dr. P. having never before seen a case where this symptom was present), but in consequence of the rapid and unexpected results which followed the treatment.

Dr. BETHUNE, referring to the fact that Dr. Perry allowed his patient to take *porter*, asked if this beverage does not contain sugar.

Dr. Perry replied that his patient took but a small quantity each day, and in this the amount of sugar must have been very small.

Dr. INCHES said that, while travelling in Europe, he met with a physician who assured him that he had himself been cured of diabetes mellitus by almost living upon *London porter*.

Dr. COALE referred to the great *inequality* in the different brewings of malt liquors; he thought that, when well fermented, there should be no sugar in them.

Dr. ABBOT mentioned the successful treatment of diabetes by yeast given in milk; this has been lately tried at the Massachusetts General Hospital with the result of diminishing the quantity of urine, and, for a time, lessening the quantity of sugar. Disturbance of the bowels being caused by the remedy, iodide of potass. was substituted, and, lately, the latter has been abandoned for creasote.

Dr. PUTNAM stated an instance of recovery from saccharine diabetes, under the use of the vapour-bath.

Fatal Poisoning by Corrosive Sublimate.—Dr. R. M. HODGES communicated the following account of the case and autopsy:—

H. McC., female, æt. 30; brought to the City Cholera Hospital at a quarter before 9 o'clock A. M., Sept. 7; reported poisoned, with what, it was impossible to ascertain. Active emesis was induced by alternate draughts of bicarbonate of soda and water, and diluted acetic acid. I saw her in fifteen minutes after her entrance. She was pulseless, with a cold, damp, sticky and livid surface; eyes sunken, pupils not contracted; no marks about lips or mouth indicated the swallowing of any caustic or irritating substance; bloody mucus issued from nostrils. She was restless, stupid, disposed to sleep; no special odour accompanied the matters vomited. Her stomach appearing to be effectually emptied, I recommended external heat and repeated draughts of warm water; this, as it was rejected, was tinged with blood, and, at the end of half an hour, the fluids vomited were deeply coloured by it. Becoming more conscious, she said that circumstances obliging her to leave a man with whom she had lived illegally for five months, she went to an apothecary's that morning, about half-past 7 o'clock, and purchased, for the alleged purpose of destroying bed-bugs,

something mixed for her in a teacup; that this she carried home and drank. The apothecary states that this mixture was composed of about ʒss of corrosive sublimate and ʒv of undiluted alcohol. When the stomach had been repeatedly evacuated, I ordered a continuation of external heat, oatmeal gruel ad lib., with small doses of brandy until reaction took place. At 11 o'clock, she had a feeble pulse of 58. At 12 M. moderate reaction. Vomiting had continued at intervals, and she had complained of general pain and discomfort. Still stupid and sleepy, lying with mouth partly open and eyes partly closed. Brandy was suspended; gruel, gum water, or white of egg and water, to drink. At 2 P. M. pulse 70, small. At 5 P. M., fluid vomited thick, dark-coloured, composed of mucus and blood in flakes and clots; compared by Dr. TAYLOR, U. S. N., who was present, to that of "black vomit." Purging commenced. Complained much of pain in throat. At 8. P. M. Dr. BUCKINGHAM made this record: "Much restlessness; pulse varies from 18–30 per quarter minute, very feeble; respirations 40; skin cool, clammy; lies with mouth half open, groaning occasionally; bloody fluid runs from nostrils; eyes sunken; abdomen quite soft, with great tenderness at pit of stomach; has passed ʒiij of dark fecal matter with considerable slime, rendered fluid by blood, very offensive in odour; has vomited large quantities of blood, smelling like emesis of cholera. Treatment continued." At 11 P. M. pulse just perceptible at wrist; at carotid, over 100 per minute, feeble. Sept. 8, 9 A. M. Skin cool, livid, damp; some warmth about head; lips very purple; capillary circulation very sluggish; mark left by pressure is obliterated after fifteen seconds; tongue pale, cold, moist, not coated; pulse extinct at wrist; respiration 48, laboured; abdomen soft, painful on pressure everywhere; numerous dejections during night of bloody mucus, without trace of fecal matter; vomited occasionally; now, retches ineffectually every few minutes; very restless; complains of pain in throat, back, and hips; unable to tell whether she has passed urine or not, but it is thought that she has not, since entrance; answers questions willingly, but with difficulty. 12 M. Dead.

Autopsy, twenty hours after death.—Great lividity externally, especially about head and shoulders; considerable muscular rigidity; abdomen full, not distended; muscles fresh, and bright-coloured on section. *Brain.* Marked congestion of external veins; substance firm, natural; no effusion of lymph or serum. *Heart* filled with coagula on both sides; slight thickening of mitral valve; rather flabby, and of large size. *Lungs.* Old adhesions in one or two places; so filled with blood, especially posteriorly, that they collapsed but little on opening thorax; bronchiæ much injected, cherry-coloured at the subdivisions, but becoming deeper-shaded as the larynx was approached, where, on posterior surface, was a patch of lymph, size of a three-cent piece. *Pharynx* livid; upper two inches of œsophagus coated with thin false membrane, easily detached; whole canal uniformly reddened; little if any thickening; no softening or ulceration. *Abdomen.* Patches of a dusky-purple colour at various points of peritoneal surface of intestines; no peritoneal inflammation; stomach, externally, uniformly of this colour, large, and feeling as if half full of a pul-taceous mass; contains ʒij chocolate coloured fluid, consistence of thick cream; mucous and submucous tissues œdematous; in many places, especially near the pylorus, half an inch in thickness, not corrugated, but smooth and velvety; no softening or ulceration. At both cardiac and pyloric extremities were large black patches of ecchymosis, which existed everywhere in smaller points, giving the whole surface a mottled and brownish colour. Quite a line of demarcation in redness and œdema at pylorus, though the former continued into duodenum, where Brunner's glands were much developed. Peyer's patches

remarkably distinct and numerous, commencing high up in the jejunum, and multiplying in size and frequency down to the cæcum, raised, softened, and, in some instances, surrounded by a purple areola. Elsewhere, no softening of the mucous membrane, either in lungs or small intestine. Solitary glands of large intestine enlarged, and near the rectum exist numerous small patches of congestion of a bright-red colour; no œdema after first two inches of duodenum. Mesenteric glands somewhat enlarged; uterus enlarged, but healthy. Other organs natural. Bladder empty.

Remarks.—Convulsions were not noticed, though it would seem, according to the various authors, that they usually occur at the last. Salivation did not occur, the case being too acute. The average duration is one to four days, though one case is recorded (Taylor) where death occurred in half an hour. The dose here was very large, 3–5 grains being a fatal one. The vehicle was remarkable, and in itself would probably have been sufficient to cause death. A case is reported by Taylor, where recovery took place after forty grains had been taken on an empty stomach (as this woman may be presumed to have done), with a certain quantity of whiskey. According to Orfila, it is impossible to say, from any anatomical appearances, that death has taken place from corrosive sublimate. According to Guy, the appearances are inflammatory; but intermediate between “those produced by the corrosive acid poisons, and those caused by the stronger non-corrosive irritants, such as arsenic.” All speak of corrosion and ulceration, which were not present in this case. Orfila says the rectum is usually inflamed, and absence of urine and contracted bladder are said to be always observed. None speak of corrugation and contraction of the stomach, nor was it present here, and no mention is made of œdema or the deposition of false membrane on the mucous surfaces. The appearances due to poison from alcohol are not well determined. Apoplexy, congestion of brain and lungs, are generally the cause of death, and there is usually inflammation of the stomach; but as those who die are generally old drinkers, the appearances found on dissection are not always clearly due to the fatal dose. How much the alcohol had to do with the rapid termination of this case cannot be satisfactorily decided; but as the inflammation of the stomach and intestines had not progressed sufficiently to explain the death, the condition of the brain, lungs, and heart, may, perhaps, entitle us to think that it at least hastened the result.

September 25, 1854. Encysted Tumour of the Eyebrow.—Dr. DURKEE exhibited a tumour sent to Dr. Shaw by Dr. Nathl. Miller, of Providence. It was encysted, of the size of a pigeon’s egg, and situated immediately over and a little under the left eyebrow, at its outer part. Patient’s age 23; duration of tumour unknown. Upon its removal, the sac was supposed to contain a fluid; but after a short time occupied in dressing the wound, it was found quite hard. The walls of the cyst were thin, composed of fine fibres and no epithelial lining membrane was to be found. The contents of the cyst were of an oily nature, and contained in large cells, having, under the microscope, somewhat the appearance of adipose tissue; but the cells were smaller, paler, and the dark contour of the adipose cell was wanting. The fatty matter was found to be margarine, nearly pure; it was fluid at the temperature of the body, and upon boiling it the cells were ruptured, precipitated, and their contents obtained.

Encephaloid Disease around the Knee-Joint.—Specimen shown and case described by Dr. J. H. WRIGHT, of the U. S. Naval Hospital, Chelsea.

The patient was a corporal of marines, æt. 32; born in New York; of a

feeble constitution naturally, but without hereditary scrofulous taint; had recklessly thrown up a good position and enlisted, and may be presumed to have been much preyed upon by depressing emotions.

On the 3d of December, 1853, while serving on board the U. S. frigate *Constitution*, on the coast of Africa, he slipped, and fell upon the deck. The resulting injury was supposed to be a fracture of the external condyle of the right femur, and rupture of the tendon near its insertion into the patella. The limb was extended in a fracture-box, and was treated for eight weeks, when apparently a firm and unusually large callus had formed. From passive exercise he gradually acquired the ability to walk with crutches, without bearing much weight on the limb. After four weeks, the reparation of the joint had not progressed satisfactorily, and a renewal of swelling and pain (the latter, according to the patient's account, inconsiderable), indicating rest, the patient was placed in a cot, and the limb was supported by bandages and pasteboard splints, and was cooled by evaporating lotions. The dressings were examined from time to time without the discovery of any change, the patient being perfectly comfortable, until the lapse of four weeks, when he spoke of uneasiness and unusual heaviness in the joint, and the tumour, to the surprise of surgeon and patient, was discovered of nearly the same dimensions as here presented. A few weeks afterwards, near the last of May, the patient was embarked for the United States, and reached the U. S. Naval Hospital, at Chelsea, on the 26th of June.

Dr. Wright's observation of the case, with few exceptions, continued daily, from July 6 until the limb was amputated, September 23.

The form, dimensions, and general aspect of the enlargement, were described upon a cast taken after the tension had been reduced; and the figure had been somewhat changed by the evacuation of considerable serous fluid.

The tumour was spheroidal, springing from the cartilages of the tibia; measuring nine inches from the patella upwards, having from twenty-two to twenty-five inches of circumference, inclosing most of the joint by a regular curve, but flattened in the popliteal region.

The skin had been discoloured by iodine; but enlarged veins were seen coursing beneath it; and on either side, corresponding to the condyles, there was a dark-red patch of enlarged cutaneous vessels, two and a half inches in diameter, where fluctuation was perceived, and the integuments appeared thin, and in danger of yielding to the pointing of the tumour. This congeries presented some of the external vessels of the size of a crow-quill, was well defined, and quite different from the superficial congestion in the popliteal space and the depending parts. The patella could be traced, thrust forwards and downwards to an angle of 120° with the tibia; it was almost immovable, but yielded, with a slightly elastic spring, when pushed backwards.

As the swelling intruded upwards upon the thigh, it was bounded by two tense, well-defined, fluctuating hemispheroidal lobes, not well exhibited upon the cast, as their fullness has disappeared by the evacuation of the contained fluid, the flow of which was promoted by gentle pressure on these lobes.

Three spots have been indicated where fluctuation was perceptible; but the mass of the swelling was tense and elastic, having an obscure fluctuation or movement, which some manipulators compared to calves-foot jelly.

There was at all times considerable œdema; but it sometimes diminished, under cooling treatment, so as to leave the skin relaxed and capable of being wrinkled, inspiring fallacious hopes of a diminution of disease.

There was considerable heat; but there was no pain. The patient said he had never felt pain, except when he received the injury; every disagreeable

sensation was expressed by one term, "heaviness," as, for instance, under prolonged manipulation or firm pressure.

The patient's appetite was good; he generally enjoyed sound and refreshing sleep, without hypnotics. He had no hectic; his spirits were buoyant as could be desired; and all the functions were well performed, with the exception of occasional constipation, and an accidental suppression of urine, followed by irritation of the neck of the bladder, which required for some days the use of the catheter. This was attributed to prolonged irrigation of the knee with cold water, and was readily relieved.

The diagnosis of the case was complicated with an alleged extensive injury, the reparation of which had been obscure, and the possible results of that injury embarrassed any judgments formed from an examination of the swelling.

Familiarity with the case excluded the hypothesis of aneurism, entertained, perhaps, at first glance.

The aspect of the tumour suggested encephaloid disease; so did the obscure fluctuation, the general elasticity and the immobility of the patella; but, on the other hand, the entire freedom from acute pain and from hectic could hardly be reconciled with the rapid development of an extensive malignant disease; and may not the patella have been so fixed, in the reparation of a previous injury, as to be thus restrained in its motions, though immersed in fluid? And, again, the superior lobes and the vascular cluster presented a positive fluctuation, only to be referred to a fluid.

The absence of pain and of hectic precluded the notion that the fluid was purulent.

Was it serous secretion occupying the capsule and bursæ and sheaths of the tendons, imperceptibly supervening upon a fractured bone and ruptured tendon, obscured by an abnormal condition of reparation?

The most careful examination could detect no bony formation, except at one point on the outside, where the pressure of the thumb was arrested, just below the skin, by a protrusion, supposed to belong to the extensive callus reported to have existed eight weeks after the fracture.

It seemed to be the wisest conclusion to confess ignorance of the true condition of the joint, and, while the health of the patient permitted, to pursue an expectant treatment, and hope for the best.

This was substantially the view of the case when committed to the charge of Dr. Wright, in consequence of the sickness and absence from the Hospital of Dr. Gilchrist, the senior medical officer.

It was treated by wetted cloths, cooling mixtures, and by irrigation. The tumour measured a little less in circumference, and the patient was very hopeful.

August 5. The skin broke within the vascular spot on the inside, and, after some serous oozing, healed up.

10th. Dr. Gilchrist punctured twice with a bistoury, near the same spot, where the skin was thinnest and fluid most apparent; the first, entering a superficial vein, was not pursued; the second penetrated about an inch, and discharged two and a half ounces of discoloured serum; it oozed for some days, and closed spontaneously.

A month elapsed, when Dr. Wright observed progressive emaciation of the patient, increasing irritability, and the tumour encroaching on the thigh upwards, and on the ham downwards; the leg and foot were also swollen with oedema. The requirements of the case called for more decided proceedings.

On the 17th of September, Dr. W., after consultation with several naval surgeons (Johnson, Harlan, Martin), punctured on the outside with a small trocar,

with explorative intent, to the depth of two and a half inches, without pain, and without any immediate flow of fluid. The probe discovered the denuded bone through the canula. A similar puncture on the inside procured three or more ounces of unwholesome serum, of urinous odour, when the canula was choked, and, after it was withdrawn, both openings discharged freely by continuous oozing, by which the tension was greatly lessened, and the rotundity of the upper lobes disappeared; but the mass of the swelling was unchanged, and it was a necessary conclusion that it was encephaloid, inclosing a cavity, or possessing cavities, filled with fluid. Two days afterwards, Dr. Townsend, Sen., of Boston, examined the case, and pronounced it to be fungoid disease, and advised amputation. The patient consented that the operation should be done after four days, on the 23d September.

In the meantime, the outer opening closed, but did not heal; the inner one discharged profusely fetid serum mixed with brain-like matter. On the evening of the 21st, the skin opened spontaneously by ulceration on the outside, three-fourths of an inch from the closed puncture, and poured out copiously fluid and brain-like matters, discoloured with blood, and mixed with shreds of disorganized tissues. Thirty-six hours after this occurrence, amputation was performed by Dr. Wright, agreeably to the patient's appointment, who resisted urgent advice to anticipate it, for the tumour was rapidly degenerating, with corresponding exhaustion of strength.

The patient had great dread of the operation, and always manifested singular abhorrence of an instrument. He was placed under the full anæsthetic influence of sulphuric ether.

Death followed the operation, after two hours, without any reaction from the first shock.

Dr. Wright exhibited the specimen.

The integuments, all the muscles, and the nerve, were readily turned off from the face of the tumour, exhibiting some fibrous bands with protruding lobes of medullary matter, and in some places adhering to the expanded muscular and tendinous fibres. The popliteal artery and vein were inclosed in the projecting posterior hemispheres of encephaloid matter. The medullary mass, with walls of varying thickness, in some places three and a half inches, was found to inclose an irregular cavity, filled with fluid, corresponding to the capsule, which approached the surface under the anterior superior lobes, and in the direction of the perforation before spoken of. Protruding into this cavity was the denuded shaft of the femur, tilted to the outside, and forming the bony projection supposed to be a portion of callus or condyle. There was no trace of condyles, or of reparative bony formation; neither fracture of the patella nor rupture of the tendon was proved.

It is possible, said Dr. Wright, that the disease began before the injury was received; the fragile bone would be easily broken by a slip or strain, and the swelling joint, the distortion, and the displacement of the patella by a growing tumour, may have produced results imputed to rupture and fracture.

The insidious character of the disease was proved by the fact that it had invaded the os calcis without having excited any complaint or suspicion.

A sac, with a smooth lining membrane, was also pointed out, burrowing under the gastrocnemii, as a probable aneurismal dilatation; but its connection with the artery was not satisfactorily exhibited. There was no general necroscopic examination.

Powerful Constitutional Effects from the Application of Atropine to the Eyes.—Dr. BETHUNE mentioned the production of dizziness, and of a sensa-

tion in the mouth and fauces, as if belladonna had been taken in large doses, and these symptoms followed the application of one or two drops of a solution of atropine (grs. iiss to ʒi) to the eye of a stout, healthy young woman.

Wen lying deep.—Dr. H. J. BIGELOW exhibited a wen, or encysted tumour of the atheromatous variety, of the size of a large horsechestnut, removed from between the bellies of the digastric muscles, under the floor of the mouth, occurring in a boy of nine years of age, a patient of Dr Storer. The tumour was congenital, and had of late grown rapidly. Dr. Bigelow remarked that this was the first wen he had met with, lying beneath the deep fascia, which was purely atheromatous, consisting of epithelial cells floating in a watery fluid, and destitute of sebaceous or oily secretion, and, of course, of hair. In the deeper and congenital variety of these tumours, as, for example, those beneath the orbit, the secretion is generally meliceric, sebaceous, or oily, as well as epithelial, and with a few hairs; while, in those occurring in the superficial cellular tissue, the inclosed material is usually a watery, pultaceous, epithelial excretion.

Double Cephalæmatoma.—Reported by Dr. HOMANS, Sen. The subject of this affection was an infant, born on June 2, 1854, after a short and comparatively easy labour of only an hour's duration. Its weight, at birth, was a little over eleven pounds, and it was vigorous and healthy. Presentation was of the head, which was not compressed so as to produce swelling of the scalp; but, on the contrary, was remarkably well shaped and natural in appearance. On the third day after birth, two tumours were discovered on its head, one on each parietal bone; that on the right side being the larger, measuring five inches by three and a half, while the dimensions of the one over left parietal bone were four inches by two and a half. They were soft and fluctuating, save at the base, surrounding which, and forming the limit of each tumour, was a firm ring, resembling the edge of an abscess. On the 22d day there was a decided diminution in the size of these swellings; and, at the end of seven weeks, they had entirely disappeared. The treatment consisted merely of lotions of cold water, and gentle friction.

Dr. H. added, that double cephalæmatoma is quite rare. SCANZONI, in his *Geburtshefte*, quotes from Dr. BOHM, some statistics relating to the frequency of the occurrence of this affection in the hospital at Prague, which are here given, as perhaps they may not have been seen by many of the members of this Society. In 21,045 infants, cephalæmatoma was met with 100 times in 96 individuals—about 1 in 219. It was 96 times in the parietal bones, twice on the occiput, once on the frontal, and once on the temporal bone. In 4 infants there were 2 tumours, and in 92, only 1. From which it appears, that double cephalæmatoma occurred only once in 5,261 births. In the four cases in which there were two tumours, one was on each of the parietal bones; the present case, therefore, corresponds with the general rule.

Dr. BIGELOW, Jr., saw a case of double cephalæmatoma, for the absorption and disappearance of which three months were required. In another instance, in the country, Dr. B. advised that the tumour be left to nature; this, however, was not done; the attending physician punctured it; in a fortnight it healed, and had become flattened entirely. This procedure and result are, of course, not the rule; there may be, as is well known, a diseased state of the bone, in which case very ill results might follow a like operation.

Dr. J. B. S. JACKSON had examined, *post-mortem*, a child who died while having a cephalæmatomatous tumour. There was disease upon the *inside* of

the occipital bone, with ossification of the perieranium; no thinning of the bone, and no disease of it *externally*. Dr. J. added, that although puncturing is not the rule of treatment, nor is generally considered advisable, or even safe, yet he knew of its having been frequently done by a physician of this city, who had had quite a number of cases. The results were, he believes, uniformly good.

Poisoning by Strong Tincture of Aconite.—Dr. PUTNAM reported a case in which most remarkable success followed the internal administration of iodine, as recommended in a recent French journal. Dr. REYNOLDS saw the patient about an hour after she had, by mistake, swallowed an ounce of strong tincture of aconite. Immediate vomiting ensued, and she was still vomiting; there was severe cramp; coldness of the whole surface, and especially of the extremities.

A solution was made of eight grains of hydriodate of potass and six grains of iodine in one quart of water. Of this a wineglassful was given, and the patient soon fell asleep. In half an hour she awoke in renewed distress; the same quantity of the solution was repeated; she again fell asleep, and, in the course of two hours, all formidable symptoms had disappeared.

Dr. Putnam remarked that, in the case of poisoning by aconite which he reported some months since, great benefit was derived from the administration of laudanum, after emesis induced by ipecac.

Unnatural Presentations of the Fœtus.—Dr. STORER remarked, that in July, 1853, he reported the case of a woman who had, in seven labours, three arm presentations, two presentations of the *feet*, and two of the head. He had lately again attended her with her eighth child, with another arm presentation; the fourth in eight pregnancies.

Dr. BOWDITCH presented a communication from Dr. WASHINGTON L. ATLEE, of Philadelphia. It was sent in consequence of remarks made by Dr. B., at a previous meeting of the Society, relative to *injections of iodine into the thorax*. It consisted chiefly of the details of a case of large pleuritic effusion, where paracentesis was performed, and subsequently iodine injections were made with great and very manifest benefit.

A gentleman had had severe acute pleuritic symptoms, while under the care of another physician. Dr. Atlee was called, December 1, 1850, a week after the first attack. Patient had been freely bled, leeches, and blistered. Cathartics had been but slightly used. By advice of Dr. A. these were directed more actively, and two grains of superacetate of lead were given every two hours. Under this treatment all the rational symptoms subsided, and Dr. A. withdrew from the consultation. On the 14th of December he was again summoned, owing to the greater illness of the patient. He learned that two days previously there had been a relapse of the chief symptoms; the pulse had become small, and 128 per minute; the dyspnoea was excessive. The patient could lie only on his left side. There was complete flatness, on percussion, throughout the left side of the thorax; that side was distended; no respiratory murmur was heard; the impulse and sound of the heart were to the right of the sternum, two inches below the right nipple.

Ordered blister to cover the left half of the chest, and to be dressed with mercurial ointment. R.—Digitalis fol., hyd. chlorid mit., scill., āā gr. i, to be taken every two hours.

15th. Pulse 132; respiration still short and frequent, although somewhat relieved by blister; oppression still great.

16th. No better. Paracentesis was performed with a trocar between seventh and eighth ribs, and midway between the spine and the sternum. A strong gush of pus followed. It continued flowing per saltum with the respiratory movements until eight pints were discharged. The air tended to enter with every inspiration; this was partially guarded against by means of the finger as a valve. The trocar was left in the wound, and plugged with sponge. Dr. A. continues as follows: "6 P. M.—The pulse had not changed in frequency, but had improved in volume. Respiration still frequent and short. The canula, which had been passed only half its length, had, during our absence, been pressed into the rim, and every respiration gave to it distinct motion. The sponge was removed, but there appeared to have been no additional collection of matter, as it did not escape more readily than at the time of plugging the tube. We drew off, however, another pint, making altogether nine pints. The canula was now removed, and a wet compress was placed over the orifice, and secured by a bandage. The heart could be recognized beating nearer the sternum, and the patient could turn on his back with much more ease.

17th, 9 A. M. Patient had a very comfortable night, sleeping more than at any time during his illness. He felt much relieved; his respirations were much deeper and less frequent; pulse 116, and more full; and there was no febrile excitement. The heart was under the sternum, to the left of which could be detected tubular breathing, and also some resonance on percussion. 6 P. M. The patient could lie on either side and on his back with equal ease. His pulse was 108, and respiration much improved, and, to use his own expression, he "felt like getting well."

Jan. 1, 1851. From the last to the present date, the patient has been pretty comfortable. The pulse, however, has not been below 100. The wound in the thorax closed after withdrawing the canula, and consequently there had been no discharge after the evening of the 16th December.

2d. On visiting the patient this morning, we found that the cicatrix had ulcerated through, and that a large discharge of matter had deluged the bed in the night. The place was now kept open, and the cavity allowed to empty itself daily, which it did to the amount of nearly a pint every twenty-four hours. This severe drain, in spite of the most generous and nutritious diet, the free use of quinia, and other tonics, soon began to make serious inroads upon the vital powers, and threatened to destroy our patient by its exhausting influence. He became extremely emaciated and anemic, his pulse feeble and intermittent, and death, from asthenia or inanition, seemed to be rapidly approaching. It was very clear that unless the discharge could be arrested, the patient must die. It was in this exigency that I proposed the injection of iodine.

Feb. 2. Half a drachm of the *liquor iodini compositus* [U. S. D.] was mixed with one ounce of lukewarm water, and injected into the cavity of the pleura. It remained in the cavity, and produced not the least distress or inconvenience.

3d. On removing the bandage and compress, the quantity of matter was found to have diminished at least one-half. One drachm of the solution, mixed with one ounce of water, was now injected. On the 4th, 3ij to 3j; on the 5th and 6th, 3ij to 3ss; on the 7th, 3ij to 3ij. Afterwards, the pure liq. iod. comp. alone was injected. At no time did the patient complain of anything more than a pleasant warmth resulting from the presence of the

iodine; and from the very first use of it there was a rapid diminution of the formation of matter, and a total cessation of it by the end of February. No precautions were used to prevent the admission of air; it flowed freely out and in with the respiratory movements, and seemed to do no harm.

Under this treatment the patient rapidly recovered his flesh, and strength, and health; his pulse came down to 84; his respiration became natural; and, in a few weeks more, he was able to attend to his business as usual.

Aug. 28, 1854. I have just seen Mr. M——; he has enjoyed the most perfect health ever since. I examined his chest, and find that the left side is shrunken. The respiratory murmur, however, can be heard in the left lung, and the impulse of the heart can be perceived in its natural location. A tape measure, encircling his body just below the nipples, meets at thirty-one and a half inches, seventeen of which belong to the right side, and fourteen and a half to the left. As an evidence of his good health, I may state that he has been subjected to an examination for an insurance on his life, and that no objection was raised.

In connection with the very interesting report by Dr. Atlee, Dr. BOWDITCH briefly stated the following case: A young married woman, who had been twice pregnant and safely delivered, and who had been usually in good health, was attacked with acute pleurisy three months since. The disease went on very rapidly, with very severe pains in the left side, and gradually increasing dyspnoea, until, after a fortnight's treatment by antiphlogistics, Dr. B. was summoned in consultation. She was found sitting up in bed, gasping, and had been unable to lie down for several nights; her pulse was very indistinct; her face was haggard, and bathed with heavy drops of sweat. In fact, she was fast approaching dissolution. There were the usual signs of the left side of the chest being filled with fluid, viz: dulness throughout, dislocation of the heart to the right of the sternum, respiration absent in the left, puerile in the right lung.

As all remedies had been used without avail, paracentesis was decided upon. The exploring trocar and suction apparatus were used, and about 3viij of thick pus were removed. Much relief followed; but, in six days, owing to increase of cough and return of dyspnoea, the operation was again performed, and a little more than a pint was removed. Dr. B. forbore to continue the details. In general, it might be stated that Dr. B. had, during the three months, punctured seven times; and, after every operation, there had been a steady improvement. Though still under treatment, all the rational symptoms have disappeared. Patient now goes about the house, and performs, in some measure, her household duties. Her appetite is sharp, digestion excellent; both of which, before the first operation, were wholly disordered. The sleep is good, except when the cough is severe. This is always almost wholly relieved by an operation. Gradually it increases as the fluid reaccumulates, and, of late, it has been the chief cause for the puncture. The intervals between the operations have gradually augmented, and, of late, have been more than two weeks. The amount of fluid has been usually one quart, but, at the last operation, it was about a pint only, and much thicker than ever before. The heart is nearer its normal position; the pulse much better. The respiratory murmur is heard with considerable distinctness in the upper third of the back, and down along the vertebræ still lower. There is some sound in the upper half of the chest, front and back. In other words, with each operation the cavity containing the pus is slowly diminishing. Meanwhile, pregnancy is proceeding healthily, and the foetus seems of lusty form, the motions being very vigorous.

In all the operations but two, the small trocar was used. On these two occasions an attempt was made, by using a larger trocar, and leaving it in the opening, to produce a permanent fistula. Dr. B. believed that, by this daily discharge, a better opportunity would be afforded for relief to the cough, and, if necessary, injections of iodine could be used. Both times, however, the operation was unsuccessful; the canula was displaced, and no fistula was obtained. The usual relief to the symptoms was, however, obtained; but there was much more suffering from the local irritation of the large, than the small instrument.

Dr. Bowditch remarked, that he had operated with the small trocar seventy-seven times during the past three years; in no one instance had he seen any evil effect. In all cases, where fluid was obtained, the puncture had afforded relief; and, in some, it had been the principal or sole cause of cure.

Fracture of Os Hyoides, produced by a Fall; Suffocation from Effusion around the Glottis; Tracheotomy; Resuscitation by Artificial Respiration.—Dr. WILLIAMS related several interesting circumstances connected with a case of which he had received an account from Dr. A. F. SAWYER, of San Francisco. The patient fell a distance of about fifty feet, and, among other severe injuries, sustained double compound and comminuted fracture of the jaw, and fracture of the os hyoides. On the fourth day Dr. S. was hastily summoned, and found the patient suffocating from closure of the glottis. He performed tracheotomy under circumstances of unusual difficulty, making his incisions without assistance, through tissues which were swollen and turgid from infiltration of air and serum, and, by means of artificial respiration, restored the patient to life. Three weeks after the accident the patient had secondary hemorrhage from the jaw, by which he was rendered nearly pulseless. This was arrested by ligature of the right facial artery. The canula was kept in the trachea for ten days, at the end of which time the patient could breathe freely through the natural passages.

October 9, 1854. Symptoms as if of Poisoning, after handling Cotton Bales.—Dr. COALE was consulted by a stout, healthy teamster, for a papular eruption, which came on immediately after handling a load of cotton bales. It affected the arms, the face, and the legs as high as the knees; the surface of these parts being almost entirely covered with the eruption, which was accompanied by a most intolerable itching and stinging. The disease seemed to yield to the following prescription: R.—Spts. mindereri $\overline{3}$ i; vin. semin. colchici $\overline{3}$ ss. A teaspoonful every four hours. There was but one other engaged in the labour, and he was reported to be laid up with some sickness he caught from the cotton; but the particulars could not be ascertained.

Meningitis—"Tâche Cerebrale" of M. Trousseau.—Dr. PARKS, in a case of meningitis, had observed the above sign, very decidedly marked. He first noticed it upon the eighth day of the disease; he had tried for it upon the fourth day, unsuccessfully. By drawing the back of the finger across the abdomen, and after about half a minute, a vivid red mark was seen. At one of his trials, Dr. P. gained this appearance in three different lines, or stripes, at once, by making as many passes upon the abdomen. The sign diminished as the disease did, and finally could not be obtained, the patient being (probably only temporarily) convalescent.

Wound of Abdomen by the Accidental Discharge of one of Colt's Revolvers; Subsequent Discharge of the Bullet from the Urethra of the Patient.—Dr. LYMAN BARTLETT, of New Bedford, Associate Member of the Society, reported the case, as follows:—

On the 7th of June, 1854, he was called in consultation with Dr. STICKNEY, of New Bedford, to see a young man who had just received a ball in the abdomen from one of Colt's revolvers, accidentally discharged. Immediately after the accident, he complained of a constant and pressing desire to void his urine. He passed a small quantity, in which a few drops of blood were seen. This prominent symptom continued for three days, being partially relieved, from time to time, by the discharge of urine, sometimes with, at other times without, the use of the catheter.

In the evening of the third day after the accident, while in the act of urinating, the ball escaped through the urethra, and passed, suddenly, and with a "click," into the chamber-vessel.

The constitutional disturbance, for the first few days, was very great; the pulse very small and frequent, accompanied by rigors and delirium. These symptoms gradually subsided, and his recovery progressed favourably after the discharge of the ball, so that in two or three weeks he was quite well.

This case, said Dr. B., will probably be more fully reported by Dr. Stickney, the attending physician.

October 23, 1854. Tuberculosis commencing at the Base of the Lungs.—Dr. BOWDITCH presented portions of a lung in which was seen the very rare example of tuberculosis commencing at the base and posterior part of the organ, and gradually extending upward. He read the following account of the case, as it was given to him by Dr. COTTING, of Roxbury, the attending physician:—

G. A. H.—, aged 26, hardware dealer (salesman and traveller for the firm), married. In February, 1854, had an attack of "lung-fever and pleurisy," from which, after a few weeks, he recovered so far as to be able to return to his business. His health, however, was far from perfect; but, being active and "ambitious," he continued to perform full service through the remainder of March and April. About the middle of May, when first seen by Dr. Cotting, he was seized with a febrile attack, accompanied with a slight irritating cough, and shortness of breath. He was confined to his bed only a day or two. The physical signs were chiefly the following: Great dulness on percussion over lower half of *right* lung, with bronchial respiration and bronchophony in same part; some crepitation or mucous râle immediately above the dull region, and at the very lowest portion of *left* lung. The crepitation in both gradually disappeared; the cough almost entirely left him, and the dyspnoea occurred only after exercise. His pulse fell to 80; his appetite was sufficient, and his bowels tolerably regular. No appreciable improvement took place in the lower half of the *right* lung.

He was excessively uneasy under restraint, and wished to start on a "business tour" south and west. This being objected to, he dissolved partnership, and gave up his business connections. As he had, in February, been advised by his attendant to go to a milder climate, he now resolved to try a voyage up the Mediterranean, and on the 5th of June sailed for Malta. After a disagreeable voyage, and considerable suffering, he reached Malta, July 12. There he consulted an English surgeon, who told him that the lower portion of his right lung was "consolidated." After remaining in Malta for two weeks, he took a steamer for Marseilles, and returned home through France and Eng-

land. He reached Boston, August 25, apparently in about the same condition as he left it in June—certainly not improved in any respect. He was not confined to the house, but passed his time in visiting his business and other friends, walking moderate distances, and riding two or three miles without much effort or inconvenience.

Twelve days after his return to Boston (September 6), he was seized with hæmoptysis, and expectorated about half a pint of florid blood, and a noticeable quantity of frothy mucus. The hemorrhage recurred the same day; and, after an interval of two days, took place two or three times daily for six days more. The whole number of bleedings was fifteen, and the amount discharged five pints. *Treatment.*—Rest, abstinence, blisters, acetate of lead, and opium.

September 14. Hæmoptysis ceased; mucous rattle, with occasional clicks, heard in middle third of right lung (the lower as before), and in lowest part of left lung; other parts resonant and respiration normal. Considerable debility and sickly aspect, from loss of blood, &c. &c. Pulse 80, above which it has never risen, except on some temporary excitement. Almost immediately on the cessation of the hæmoptysis, he began to have febrile paroxysms in the after-part of the day and evening—at first very slight, but gradually increasing in duration and intensity. These were followed by sweats, in the same manner, until of late the clothes are usually completely drenched thereby. A turn of coldness and prostration, in the morning, usually succeeds. The pulse rose from 80 to 90; in a few days to 100; then to 110–115. For the last week it has been 120–130. Respiration 34 to 40 a minute, varying during the same visit. The bowels have been irregular, alternating from costiveness to diarrhœa, from the commencement of this attack. The appetite good, demanding more than could be borne without discomfort.

On October 5, 1854, Dr. Bowditch saw him in consultation. He was then very feeble, could not rise in bed without assistance, and was unable to remain more than a few moments in a sitting posture. Dyspnœa very great; spoke only in a whisper, and preferred to write his wants. Pulse, 122, feeble; sweats copious at night; sputa, ragged, opaque, with a watery fluid; digestion good, though tending to irregularity of bowels; urine dark, slimy. On auscultation, there was evidence of great obstruction in both lungs at their lower portions; coarse crackling at the right; fine explosion of crepitation in the left; above the lower third, something similar was heard in both, mingled with a sonorous râle; respiration becoming most pure at the apex; loud and rough; but the purest respiration was heard in front, under the clavicles.

By Dr. Cotting's account, the crepitation gradually extended to the apex of the left lung, and a week subsequently (October 15) the same thing occurred in the right.

October 15. Much blueness of the nails, and coldness of the extremities; almost pulseless; very great dyspnœa. He died in the evening of October 18.

The autopsy was procured with difficulty, and made somewhat hastily, with the following results: Both lungs were adherent; the right by old, firm, white adhesions, especially at the lower lobe; the left by more recent, soft lymph, effused over the lower lobe and lower half of the upper; pleura of a vivid red injection in the same parts. A large, old, ragged tubercular cavity occupied the major part of the lower lobe of the right; it had exactly the aspect of such cavities usually found at the apex of a lung. The walls were from one-eighth to one-half an inch thick; and numerous firm bands traversed the cavity. A very few crude tubercles in this lobe. The middle was studded with them, and the interstices, though containing some air, were denser than usual. A few small cavities, containing pus, were found in the lower part of this lobe.

The upper lobe was quite œdematous, and a few small, scattered tubercles were found; but generally it was quite healthy, save the œdema.

The lower lobe of the *left* lung was uniformly dense, and felt like the second stage of pneumonia. The upper was pale, swollen, and somewhat emphysematous; tubercles were, however, easily felt in it. On incision, this lobe, as in the other lung, was more healthy at the top than the bottom, the tubercles being very few at the top, and increasing towards its base. The lower lobe presented a smooth, shining, semitransparent, gray cut, and was everywhere solid. It was less friable, less granular than usual in pneumonia, and no pus exuded from the cut surface. A few opaque tubercles in it.

The heart was healthy, but its left cavities were distended by soft grumous blood. Liver, normal size, &c. Other organs, cursorily examined, seemed healthy.

The specimens were submitted to the microscopic investigations of Drs. Ellis and Shaw, and, independently, each of these gentlemen decided that the diseased parts, even those most resembling pneumonia, contained the tubercular corpuscles, and not the usual characteristics of inflammation.

Dr. Bowditch remarked that he regarded the specimens as of deep interest, for they presented the pathological appearances of a very rare form of tuberculosis. Out of more than five hundred cases of phthisis, of which he had records, he had found this anomalous course of the physical signs in only seven. He had never seen such a remarkable specimen as that before the Society. The appearances were exactly the reverse of what usually occur, cavities and greater disease being at the base, and healthy lung at the apex. The diagnosis of these cases is, at times, very difficult. In their earlier stages they are frequently mistaken for pneumonia, the crepitation being exactly similar in the two diseases. The unusual persistence, however, of that rôle for weeks or even months, soon reveals the difference of the two diseases. The absence of most of the usual signs of pneumonia, such as prostration, fever, rusty sputa, and even the frequent absence of all save the most trivial signs of any alteration from usual health, and at times the presence of some of the signs of phthisis, such as slight antecedent cough for several weeks; insidious, though slight, undermining of the health, and, more rarely, a copious hæmoptysis, will enable us to make a diagnosis at an early period.

Dr. C. E. WARE referred to the case of a boy at the Hospital, some years since, who had a cavity at the base of one of the lungs. During the present year, Dr. W. has had a young woman under his care who, after typhoid fever, had serious disease of the lungs. At the upper part of the chest, there was some irregularity in the respiration; but, at the lower part, there was crepitus. There were, accompanying, emaciation, cough, and copious expectoration through the past winter. The patient improved in the spring; and, having passed the last summer in the country, taking cod-liver oil, she has lately been found to have gained fifty pounds.

Dr. J. B. S. JACKSON mentioned a case of tuberculous disease of the lungs, seated at their base. Cavities existed there, but not at the apices.

Dr. BOWDITCH remembered seeing this patient, and supposed then he would recover; death, however, occurred.

Dr. GOULD asked if pneumonic symptoms preceded these attacks.

Dr. BOWDITCH thinks not; or, at least, that it is very rarely the case. These patients often feel well enough to be out and walking, when we would suppose them, rather, fit for being in bed.

Dr. ABBOT asked if these sort of attacks may not be true pneumonia, excited around tubercle, and the crepitous rôle be thus derived?

Dr. BOWDITCH replied that this might be so, but, at any rate, the disease does not yield to antiphlogistic treatment.

Pustule on the Face, of Carbuncular Nature.—Dr. GOULD read the following account:—

Wednesday, September 27, at evening. S. B. L., æt. 18, a young man of excellent habits, exhibited a small pustule on the upper lip, near the junction of the red and white skin. It contained a minute globule of pus, and had a hair passing through it. It was somewhat swollen and hard around it. Regarding it as an affection of the bulb of the hair, I extracted the hair, and directed an astringent ointment to be applied during the night. In the morning, the hardness had extended to the size of a pea, and there was redness around it to the extent of a dime. Advised him to stay at home, and apply small compresses dipped in cold water. I saw him again on the evening of the 29th. The lip was then swollen so as to disfigure the face; the redness had extended half-way up the cheek, and there was considerable pain. The pulse was rapid; and there was stitch in the right side, with hurried respiration. It was proposed to make incisions through the swelling, to which the patient would not consent. Apprehending an attack of erysipelas, and not wishing to deplete, I directed that hot fomentations should be used to the side, that the cold lotions should be continued, and a saline purgative administered.

Saturday, 30th. Pain in side relieved, but breathing still hurried; no modification of respiratory murmur detected; pulse over 100; bowels freely evacuated. Swelling still advancing, without any tendency to subside at its origin, and with no definite line of demarcation. It had a purplish, rather than a vivid red colour; a firm doughy or leathery feel, with no tendency to superficial vesication or sloughing.

Sunday, October 1. Symptoms much the same as to circulation and respiration; swelling still extending in various directions, involving both sides of the face, the neck, and reaching the eyelids. A little pus was squeezed from the lip, but there was no general destruction of the cellular tissue. Lip rigid and dry.

2d. Has had some delirium during night. Swelling not diminished nor much increased; lips stiff and everted, like full-developed African lips, keeping the mouth open, and obliging him to imbibe through a tube. Eyelids not much swollen, but eyeballs somewhat protruded. Delirium and dyspnœa continue. No complaint of pain. Sitting upright, there is flatness at lower right back, doubtless from effusion.

3d. There was a discharge of curd-like matter from the lip on pressure, from several small openings, and the edge of the lips appeared as if worm-eaten. The eyes were frightfully thrust from their sockets, the infiltrated cellular substance oozing out around them; swelling still remaining about the same in extent and amount; no appearances of superficial vesication or sphacelation; delirium constant; no pain; pulse very rapid, becoming feeble; respiration greatly hurried; abdomen tympanitic.

Death supervened on the morning of Wednesday, the 4th, six days from the first appearance of the disease as noticed by the patient.

This disease must be grouped with phlegmonous erysipelas, malignant pustule, carbuncle (and, perhaps, we might add plague), with each of which it has some points in common. In its early stages, we would be led to expect erysipelas, supervening upon a slight lesion; but there is not the bright blush, rapidly returning after pressure; no well-marked line of demarcation between

the diseased and healthy portion; no vesication of the surface, or extensive suppuration of the subjacent cellular tissue; and no tendency to resolution at its origin, as it extends to remoter parts; and, we may also add, in distinction, there is a tendency to extend in all directions. From malignant pustule, as ordinarily described, it differs in not being traceable to animal poison, and in the slight destruction of the parts affected. From carbuncle, it differs in the youth and healthiness of the subject attacked; from its position on the anterior rather than the posterior surfaces; in the rapidity of its fatal course; while there is a resemblance in the color and firmness of the swelling, and in the deposit of matter in numerous small foci at the original centre of the disease.

P. S. Since communicating the above, my attention has been called to the article by Prof. WILLARD PARKER, in the May number of the *New York Journ. of Medicine*, on "A Peculiar Form of Malignant Inflammation of the Lips and Face, resembling Malignant Pustule." On a comparison of the above with the four cases described by him, the points of coincidence are numerous and striking. The location of the pustules especially at the junction of the red and white skin of the lip, is remarkable. They were all fatal, as well as one other which was reported to him, and four others of which I have since heard. The symptoms are as constant and as well marked, and the course as uniform, as in measles or scarlatina; and the cases, though scattered here and there, are sufficiently numerous to justify the fear that a most malignant disease, till recently unnoticed, is about to be added to the catalogue of maladies, perhaps to take the place of the plague, or some of the other terrible scourges which have seemed to be disappearing. I should, however, approximate the disease to carbuncle, rather than to malignant pustule, as Prof. Parker has done, since the condition of the parts, at the final development, more nearly resembles those of ordinary carbuncle. It might be designated Facial Carbuncle; or, if a more technical term be desired, Prosopanthracion.

Dr. PARKS made the following statement of a case of similar nature which he had seen, and which had been formerly alluded to in the Society (April 10, 1854):—

Miss G., ætat. 16, of slender frame, and rather delicate constitution, presented, on Sunday evening, April 2, 1854, a small papule upon the right side of the under lip, resembling somewhat (as Dr. P. judged by the description) one of the old petechiæ of typhus fever. The patient at this time was so well that she walked upwards of two miles so rapidly as to elicit the remarks of friends. Her previous health, so far as Dr. P. is aware, has been good as usual.

April 3. Dr. P. was called, and found the patient in bed, but partially supported in the erect posture by her father, who sat behind her; each expiration accompanied by a short groan; position chosen as affording most comfort to the patient, who complained of pain in one side of the chest, which pain, *a short time previously, had been seated in the opposite side.* The papule had, at this time (twenty-four hours from its first appearance) become the centre of a tumefaction occupying the greater part of the right side of the under lip. The pain in the chest was so intense and sharp that the friends would hardly be convinced of the non-existence of pleurisy; besides, however, that the sudden shifting of the pain from one side to the other militated against this supposition of pleurisy, a thorough auscultation revealed none of the signs of any pulmonary affection. An examination of the chest by Dr. BOWDITCH, twenty-four hours later, confirmed Dr. P. in his diagnosis in this

respect. Supposing the swelling about the lip to be an abscess, it was treated accordingly. Incision was recommended, but not, at the time, acceded to. Dr. P. was, however, summoned in the course of the night, the pain and distress having greatly increased. The patient was etherized (sulphuric ether being used), and the tumefied portion of the lip laid open freely by an abscess lancet. A little blood flowed, but not more than one or two drops of pus, as Dr. P. thinks, and even of this he was hardly sure. The patient was more comfortable for a few hours after this operation.

4th. Swelling much extended, the line of demarcation between the tumefied portion and the surrounding skin being very marked. On passing the finger over the cutaneous surface, the elevation of the diseased portion was felt to be sudden and decided; the swelling was quite hard. The constitutional symptoms were now become intense, and were out of all proportion to the visible lesion. The pulse, at the first visit about 100 per minute, had now greatly increased in rapidity, and was extremely weak; there were stupor and great general prostration. At Dr. Parks's request, Dr. Bowditch now saw the patient in consultation, and, considering the disease probably of an erysipelatous nature, advised the administration of quinia. Concurring with Dr. Bowditch in his view of the case, Dr. Parks resigned the management of it into his hands, on account of one or two puerperal patients then about to fall under his care.

On the following day, Dr. HOMANS, Sen., saw the patient with Dr. Bowditch, and, at that time, suggested its resemblance, in some respects, to *malignant pustule*.

All treatment was unavailing, and the patient rapidly sank, and died between three and four days from the commencement of the attack. Previously to her death, the lip became completely black.

No *post-mortem* examination was allowed.

Dr. COALE had a case some years since, which he thinks much resembled Dr. Gould's. It was in a female, thirty years of age, healthy. She sent for Dr. C. in consequence of much swelling, and intense stinging at the root of the ala of the nose. There were high fever and headache; tongue furred and dry; bowels constipated. This latter symptom was met with an active purgative, and poultices applied to the supposed phlegmon. The swelling continued, transforming the whole upper lip, and half the cheek, into a hard, leathery mass, over which, at intervals, there seemed to be promise of pointing, inducing Dr. C. to incise in two or three places, and, at last, to cut to some depth and length on the inner edge of the upper lip. No true pus was discharged, but only a curdy matter from one or two of the incisions. Opiates, and spts. mindereri were used at first, and afterwards tinct. of bark and brandy substituted, but the patient became comatose on the fifth day after Dr. C. saw her, and speedily sank.

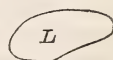
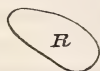
Dr. BIGELOW, Jr., related the case of a medical student, who died last winter with a similar disease. The symptoms were the same. The affection hardly seems of the nature of *malignant pustule*; nor is it *derived*, as that always is. The prostration of the system is as complete as if occasioned by some powerful poison; the whole aspect of the disease is that of the *carbuncle* of old and worn-out constitutions.

Dr. PUTNAM mentioned the case of a lad at school, who, two years ago, had a pustule upon the lip; it was incised; the texture was granular, and spongy; there was a discharge of sanious matter; delirium and coma followed, and the patient died in one week from the first appearance of the pustule.

Erysipelas was prevalent at the time, but this affection did not seem referable to that disease.

Dr. GOULD asked of those who had seen these cases, whether, in the early stage, they had observed anything which would enable them to foretell this disease, so fatal in its result? No affirmation.

Congenital Malformation of the Pupils of the Eye; Strabismus.—Dr. BETHUNE. Charles T., aged 23, seaman; parents not related. Sight imperfect, but can read with *right* eye. The left, which is quite defective in sight, turns somewhat in, and for this the patient applies for relief. On examination, a singular defect of the pupils is seen—they appear as if ruptured at the outer edge, but at different points of the circumference. The following is nearly their outline:—



Dr. COALE had met with two cases of congenital malformation of the iris. One in an infant, where the iris seemed tied down to one side—possibly, the result of intra-uterine iritis. The other was the total absence of a pupil, there being no perforation whatever in the iris. This was in a man thirty years of age. The eyes were dark hazel, and the peculiarity was so little apparent, that the man had been examined for the naval service, and had been passed without the defect being discovered.

Nov. 13, 1854. Falls from a great height; Remarkable Escapes from Injury.—Dr. COTTING, of Roxbury, Associate Member of the Society, related the following instances of escape from injury, after falling from a great height; and, in one, from a railway engine:—

1. An Irishman, at work upon the steeple of the new Roman Catholic church, in Roxbury, fell from a height of fifty feet, by estimate, to the roof of the building, struck upon his hip and back, and slid to the eaves. No injury was sustained by him except some slight contusions; he returned to his work upon the same steeple *in two days afterwards*.

2. S. H., 16 years old, of stout frame, while attempting to disengage the hook of a hoisting apparatus in a store, lost his balance, and fell headlong through the scuttle, from the fifth story, fifty-four feet, by exact measurement. When about half way down, he caught the rope with his left hand, and thus partially came into right position, the feet downwards. Within ten or twelve feet from the lower floor, he caught the rope again with his right hand, and struck the floor upon his feet. He fell fainting into the arms of a bystander, with the exclamation, "All the way from the top, sir!"

Injuries.—*Left hand:* three fingers and the ball of the thumb blistered, not severely; skin slightly torn from the little finger. *Right hand:* flesh abraded from the ball of the thumb one inch in length by three quarters of an inch in depth; from outside of little finger three quarters of an inch in length by half an inch in depth; next two fingers excoriated. *Neck:* chafed slightly on the side, over the sterno-cleido-mastoid muscle, two inches by three-quarters of an inch.

When asked of what he thought while falling, he said that he thought he should be killed if he could not catch the rope; that the rope was secure from slipping, because of the hook's being fast (he was, as above stated, attempting to disengage the hook when he fell); he thought he should be man-

gled and killed; thought of his home and his parents, and of a great many other things. The time occupied in falling could not have been more than one and three quarter seconds.

The common expression with regard to one killed in any similar manner, that such an one "never knew what hurt him," would seem to be disproved by this boy's experience.

3. On November 4, 1854, an engineer on the Providence Railroad fell from the locomotive engine while it was going at the rate of thirty miles an hour. His shoulder was bruised, but there were no fractures, nor any contusions of consequence besides that upon the shoulder.

Dr. J. B. S. JACKSON, referring to the retaining of consciousness at the time of falling from a height, said that he had often been assured by persons thus falling that they had not been sensible of falling.

Dr. C. E. WARE mentioned having once fallen from a height of twenty or thirty feet; he was conscious while falling, and thought of the probable consequences. Dr. W. mentioned another instance: a young man fell forty-five feet from a belfry to the ground, striking upon a wooden pump-handle, and breaking it short off. On recovering from the insensibility resulting from the blow, he was not sensible of remembering anything relative to falling; he recollected only leaving his house just before ascending to the belfry.

Dr. COALE thought it certain that *some time* is necessary in order that impressions may be communicated to the brain; if a person be *stunned*, although there may have been an impression of occurrences immediately previous, these will be obliterated by the concussion received. Dr. C., however, related a remarkable case, in which a sailor was conscious, during a fall of about seventy feet from the foretopsail yard of a frigate. He was aware of striking the foretop in his descent. It is to be remarked, that in cases of falling a short distance only, as from a chair to the floor, if unconsciousness be produced, the obliteration of memory in the person, as to the circumstances, is usually complete.

Dr. PUTNAM knew of a child, five years old, who, while on his way down a flight of stairs in search of a custard which had been promised to him, fell, and sustained fracture of the skull, was unconscious for some days, and lost a piece of bone from the skull; on recovery of consciousness, his first question was in reference to the custard.

Dr. PARKMAN remarked, that where compression of the brain exists, the above is always true of patients; the last idea previous to the accident being the first presenting itself on recovery.

Dr. PUTNAM added, that trephining was contemplated in his patient's case, but was not found necessary.

Ligature of External Iliac Artery.—Dr. S. D. TOWNSEND read the following account:—

Aug. 11, 1854. T. L., æt. 35, Irish labourer, was admitted into the medical department of the Massachusetts General Hospital with eczema of right leg. While under treatment, it was discovered that he had aneurism of the femoral artery of the same side, immediately under Poupart's ligament. It measured three and a half inches in length by two and a half inches in width. Impulse strong, no sound emitted by the passage of the blood through the aneurism.

Sept. 23. The external iliac was tied in the usual manner. When the vessel was laid bare, the rays of the sun shining into the wound, which was about two inches in depth, gave a distinct view of all the parts. The aneu-

rism needle used on the occasion was fixed at a right angle with the handle, and was readily passed around the artery. One hour after operation, pulse 80; temperature of legs nearly equal, about 90.

24th. Pain in the limb during the night; had an opiate, after which slept well. Has passed no urine since operation; bladder emptied by catheter. Temperature of legs, 98; pulse 112. Complains of cough, which gives him pain in the wound.

26th. Temperature of legs about the same; between the toes of right foot, 74; left foot, 86. Cough still troublesome at night; suppuration commenced in wound; skin covered with cold perspiration.

27th. Better to-day; has had no dejection since operation; ordered an enema; pulse 104.

28th. Still continues to perspire; sutures all taken away.

Oct. 3. Improving; ordered two grains of quinia three times a day, with ale.

16th. Twenty-three days after operation the ligature came away. At this time, some swelling, with inflammation, appeared three inches above the upper extremity of incision, extending posteriorly, to which a poultice was applied.

23d. When patient sits up, there is a profuse discharge of pus, by pressure from this part, at the incision.

25th. An opening was made at the inflamed point, giving exit to the pus, with entire relief to the patient.

Nov. 5. He is now in good health, and is able to walk around the ward. No pulsation as yet discernible in the arteries about the foot.

Since the establishment of the Hospital, the external iliac has been tied twice before this, successfully; the common iliac once, and the internal once, unsuccessfully.

Spontaneous Absorption of the Crystalline Lens; Disease of the Vitreous Humour.—Dr. BETHUNE reported the case.—B. H., aged 40, baker; health good. First seen October 17, 1854. Says, that two months ago he suddenly lost the sight of right eye, except perception of light; has not varied in this respect since he first observed it; sees more from each corner, and below, than straight before him. On first inspection of the eye, it appears normal; but, after dilatation by atropine, the lens, slightly greenish, is seen floating in posterior chamber. By examination with the taper, three images are distinct. But the concentrated light of the ophthalmoscope puts the character of the lesion beyond a question. The patient says, that for a week before the attack, he had been unusually exposed to heat and light.

Nov. 12. The same, or sight rather less.

So much of the natural transparency of the eye remains, that the great loss of sight can hardly be accounted for by the *obvious* disease. Taking this, with the fact of the movable appearance of the lens, the *original* seat of disease is strongly suspected to have been in the vitreous humour.

Dislocation of the Crystalline Lens.—Dr. WILLIAMS mentioned a case of dislocation and slight opacity of the lens, seen by him since the last meeting of the Society, and of which the history renders it uncertain whether the displacement was traumatic or spontaneous.

The patient was first under the care of Dr. W. in December, 1852, in consequence of traumatic dislocation of the lens into the anterior chamber of the *left* eye; the eye having been struck by a stick of wood he was splitting. The accident had already given rise to symptoms requiring the extraction of

the lens, which was accordingly performed. At this time, and for nearly a year and a half, vision of the right eye remained as usual. On the first of the present month the patient again made his appearance, complaining of loss of vision in the *right* eye. On examination, a tremulousness of the iris, and appearance as if the lens had been removed from behind the pupil, induced Dr. W. to surmise that displacement or absorption of the crystalline had occurred; and, on placing a cataract glass of four inches focus before the eye, he found vision instantly restored. The patient was now placed in a better light, when the lens could be distinguished, floating in the posterior chamber, and now and then rising so as partly to obscure the pupils. It had become *very slightly* cloudy. Was the right eye struck or concussed at the time the blow was received on the other eye? and was this the cause of the subsequent displacement and opacity? or, on the other hand, may there not have been, in both eyes, some abnormal condition, tending to displacement of the lens, which occasioned its passage into the left anterior chamber on the receipt of a blow, which, under other circumstances, would not have been sufficient to produce such a result?

ART. IV.—*One Hundred and Eighty Cases of Intermittent Fever treated in the Philadelphia Hospital with Sulphate of Quinidia (Quinidine).*—Reported by J. S. DORSEY CULLEN, M. D., one of the Assistant Physicians.

THE increased consumption of sulphate of quinia, and the fears lest the source from which it is derived should fail, have excited the ingenuity of the medical public to find some cheaper preparation of, or substitute for Peruvian bark. Quite recently the attention of the profession has been called to *quinidine*, the newly discovered alkaloid of cinchona, as possessing anti-periodic and febrifuge properties equalling those of quinia.

By permission of the physician in chief, Dr. A. B. Campbell, an opportunity has been afforded the writer of testing the virtues of this medicine in a large number of cases of intermittent fever treated in the Philadelphia Hospital during the last few months. A tabular report of these cases, with the result of the treatment, is subjoined.

From the similarity of their names much confusion has arisen respecting the *quinoidine* of Serturmer ("the amorphous quinia of Liebig"), and the alkaloid *quinidine* here alluded to. It may, therefore, be best to give the following notice of these articles, taken from the new (10th) edition of the U. S. Dispensatory. After adopting, for the sake of an uniform nomenclature, the termination *ia*, the author says: "Besides quinia and cinchonia, there can be no doubt that one other alkaloid, *quinidia*, exists in Peruvian bark, and it is highly probable, that though found most abundantly in the pale, and some of the Carthagena barks, it is contained, occasionally at least, to a greater or less extent, in all. * * * With acids it forms salts, most of which are beau-

tifully crystallizable, and much more soluble than those of quinia. * * * When treated, first with chlorine, and then with ammonia, it does not, like quinia, yield a green colour, nor like cinchonia, a white one, but remains unaffected. It differs from quinia, too, by its much less solubility in ether. * * * The sulphate of quinidia is obtained from the quinidia barks by the same process as that by which the sulphate of quinia is from Calisaya barks. *Quinoidine* (or the substance left after the crystallization of sulphate of quinia, purified by solution and precipitation) consists of the alkaloids, mixed with a large proportion of resinous and colouring matters, into which the salts of the alkaloids have been transformed."

The price of sulphate of quinidia, though less than that of the salt of quinia, is higher than that of cinchonia; but, the fact that it abounds in the cheaper kinds of bark, especially in the Bogota cinchona, from which, at this time, it is extensively manufactured in Massachusetts, induces the belief that it will yet be obtained at a rate much lower than it now is.

The patients treated were chiefly Irish and German labourers, as their names indicate. Most of them had been employed on the canals, the banks of the river, and other exposed situations, which may explain the great frequency of the quotidian type. Many of them, at the time of their admission, by their anemic appearance and enlarged spleens, showed that the disease was of long standing.

In every case, if time permitted, a purgative was given before commencing with the quinidia. Where there was much reason to doubt the truth of the statement made by the patient, as to the type of the chill—as, *e. g.* in the cases of the double quotidian, the medicine was not given until a paroxysm had occurred in the house. The quinidia was administered, in most cases, in doses of three grains every hour for five hours preceding the expected recurrence of the chill. If this failed to prevent the return of the chill, it was repeated on the following day. In cases of long standing, and in others of less duration, where the patient had evidently suffered very seriously from the attack, it was thought best to give him the security of a somewhat larger dose; and, in several instances, twenty grains were given on the first day. In all the cases reported in which the amount taken is a multiple of fifteen, the additional quantity was given on the following day. On the day after the arrest of the chill, each patient was put upon the use of the subjoined prescription, and in this way a decided anti-periodic influence kept up; the chief use of the quinidia being in the first place to avert the recurrence of the paroxysm, which it will be seen it did admirably:—

R.—*Serpentariæ, cinchonæ, gentianæ, ʒiij contus.; ferri citratis ʒss; aquæ Oj.* S. A wineglassful three times daily.

No.	Name.	Age.	Type of fever.	Duration of sickness.	Admitted.	Amount of quinidia required to arrest the chill.	Return of chill.	Discharged.
1	Eliza Hanson ¹	6	Quotidian	3 days		10 grains	None	
2	James McCall ¹	10	"	3 "		10 "	"	
3	W. Toyle ¹	12	"	7 "		10 "	"	
4	John Wolf ¹	9	"	1 day		10 "	"	
5	Michael Ray	38	"	14 days	Sept. 14	20 "	"	Oct. 1.
6	Thomas Kennedy	58	"	14 "	" 13	15 "	"	" 7.
7	Matthew Connor	19	"	21 "	" 16	15 "	"	" 8.
8	John Smith	22	"	10 "	" 18	15 "	"	" 4.
9	Samuel Scott	30	"	8 "	" 17	15 "	"	" 4.
10	Andrew Brady	19	"	3 "	" 19	15 "	"	" 5.
11	August Wineberger	28	Tert. and quot.	6 weeks	" 18	15 "	"	" 5.
12	Thomas Zeitzer	34	Quotidian	8 days	" 17	20 "	"	" 3.
13	Dennis McBride	40	Tert. and quot.	6 weeks	" 19	20 "	Oct. 11	Remaining.
14	Archibald Campbell	40	Quotidian	3 days	" 20	15 "	None	Oct. 5.
15	August Vansant	25	"	6 "	" 19	30 "	"	" 5.
16	Michael McGuire	20	"	6 "	" 19	15 "	"	" 5.
17	Christian Snyder	22	"	10 "	" 19	30 "	"	" 5.
18	Anthony Winters	56	Tertian	4 weeks	" 19	20 "	Nov. 1	Remaining.
19	John Everhart	24	"	7 days	" 20	15 "	None	Oct. 5.
20	John Connor	30	Quotidian	7 "	" 20	15 "	"	Remaining.
21	Philip Scott	23	"	9 "	" 20	15 "	"	Oct. 7.
22	Theodore Sherman	22	"	14 "	" 20	20 "	"	" 5.
23	Michael Nestor	18	Tert. and quot.	21 "	" 20	15 "	"	" 10.
24	Josiah Farren	17	Quotidian	7 "	" 18	15 "	"	" 5.
25	James Scott	34	"	6 "	" 19	15 "	"	" 4.
26	William Stoff	36	"	21 "	" 21	30 "	"	" 7.
27	John Malone	59	Tertian	4 weeks	" 21	15 "	"	" 7.
28	George Roth.	32	"	2 "	" 11	15 "	"	" 2.
29	Alto Stimmel	21	"	7 days	" 20	20 "	"	" 12.
30	Frederick Schletner	28	"	8 "	" 20	15 "	"	" 6.
31	Patrick Smith	19	"	3 "	" 21	15 "	"	Nov. 6.
32	John Homer	52	Quotidian	3 "	" 24	15 "	"	" 7.
33	August Kendrick	40	Tert. and quot.	11 weeks	" 23	15 "	"	Oct. 7.
34	George Foster	40	Quotidian	2 "	" 23	15 "	"	" 10.
35	John Brodinder	30	"	7 days	" 25	15 "	"	" 10.
36	James Campbell	22	"	14 "	" 24	15 "	"	" 10.
37	James Coyle	34	"	4 "	" 23	20 "	"	Remaining.
38	Edward Hart ²	23	"	14 "	" 23	15 "	"	Oct. 14.
39	Charles Voight	36	"	5 "	" 25	15 "	"	" 16.
40	Patrick Flinn	23	"	9 "	" 28	15 "	"	" 18.
41	Laurence Collins	32	"	4 "	" 24	15 "	Nov. 8	Remaining.
42	Charles Harmer	29	"	8 "	" 23	15 "	None	"
43	Henry Mintzer ³	15	"	8 "	" 23	15 "	"	Oct. 20.
44	Patrick Beck	18	Tertian	6 weeks	" 25	20 "	"	" 17.
45	Bartholomew Wadden	30	Tert. and quot.	4 "	" 25	15 "	Sept. 28	" 10.
46	Christian Lutz	20	Tertian	10 days	" 25	25 "	"	" 18.
47	Albert Moore	19	Quotidian	8 "	" 26	15 "	None	" 12.
48	John Latham	40	"	11 "	" 26	15 "	"	Nov. 6.
49	E. M. Cahill	56	Double quot.	10 "	" 26	18 "	"	Oct. 30.
50	William Dougherty	53	Quotidian	5 "	" 26	20 "	Oct. 7	" 22.
51	William Allen	29	"	10 "	" 26	30 "	" 6	Nov. 3.
52	Edward Wagermann	24	"	5 "	" 27	15 "	None	Oct. 13.
53	Frederick Schwartz	22	Tertian	8 weeks	" 27	15 "	"	" 19.
54	Thomas Jackson	21	Tert. and quot.	4 "	" 20	"	"	" 24.
55	John Collins	25	Quotidian	3 "	" 27	30 "	Nov. 8	Remaining.
56	Thomas Conelly	30	"	2 "	" 27	15 "	"	Oct. 15.
57	Alexander McDonald	30	"	1 week	" 27	15 "	"	" 10.
58	James McDonald	21	"	10 days	" 26	15 "	"	Nov. 10.
59	Oscar Jacob	20	"	21 "	" 28	15 "	"	Oct. 5.
60	John Hunter	43	"	10 "	" 26	15 "	4 weeks	"
61	Charles Cutler	40	"	7 "	" 29	15 "	None	Oct. 17.
62	Albert Grassel	20	"	8 "	" 20	20 "	"	" 15.
63	Daniel Homer	39	Tertian	21 "	" 29	20 "	"	" 18.
64	Robert Doyle	27	Quotidian	7 "	" 25	20 "	"	" 15.
65	Matthew Kersey	20	"	7 "	" 30	20 "	"	" 19.
66	Thomas Parker ⁴	17	"	10 "	"	"	"	" 18.
67	Richard Laughlin	40	"	14 "	Oct. 1	15 "	"	" 23.
68	Wilhelm Herr	19	"	10 "	Sept. 29	20 "	"	" 8.
69	Jacob Plank	22	"	14 "	" 10	15 "	"	" 14.
70	Jacob Banbeck	34	"	14 "	" 30	15 "	"	" 14.
71	Charles Hayes	24	"	14 "	" 28	15 "	"	" 14.
72	William Stoff	36	Tertian	7 "	" 29	20 "	"	" 12.
73	Philip Scott	23	Quotidian	7 "	" 29	20 "	"	" 12.

¹ These four belong to the Children's Asylum.² This man was removed to a lower story, and returned to the wards Nov. 1, with a chill, which was checked by 15 grains sulphate quinidia.³ Returned with chills, Nov. 5.⁴ Returned, Nov. 7, with chills.

No.	Name.	Age.	Type of fever.	Duration of sickness.	Admitted.	Amount of quinidia required to arrest the chill.	Return of chill.	Discharged.
74	John Eisenhart ¹	24	Tertian	14 days	Sept. 29	30 grains	None	Oct. 15.
75	Michael Kay	38	"	7 "	" 29	20 "	"	" 20.
76	James Dock	25	Quotidian	15 "	" 18	15 "	"	" 24.
77	William Stokes	19	"	10 "	" 28	15 "	"	" 16.
78	Bryan Buckley	45	Tert. and quot.	10 "	Oct. 3	15 "	Oct. 24	Remaining.
79	John Feeney	30	Quotidian	10 "	" 1	20 "	None	"
80	W. McCormack	40	"	7 "	" 1	15 "	"	Oct. 19.
81	Hugh Gilbert	27	Tert. and quot.	21 "	" 1	15 "	"	" 19.
82	Daniel Sullivan	30	"	14 "	" 2	20 "	"	" 22.
83	Robert Pomeroy	22	Tertian	3 mos.	" 1	15 "	"	" 25.
84	Bernard Daley	30	Tert. and quot.	2 "	" 1	15 "	"	" 14.
85	Caspar Fisher	29	Quotidian	7 days	" 1	15 "	Oct. 17	Remaining
86	August Bretaine	37	"	14 "	" 1	15 "	" 8	"
87	Henry Duncan	40	"	7 "	" 2	15 "	None	Oct. 12.
88	Richard Franklin	25	"	5 "	" 2	20 "	"	Nov. 9.
89	John Rouke	40	"	10 "	" 3	15 "	"	" 1.
90	John Evans	19	Quot. and tert.	8 mos.	" 6	15 "	"	" 8.
91	James Dolas	27	"	1 mo.	" 2	15 "	"	Oct. 27.
92	James Brown ²	14	Quotidian	2 weeks	" 4	20 "	"	" 19.
93	James Connelly	30	"	7 days	" 5	20 "	"	" 20.
94	Michael Toner	32	"	14 "	" 5	15 "	"	Nov. 1.
95	John McEuen	40	Quot. and tert.	4 weeks	" 3	15 "	"	Oct. 15.
96	Hugh Keenan	23	Quotidian	8 days	" 5	15 "	Oct. 26	Remaining.
97	Patrick O'Neill	43	"	4 "	" 6	15 "	None	Oct. 30.
98	Martin Royal	47	"	4 "	" 6	15 "	"	Nov. 8.
99	John Greissenberger	17	Tert. and quot.	8 weeks	" 5	15 "	"	Oct. 27.
100	Michael Henley	23	Quotidian	7 days	" 5	20 "	Oct. 19	Nov. 6.
101	Peter Henley	23	"	7 "	" 5	20 "	None	Oct. 25.
102	Dennis Alexander	27	Tert. and quot.	4 weeks	" 6	15 "	Nov. 2	Remaining.
103	Barney Kane	23	Quotidian	2 "	" 6	15 "	None	Nov. 8.
104	William Thompson	39	Quot. and tert.	5 "	" 8	15 "	"	Oct. 20.
105	George Wilson	27	Quotidian	5 days	" 7	15 "	"	" 15.
106	Frederick Unger	30	Tertian	3 weeks	" 7	15 "	"	" 22.
107	Samuel Parsons	40	Quotidian	6 days	" 9	15 "	"	" 28.
108	Matthew Find	19	Tertian	7 "	" 9	15 "	"	" 19.
109	John Brocker	22	Tert. and quot.	6 weeks	" 9	15 "	"	" 20.
110	John Keef	21	Quotidian	2 "	" 9	20 "	"	" 18.
111	Peter Armbruster	24	"	10 days	" 9	"	"	" 27.
112	John Irlan	27	"	10 "	" 9	20 "	"	" 20.
113	Samuel Homer	18	Tertian	14 "	" 9	"	"	Nov. 8.
114	Stephen Schultz	19	Quotidian	5 "	" 9	20 "	"	Oct. 20.
115	Nicholas Barker	28	"	14 "	" 10	15 "	"	" 22.
116	Martin Mahoney	23	"	14 "	" 10	15 "	"	" 29.
117	Edward Clarke	30	Tertian	8 weeks	" 10	15 "	Oct. 16	Nov. 1.
118	Joseph Baincourt	30	"	3 "	" 10	15 "	None	Oct. 24.
119	Edward Cummerly	28	"	2 "	" 10	15 "	"	" 18.
120	Joseph Miller	19	"	2 "	" 11	15 "	"	" 19.
121	Samuel Scott	35	Quotidian	4 days	" 14	15 "	"	Nov. 8.
122	James Ford	18	Tertian	4 mos.	" 12	20 "	"	" 1.
123	Christian Coutts	23	"	5 days	" 13	15 "	"	Oct. 28.
124	John Marlborough	18	"	3 weeks	" 14	15 "	Oct. 21	" 30.
125	John Bradley	25	"	3 "	" 14	15 "	None	" 28.
126	Arthur McGarrey	30	"	7 days	" 14	15 "	"	" 29.
127	Thomas Griffith	23	"	6 "	" 14	15 "	"	" 29.
128	John Flannegan	21	"	12 "	" 14	15 "	"	Nov. 1.
129	Benjamin Hughes	34	"	9 "	" 14	15 "	"	Oct. 29.
130	Peter Fullner	16	Tert. and quot.	10 weeks	" 14	15 "	Nov. 3	Remaining.
131	Samuel Mahinzy	29	Quotidian	7 days	" 14	15 "	None	Oct. 29.
132	James Kelly	23	"	10 "	" 14	15 "	"	" 30.
133	Adam Glutts	45	Tertian	3 weeks	" 13	20 "	None	Nov. 8.
134	George Cheffle	23	Tert. and quot.	3 mos.	" 13	15 "	Nov. 12	Remaining.
135	Daniel Warren	30	Double quot.	unknow.	" 13	15 "	None	Nov. 2.
136	Godfrey Freund	22	Quotidian	7 days	" 13	15 "	"	" 4.
137	Henry Malley	21	Tert. and quot.	7 weeks	" 16	15 "	"	" 8.
138	Frederick Burt	23	Quotidian	6 days	" 16	15 "	"	" 1.
139	Charles Starr	20	Tert. and quot.	6 weeks	" 15	20 "	"	" 8.
140	Ernest Myer	30	Quotidian	2 "	" 14	15 "	"	Oct. 26.
141	Frederick Deutche	29	Tert. and quot.	5 "	" 17	15 "	"	" 26.
142	Francis Haurte	40	Double quot.	14 days	" 18	20 "	"	" 26.
143	John Brady	27	Quotidian	10 "	" 14	15 "	"	Nov. 1.
144	Louis Pecavet	50	Tert. and quot.	6 weeks	" 18	15 "	"	" 1.
145	Henry Suider	28	"	10 "	" 19	15 "	"	" 2.
146	Mattbias Lanaghan	53	"	4 "	" 18	20 "	"	" 8.
147	Patrick McGuire	30	Tertian	3 "	" 19	20 "	"	" 1.
148	Robert Carnahan	27	"	2 "	" 21	15 "	"	" 1.
149	Frederick Krass	41	"	6 "	" 22	15 "	"	" 1.
150	Simon Henck	39	Quotidian	12 days	" 20	15 "	"	" 1.
151	George Roller	40	Tertian	2 mos.	" 21	15 "	"	" 9.

¹ Returned with chills, Nov. 8.² Returned with chills, Nov. 6.

No.	Name.	Age.	Type of fever.	Duration of sickness.	Admitted.	Amount of quinidia required to arrest the chill.	Return of chill.	Discharged.
152	Thomas Toner	30	Tertian	3 weeks	Oct. 22	15 grains	None	Nov. 7.
153	Louis Miller	23	"	2 "	" 23	15 "	"	" 7.
154	Morris Griffiths	42	Tert. and quot.	3 mos.	" 23	15 "	"	" 7.
155	William Cummings	43	"	2 weeks	" 23	15 "	"	" 7.
156	Lawrence Quigley	26	"	2 mos.	" 23	15 "	"	" 7.
157	Stephen Hauser	33	"	2 "	" 23	20 "	"	" 7.
158	John Connor	30	Quotidian	2 weeks	" 23	15 "	"	" 7.
159	John Kane	27	Tert. and quot.	1 mo.	" 25	20 "	"	" 8.
160	Barney Regan	33	Quotidian	3 weeks	" 25	15 "	"	" 8.
161	William McCormick	32	"	2 "	" 31	15 "	"	" 8.
162	Matthew Connor	19	"	1 week	" 31	15 "	"	" 9.
163	James Brown	18	"	1 "	" 30	15 "	"	" 9.
164	Arvelt Gilderman	32	"	1 "	" 25	15 "	"	" 9.
165	Thomas Downey	35	Tertian	1 mo.	" 23	15 "	"	" 9.
166	Henry McCretzer	30	Tert. and quot.	2 mos.	" 31	15 "	"	" 9.
167	Edward Godan	26	Quotidian	10 weeks	" 31	15 "	"	" 9.
168	John H. Colar	23	Tertian	"	" 31	15 "	"	" 9.
169	Henry Mintzer	19	Quotidian	2 "	" 31	15 "	"	" 9.
170	Edward Keegan	27	Tertian	2 "	" 31	15 "	"	Remaining.
171	Michael Krider	32	"	1 "	" 31	15 "	Nov. 7	"
172	Victor Kergan	29	"	2 "	" 31	20 "	None	Nov. 8.
173	Edward Goden	31	Quotidian	1 "	" 31	15 "	"	" 9.
174	John Williams	43	Tertian	2 "	" 31	15 "	"	" 9.
175	George Parker	39	"	2 "	" 31	15 "	"	" 9.
176	George Connor	27	Quotidian	10 days	" 31	15 "	"	" 9.
177	John Kline	23	"	9 "	" 31	15 "	"	" 9.
178	John Kelly	27	"	8 "	Nov. 1	15 "	"	Remaining.
179	Winfield Wright	39	"	6 "	" 1	15 "	"	Nov. 10.
180	Christian Gildersauss	40	Tertian	10 "	" 1	15 "	Nov. 7	Remaining.

In the preceding table, the writer has endeavoured to state the results faithfully, with little comment, as they were recorded in his note-book at his daily visit to the wards. With him the object has not been to state anything new respecting quinidia, but to *test* the truth of what has been said respecting its worth.

Of the 180 cases recorded, 111 were of the quotidian type, 35 of the tertian, and thirty-one of the tertian and quotidian, *i. e.* began as tertian, and entered the house as quotidian, or the reverse, while there were three cases of double quotidian. This is an unusually large proportion of quotidians, but it must be remembered, that the patients were, at least many of them, of bad habits, greatly exposed, or unwilling to give up their means of support so long as there was not a daily interference with the pursuit of it; besides which, much allowance must be made for what may be their erroneous statements. In 129 cases the chill was arrested by fifteen grains of the salt, and there was no return of it; though it must not be forgotten, that these patients were, as before said, kept steadily under the use of a compound infusion of cinchona with iron. On the seventh, fourteenth, and in those remaining on the twenty-first day, ten grains of the sulphate of quinidia were again exhibited.

Upon the whole, so well convinced is the writer of the merits of sulphate of quinidia, that with him it has entirely superseded the salt of quinia; being, he believes, quite as efficient in the treatment of intermittent fever, while its price, about one-third less than that of sulphate of quinia, renders it especially desirable for large hospitals, among the poor in private practice, and with all who are influenced by considerations of economy.

ART. V.—*On the Climate and Salubrity of Fort Moultrie and Sullivan's Island, Charleston Harbour, S. C., with Incidental Remarks on the Yellow Fever of the City of Charleston.* By JOHN B. PORTER, M. D., Surgeon U. S. A. (Continued from p. 366.)

ENTERING the profession from a northern college, where yellow fever was unknown and the doctrine of Hosack had full sway, I was a believer in the contagiousness of the disease; but my first experience changed that opinion. The two principal reasons for the change were, that the disease could not possibly have been imported, and so spread by contagion; and that the physicians, attendants, &c. of the hospital, were remarkably exempt from the fever. Every subsequent epidemic has strengthened the opinion of its non-contagiousness, none more than that at Fort Moultrie and Sullivan's Island in 1852, so that, at the present time, I would as soon think of becoming an abolitionist as a contagionist.

"Dr. Louis Valentin, First Physician to the French Army at St. Domingo, 1803, is of opinion that the yellow fever is not contagious, as he never observed it to be communicated to the medical men who were engaged in the charge of the sick, and in examining their bodies after death; nor did the clothes and furniture that had been used about persons dead of the fever, though neither aired, washed, nor fumigated, communicate infection to others. Dr. Valentin attributes its origin wholly to local circumstances."—*London Med. and Chirurgical Review*, July, 1804, vol. xi. p. 238.

Dr. Ramsay says:—

"The uniform experience of the physicians of Charleston, since the year 1792, proves that it is neither one nor the other [imported and contagious]; for in no instance has a physician, nurse, or other attendant on persons labouring under this disease, caught it from them. Several, after taking it in Charleston, carried it with them and died in the country, yet it never spread nor was communicated to any one who attended on them. In every such case of mortality the disease and the subject of it expired together."¹

In the *Medical Repository* for 1803, vol. vi. p. 339, is the following passage in relation to the yellow fever in Boston, in 1802:—

"It was remarkable, that if a patient under the disease was carried out of the range of the morbid atmosphere into a healthy part of the town, and attended by persons there resident, the disease was not communicated in a single instance; but not so if he remained on the spot where he took the disease—they less rarely escaped. None of the physicians took the disease, for none resided within the infected atmosphere."

Dr. Rush says:—

"It does not spread in yellow fever hospitals, when they are situated beyond the influence of the impure air in which it is generated."

Dr. Ramsay, on the yellow fever of Charleston, in 1802, says that

"There was no ground to suppose it was either imported or contagious; that no physician, nurse, or other attendants on the sick, caught the disease, &c."

¹ History of South Carolina, vol. ii. p. 87.

Dr. Lebby states to Dr. Strobel (see letter):—

“In August, 1827, three soldiers went from Fort Johnson to Charleston, during the existence of yellow fever, and remained over night; on the ninth day after, they were attacked with the disease, within a few hours of each other: they were not removed from their quarters; during the period of their sickness, ten or twelve men slept in the same room with them, who were alike strangers to the climate; yet there were no other cases of disease in the garrison.”

This is a strong case. The hospital at Fort Johnson was dilapidated and undergoing repairs, consequently it was not in condition to receive the sick, and it was necessary to convert the company quarters into a temporary hospital; in this temporary hospital there were “ten or twelve men alike strangers to the climate,” yet there were no other cases of the disease. It cannot be supposed that ten or twelve men could have escaped if three others had been sick in the same room with smallpox, measles, or ship fever. It was not possible for these ten or twelve men to escape a contagious disease.

Another instance in Dr. Lebby's letter to Dr. Strobel, in September, 1838: The mate of one of the lighters slept one night in the city; on the fourth day he sickened, “and although he was among children at Sullivan's Island, and others who were liable to contract the disease, no other instance of it occurred in the family.”

Here is the residence of a family converted into a temporary hospital; and is it possible that no one should contract a contagious disease? It is said that children are not liable to yellow fever, which is in a great measure true, thus proving, in connection with other facts, that the disease is not contagious; but there were “others who were liable to contract the disease,” yet “no other instance of it occurred in the family.” This seems to show that the fever is not contagious.

What contagion was there in the Fort Moultrie Hospital in 1852? It must be admitted that the physicians, stewards, attendants and nurses of a yellow fever hospital are especially liable to contract the disease if it is contagious. During this epidemic, the surgeon, steward, and four nurses, all continuously exposed for weeks together, had no kind of fever; three nurses and one cook had remittent fever in September, but nothing like yellow fever afterwards; and three nurses and one cook had yellow fever. Thus, we have 14 persons on duty in the hospital, of whom 6 had no kind of fever, 4 had remittent, and 4 had yellow fever. Throwing out the cooks, who were not so much exposed among the sick as the others, but were more exposed to heat in the exercise of their vocation, and we have 6 who had no fever, 3 with remittent, and 3 with yellow fever, and these 12 persons were exposed with the sick in an especial manner. We have, then, only 3 persons in 12 sick in a yellow fever hospital, a circumstance which seems incredible in a contagious disease. All these persons were on hard duty, which would predispose to the disease, yet three-fourths of them escaped. This would not seem possible with a really contagious disease, for instance, smallpox, measles, or even ship fever.

Having had much to say in a former paper in relation to the drunkenness and worthlessness of a portion of the command during the epidemic of 1852, it affords me great pleasure to bear testimony to the good conduct of the remainder, particularly those on duty in the hospital. The attendants on the sick in hospital were constantly on hard service during the day, and at night by detail, in all cases doing their duty with cheerfulness and alacrity, and I have repeatedly seen them carry the sick in their arms, in all stages of the fever, without flinching. If soldiers have some bad traits of character, they have also good ones, as I have had reason to know in several epidemics; the class of reliable men, however, must be carefully separated from the unreliable, in all cases, for duty in a military hospital. Importation and contagion, with their consequences, would paralyze any military hospital, and will create panic among the best troops in the world; and conscientiously believing that yellow fever is not contagious, I took special pains on all occasions to eradicate the pernicious doctrine, emphatically declaring at all times and in the most public manner, that the disease is never "catching" or contagious. On no account can such doctrines, discouraging to both sick and well, be tolerated in a military hospital, especially when we consider them to be false doctrines.¹

Numerous instances might be cited in yellow fever hospitals, where physicians, nurses, and all persons on duty, were remarkably exempt from fever.

How is it with ship fever, the real typhus? Do the physicians, nurses, and attendants of all sorts, escape in these fever hospitals, particularly those in the vicinity of the city of New York, so universally as in the yellow fever hospitals? On the contrary, all these persons are so commonly attacked, and so many have died, including eminent physicians, that it is difficult to resist the conviction of its being a contagious disease.

We know that it is common to say—"there are exceptions;" that "every one is not susceptible to smallpox;" that "persons escape it just as in yellow fever;" &c.; but what proportion of those who have not had smallpox may be expected to escape, and do actually escape? Dr. Gregory says, in relation to susceptibility:—

"All mankind, with few exceptions, are born with a susceptibility of smallpox. This susceptibility, unless altered by vaccination, remains, for the most part, equally strong at all ages." Again: "A few persons pass through life apparently insensible to the variolous virus, whether exposed to it casually or by the mode of inoculation. These cases are exceedingly rare."

It is not so with yellow fever, scores and hundreds in our cities escaping in epidemic seasons, and those who have been exposed in all ways, as physicians,

¹ Every one knows the history of Napoleon, and his entry into the plague hospitals of Egypt and Syria, by which act he showed the philosopher and the man of sense. It is said that one of our generals during the Mexican war, when nearing Vera Cruz, applied to his mouth sponge moistened with vinegar, in order to keep off the contagion of yellow fever. It is clear that our hero was not a Napoleon.

hospital attendants, &c. Again, children and those born and raised in a place where yellow fever is endemic rarely have the disease; but it is not so with smallpox; all are liable to it, in all circumstances, in all climates, and at all ages, from the cradle to the grave.

There is great difference in the recurrence. Those who have had yellow fever are not considered so liable to it as before, but a recurrence is common, and an absence of two or three years in a cold climate renders a return to a fever region hazardous; but not so with smallpox, for a person who has once had it may consider himself safe from all future attacks for the rest of his life, in all situations, as well at the equator as in the frigid zones. On this subject Dr. Gregory remarks:—

“The rarity of such cases may be inferred from the fact that no instances are recorded of persons being received twice into the smallpox hospital; and the instances of alleged smallpox, admitted in that institution have been very few. Some physicians of the last century could with difficulty be persuaded that such ever occurred. Heberden estimated them at 1 in 10,000; other writers at 1 in 8,000, or 1 in 50,000.”

Yellow fever ceases, is annihilated, by the occurrence of frost, like all malarious fevers. No contagious disease is affected in this way by the occurrence of cold weather. Smallpox, measles, Irish typhus, and the typhoid fever of New England, allowing the last two to be contagious fevers, are not rendered milder by frost; none of them are checked by cold weather, and the most of them are aggravated by winter.

Yellow fever does not spread by contagion when carried from a city to the country. This is a well known and established fact, one which has been acknowledged for many years, even by Dr. Lining himself. Dr. Shecut says that—

“The fever which it (the combination of marsh miasmata and animal effluvia) generates in the system, is confined to its own limits, and has never extended to half a mile beyond the limits of the city in which it originated. Of this fact, Sir Nathaniel Johnson appeared to have been apprised, even at the earliest period of the yellow fever. Mr. Hewitt tells us, that while the fever raged in Charleston, in the year 1703, the governor held his head-quarters *about half a mile distance from the town*, not wishing to expose his men to the dangerous infection, unless from necessity; nor do we find that any of his army received the infection, either from the air or from contact, by their communicating with the garrison, in their necessary intercourse with each other.”—P. 96-97.

A better instance occurred more than one hundred years after the encampment of Sir Nathaniel Johnson. Malignant yellow fever raged with great violence in the city of New York, in 1822, and every one who was able fled to the country. Dr. John B. Beck gives the following account:—

“There were a large number of persons who, after having contracted the seeds of the disease in the city, had the disease developed in them after their removal into different and distant parts of the country. There occurred about thirty-six cases of this sort, and at the following places, viz: three at Newark, N. J.; one at Harlaem; three in the city of Jersey; one at Tappan, N. Y.; six at Bloomingdale; one at Albany; three at Middletown-point, N. J.; four in different parts of New Jersey; one at Newtown, L. I.; one at New Canaan, Conn.; two at Amboy, N. J.; one at Hempstead, L. I.; one at West Chester;

one at Bloomfield, N. J.; one at Saugatuck, Conn.; two at Bushwick, L. I.; two at Elizabethtown, N. J.; and one at Boston. Of this number there were twenty-seven deaths. From the very extraordinary proportion of deaths among these cases, it is evident that they must have been very decided and malignant in their character, and yet not in a single instance was the disease communicated." In relation to the case at Boston: "The patient lodged at one of the largest hotels in that place, filled at the time with persons from all parts of the country, and there sickened and died of yellow fever; and although he was constantly watched and attended in a small and badly ventilated apartment, no one was in the slightest degree affected by his sickness."

We see that cases of yellow fever were scattered over the country, sufficiently malignant to produce a mortality of 75 per cent., yet not a single person was in the slightest degree affected.

Again, Dr. Beck says that—

"Upwards of one hundred persons, sick of yellow fever, lay in different parts of the country, without in a single instance, communicating the disease. That one hundred, sick of a disease highly malignant and contagious, located in different parts of the country, should not, even in a solitary instance, have communicated it to a second person, seems to us literally impossible."

The instances related by Dr. Beck seem of themselves sufficient to convince us that yellow fever is not a contagious disease.

We will suppose a contagious yellow fever. A person leaves New York where yellow fever is rife, as related by Dr. Beck, and becomes a member of a family in the country. Soon after his arrival he is attacked with malignant yellow fever, and dies, having had black vomit. The family in which he was residing have very kindly nursed him, and all are taken with the disease in rapid succession, and there was no one able to care for the sick; but in a civilized and Christian country the sick must not be allowed to suffer, and several of the neighbouring families band together to assist this one sick and suffering family; in a short time cases of yellow fever break out in all these charitable families, and so the disease spreads until frost occurs. But why should frost check it? kill it? for no contagious disease dies out in this way when the thermometer falls to 32°. The fever should continue to spread in concentric circles, *transmitted*, if you please, until a vast extent of country is ravaged, nothing stopping it except strict quarantine, a perfect non-intercourse, leaving the sick to take care of themselves, and the "dead to bury their dead," for such is the practical effect of the doctrine of contagion. This is what I would call contagious yellow fever; thank heaven no such fever has ever appeared in this country.

Complete isolation is no protection from yellow fever in an epidemic season. The Misses B——, maiden ladies, related the following case in their family, in 1838:—

"Resided in St. Mary's Street, near Cooper River, Charleston Neck; had the young daughter of a country friend, 13 or 14 years of age, with them, who had not been from the house and yard (detached from other dwellings) for six weeks, and had seen no sick person; she was taken with yellow fever, had black vomit, and died."

If yellow fever is never engendered in Charleston, as has been recently

asserted, and is contagious, how came this young and interesting girl to have it? and why did not the two ladies, who were with her at all times, contract it? See the case of the head master of the grammar school, related by Dr. Ramsay; the cases in the New York and Richmond (Va.) penitentiaries; and the case of Norris, in Augusta jail, 1839, given by Dr. Robertson. These cases, with numerous others, serve to confirm the conclusion in the Second Report of the General Board of Health of England on Quarantine in Yellow Fever: "That when yellow fever is prevalent in a locality, the most rigid seclusion in that locality affords no protection from the disease."

December 1, 1853. Dr. Hume propounds the following questions to the Charleston Medical Society:—

"1. Have you reason to know or believe that the yellow fever of Charleston is always of domestic origin, from sources engendered in the city?" Negated.

"2. Have you reason to know or believe that it is ever of foreign importation?" Negated.

"3. Have you reason to know or believe that it is ever of domestic origin?" Affirmed.

"4. Have you reason to know or believe that the fever is ever the result of a foreign importation, grafted on the impure air of the city?" Affirmed.

"5. Is it the sense of the Society, that quarantine regulations should be enforced to aid in preventing the introduction of yellow fever, in conjunction with strict Hygienic precautions?" Affirmed.

Then comes the Report of the City Council, December 20, 1853, in which Dr. Hume says:—

"We are fully aware that, on this point, we now have the full sanction of the Medical Society of South Carolina,¹ which has been hitherto withheld, and they now abjure the dangerous doctrine, that the rigid enforcement of the quarantine laws was by no means necessary on account of yellow fever."

If the Medical Society has "abandoned the conclusions," there is still room for inquiry, for questions of this kind cannot be settled by a vote of the majority *ex cathedra*. In the minority there may be physicians, having talents, intelligence, learning, experience, and reputation, who are still disposed to continue their investigations, although the Medical Society may have "abandoned the conclusions." The decisions of a society are as liable to be wrong as were the conclusions of their predecessors, particularly as men and measures change; but we regret the expression of the idea—"we rejoice that we never will learn from what observations and facts these results were drawn," for we had always considered the recorded "observations and facts" of astronomers, navigators, and all professors of science, all learned men, those of the law, medicine, theology, &c., as invaluable; and our Saviour "healed the sick, cleansed the leper, and cast out devils"—facts which have been registered for the benefit of the whole human family. Whatever may be the consequences, let facts and observations be sought out and recorded in the most careful manner.

¹ What I have called the Charleston Medical Society is styled the Medical Society of South Carolina, but when the Society was founded, in 1789, Charleston was the State; the Society has always been, in fact, the Medical Society of Charleston. The State Medical Society is termed the South Carolina Medical Association.

We will briefly review the epidemic yellow fever of Charleston; and as Dr. Hume's Report is exceedingly meagre, whole epidemics being omitted, we will have recourse to the writings of Drs. Ramsay, Wilson, Shecut, Joseph Johnson, Simons, and others, to supply the deficiencies.

About twenty years after the settlement of the town on its present site, in 1699 or 1700, yellow fever broke out; and it is remarkable, as stated by Dr. Ramsay, that a desolating fire preceded this calamity. "Never had the colony been visited with such general distress and mortality; some whole families were carried off, and few escaped a share of the public calamities." It was an epidemic year, for in addition to the other misfortunes, a "fatal epidemic of smallpox" carried off great numbers.

1703. Dr. Hewatt states that an

"Epidemic distemper raged at Charleston, which swept off a vast number of the inhabitants; and the town being threatened by the French and Spaniards, the governor removed his head-quarters about half a mile out of town, 'not wishing to expose his men to the dangerous infection unless from necessity.'"

1728. "The summer was uncommonly hot, the face of the earth entirely parched, the pools of standing water dried up, the beasts of the field were reduced to the greatest distress, and an infectious and pestilential distemper, commonly called the *yellow fever*, broke out in town and swept off multitudes of the inhabitants, both white and black." We are informed that the fear of "catching the infection" was great; fresh provisions could not be procured from the country; "the physicians knew not how to treat the uncommon disorder, few could assist their distressed neighbours, and there were so many sick that scarcely white persons enough could be found to assist in burying the dead."

1732. "Yellow fever began to rage in May, and continued till September or October. In the height of the disorder there were from eight to twelve whites buried in a day, besides people of colour. The ringing of bells was forbidden, and little or no business was done."

1739. "The yellow fever raged nearly as violently as in the year 1732. It was observed to fall most severely on Europeans."

1745. "It returned, but with less violence; however, many young people, mostly Europeans, died of it."

1748. Nearly the same remarks as for 1745.

1753. "It appeared again in a few cases, but did not spread."

1755. Same remarks as in 1753. Dr. Ramsay states that from 1732 to 1755, his information was derived from the manuscripts of the late venerable Samuel Prioleau: *Hist. S. C.*, vol. ii. p. 84.

It has been usual to make three eras in the history of yellow fever in Charleston. The first extends from 1699 to 1748, the second from 1792 to 1807, and the third from 1817 to the present time.

"For forty-four years after 1748," says Dr. Ramsay, "there was no epidemic attack of this disease, though there were occasionally in different summers a few sporadic cases of it. In the year 1792, a new era of the yellow fever commenced. It raged in Charleston in that year, and in 1794, 1795, 1796, 1797, 1799, 1800, 1801, 1802, 1804, and 1807. It appeared slightly in the years 1803 and 1805. In both years its victims did not exceed 59. In the years 1793, 1798, and 1808, the disease is not mentioned at all, and in the year 1806 it is only mentioned as having occurred in a very few cases under particular circumstances. The unseasoned negroes were not exempt from its ravages, but they escaped oftener than other strangers; and, when attacked, had the disease in a

slighter degree, and if properly treated were more generally cured. In the years 1796 and 1799, it raged with its greatest violence, but has since considerably abated, both in frequency and violence. This abatement is partly owing to the diminished number of strangers, for strangers have been cautious of residing in or even visiting Charleston in the warm months. It is also to be in part ascribed to a more judicious treatment of the disease. Nevertheless, there is reason to believe that a real abatement has taken place. Nor is this uncommon, for diseases, like other natural phenomena, come and go. Such has been the history of the yellow fever in Charleston from its settlement to the present time. Solitary cases originated in the country, but they were few in number and not often repeated."

It is often asserted, as by Dr. Hume, that yellow fever, unless imported and propagated by specific contagion, should occur every year, the local and general causes being always present; but neither did the sporadic or endemic cases of the disease, mentioned by Dr. Ramsay as occurring in several of the years in which there was no epidemic, kindle into an epidemic, though many of the essential causes might be present; and we account for the presence of malignant fever at irregular intervals with Dr. R., that "diseases, like other natural phenomena, come and go," the epidemic causes being superadded to the endemic ones at irregular intervals. So it is with influenza and dysentery.

1792. It is seen that yellow fever raged in Charleston this year, though the Bulam fever had not yet been imported from Africa to Grenada, for the ship Hankey did not arrive at this island until the next year, 1793; and both Dr. Chisholm and Sir W. Pym state that the ordinary yellow fever of the West Indies is not contagious.

1794. Dr. Rush says that

"The disease was common in Charleston, where it carried off many people, and where no suspicion was entertained of its being of West India origin."

Dr. Joseph Johnson says:—

"The yellow fever appeared in 1794, and in several years successively, generally making its first attack within a fortnight after heavy rains, if succeeded by hot weather and light west and southwest winds, not sufficient to refresh the atmosphere by dispersing the exhalations, and not even carrying off the perspiration. It generally prevailed in boarding-houses, many of them filthy, filled with seamen and intemperate persons; and the very first case of black vomit was in a house of this kind in the year 1794, on what is now Boyce's wharf."

1796. Dr. Rush says:—

"The yellow fever prevailed this year in Charleston, where it was produced by putrid exhalations from the cellars of houses which had been lately burnt. It was said by the physicians of that place not to be contagious."

Here is another remarkable instance of yellow fever after a sweeping fire. Dr. Joseph Johnson mentions the extensive fires of different years as a pre-eminent cause of yellow fever:—

"The first instance of this kind was after the devastating fire which broke out in the evening of the 13th June, 1796, extending from Lodge Alley, near East Bay, to the corner of Broad and Meeting Streets, on which the City Hall now stands. The summers of 1796 and 1797, were terribly marked by the prevalence of yellow fever in Charleston."

1797. The yellow fever of this year was severe. (See the preceding remarks of Dr. Johnson, the cause being the same as in 1796.)

1799. We will draw largely from Dr. Ramsay's Address to the Medical Society, December 24, 1799.

The summer had not been very hot; there were no more than three days in which the mercury rose above 88°. On the hottest day, July 19, the mercury rose to 91°. June 14, and July 15, it reached 89°. Coldest day in the year, January 7, when the mercury fell to 23°; in November it fell to 30°. First frost, November 13. Rain during the year, "rather more than 75 inches." The last months of the year uncommonly wet. September 25, more than 8 inches of rain fell; and "in the months of August, September, and October, there fell upwards of 35 inches."

"One solitary case (of yellow fever) in the month of May, a person thirty days from Havana." In the month of June, some sailors from a Spanish vessel had the disease, and "in the month of July, some other seafaring persons." About the "middle of August it became epidemic." Mortality, 239.

"We have no reason to believe that the yellow fever was imported among us, or communicated by contagion. It raged most in the north end of King Street, where the greatest number of persons from the country resided, and in those streets where seafaring persons usually fixed themselves.

"No physician nor nurse took the disease. Strangers who left the city, and afterwards sickened and died in the country, were not the occasion of death, or even of disease, to those who attended them in their last illness.

"Our knowledge of this fever is very limited. It appears that there is a certain something in the air of Charleston that is comparatively harmless to the inhabitants, but the source of disease and death to the stranger. What is that something?

"The yellow fever is eminently the disease of cities. Where multitudes are crowded together, the dissolution and putrefaction of every animal that dies, and the breath of every animal that lives, tend to contaminate the air. By habit we may accustom ourselves to take poison in small doses, without any deleterious effects. The inhabitants of a city may, without particular injury, inhale a tainted atmosphere that would, in certain seasons, be fatal to strangers, used to the purer air of the sea or country, especially of a colder latitude."

Dr. Ramsay notices the suffering from water in the cellars:—

"It is true, orders were issued to empty the cellars, after they were filled with water. That is, when the nuisance became intolerable to the inhabitant, he was directed to disperse it among his neighbours." And he recommended that power should be used, "to compel housekeepers, in a summary way, to keep their cellars, their stables, their privies, and their yards, wholesome and clean."

The remainder of the Address is in relation to wide streets and narrow ones, confined lanes, dirty alleys, subdividing lots, and crowding houses together, so as to prevent free ventilation, opening and cleansing the present drains, increasing their number, and in removing every accumulation of filth. The Address is worthy of the great historian of South Carolina.

1800. Dr. Ramsay wrote to Dr. Miller, of New York; the fever was not so bad this year as in the last, there being but 184 deaths.

"The disputes about the origin of the yellow fever, which have agitated the Northern States, have never existed in Charleston. There is but one opinion

among the physicians and inhabitants, and that is, that the disease was neither imported nor contagious.

"I have nothing new to communicate on our mode of treating this disease. Under the most approved methods, and the ablest physicians, when clearly marked, it has in most cases proved fatal. Many of our yellow fever patients are in circumstances unfavourable to their recovery. Such is the rapid and violent nature of the disease, that they should have free ventilation, and several attendants by night as well as by day; but they are mostly unknown strangers, and often in crowded lodging-houses, in the most thickly settled parts of the city, where the apartments are small and the servants few.

"In one case, the disease proved fatal to a gentleman on Sullivan's Island, who had not been off the island for six weeks before; in another, to the head master in the College Grammar-School, who had confined himself to the yard and vicinity thereof, from and after the first of July.

"My private opinion is, that our yellow fever is a local disease, originating in the air of Charleston; that the chances of escaping it may be considerably enlarged in favour of strangers residing in the city, who avoid the night air and all excesses; live prudently, and in clean, airy apartments; and observe a medium between living too high and too low; and that the disease might be generally avoided by such, provided they kept entirely out of the city, or even in the sparse settlements in its outskirts. And, on the other hand, that the most transient visit to, or passage through Charleston, when the yellow fever is prevalent therein, may as certainly and as fatally induce that disease as a more permanent residence."

1802. The fever this year was less severe; 96 deaths. From Dr. Ramsay we learn that

"There was no ground to suppose it was either imported or contagious; that no physician, nurse, or other attendants on the sick, caught the disease; and that among the strangers who were so susceptible of the disease, there was no evidence of its being communicated from one to another."—*Med. Repository*, vol. vi. p. 335.

1804. 148 deaths. "A few cases of yellow fever occurred prior to the 10th of July, but from that day till about the 20th of September, it might be said to be epidemic. From and after that time it gradually declined, and finally disappeared about the first of November.

"The weather was uncommonly warm while the epidemic raged, and the number and mortality of its subjects increased with the increase of the heat.

"Neglected intermittents frequently terminated in the yellow fever. The black vomit was neither violent nor constant, even in fatal cases, when the depleting system was carried to a proper extent. Several cases of clearly-marked yellow fever terminated in low nervous fevers, which ran on to a period of two or three weeks, and in different cases with opposite results. The disease bore tonic medicines better and earlier than in the preceding years. Blisters were uncommonly useful, and when applied freely and judiciously, saved several lives.

"About one-fourth of the strangers among us escaped the disease, and more than one-half of those who took it got safely over it. This disease, in no instance proved contagious. Neither physicians nor nurses, attending the sick, were attacked with it."—Dr. Ramsay to Dr. Mitchell, *Med. Repository*, vol. viii. pp. 365-366.

1807. "About the middle of August the yellow fever commenced, though that and the preceding months had been unusually healthy.

"The unseasoned negroes were not wholly exempt from its ravages; some who had taken it retired to the adjacent country, but oftener to Sullivan's Island.

"In both cases, whether they lived or died, the disease terminated with them, and was in no instance propagated from them. I mention this circumstance with pleasure, as it will assist in confirming your learned and judicious observations on the absurdity of quarantine laws. I most heartily wish you success

in your endeavours to illuminate the public mind on that misconceived subject, and to rescue the nineteenth century from the reproach of continuing useless laws, founded in the ignorance and error of a comparatively unenlightened period."—Dr. Ramsay to Dr. Mitchell, *Med. Repository*, vol. xi. pp. 233-236.

Dr. Joseph Johnson, President of the Medical Society, delivered an Address to the Society, December 24, 1807, from which we extract.

The winter previous was very severe. "Occasional showers alone prevented the occurrence of ice on every night in January, except the last." February was a very cold month, and "the changes in February and March were very great and sudden." Both months were very wet; in the first there fell 7.30 inches of rain, and 6.05 inches in March. "The spring was unusually cold and backward; as late as the 3d of May there was a frost."

"The spring having been so backward, the accession of summer was rapid in proportion, and the 11th of June was one of the hottest days to which our climate is subject; the thermometer standing at 92° in a very cool situation, and at 94°, generally, through the city. The average heat of July was 86°; a range considerably higher than had been observed since 1796; and somewhat exceeding the great heat of that year. From the 26th of July to the 18th of August, there had been but one shower; the heat being steady and considerable, the *endemic* *causus* commenced about the latter date, and was aggravated by the extremely hot weather from the first to the fifth of September, when the thermometer, at noon, in the coolest situation, varied from 90° to 92.5°. September was, from sickness and death, the blackest month ever recorded in Charleston, there having been 328 interments; of which 114 were from *endemic* *causes*; and at least one-fourth of the inhabitants were affected with the *influenza* about the last of the month. From the 2d of October to the 5th of November, there had been no rain, and on the twenty-one days preceding, there had fallen only 0.15 inch. During the first six weeks of this time, the weather was clear, and generally calm; but when the winds did increase, the inhabitants were distressed still more by the clouds of dust which drifted in every direction; the winds, fortunately, prevailed from the east, so that the heat was not oppressive. All the ponds, and many of the wells and springs were dry, so that in country places many cattle died for want of water, and travellers could not obtain a sufficiency, near the roads, for themselves or their horses. The atmosphere was hazy and thick, as if filled with a subtle dust, and the sun, as if deprived of its rays, appeared of a fiery redness. On the 20th of October the weather became cool, and a frost took place within two miles of the city, which being occasionally succeeded by others, the atmosphere was cleared, and the effects of the drought were not so oppressive. It may, however, be said to continue even at this time (December 24), for from the 11th of September to the 21st instant, a term of three months and ten days, there had fallen only 1.40 inch of rain.

"If all human strength, supported by medical skill, cannot avail against the furious attack of this monster, may not enterprise, conducted by observation and reason, prevent its origin. Observation points to the drains and other receptacles of filth, and reservoirs of stagnant water, as the sources of its being. Reason convinces us, that if these receptacles of filth were daily cleansed of their putrefying contents, so as to prevent exhalation, this hydra could not exist. Enterprise, confirmed by experience, assures us that water may be conducted through our streets, so as not only to remove the fermenting matter from the drains, but answer many other valuable purposes."

Both Drs. Johnson and Ramsay give a graphic description of the distress occasioned by the epidemic *influenza*. Our limits permit only a short extract from the former:—

"*Influenza* is noticed as occurring in March and April, although not sanc-

tioned by our journals.¹ My opinion was then supported by the concurrence of several eminent physicians, and has since been confirmed by a correspondence of symptoms with that which prevailed in the fall. On the first of its appearance, in the latter period, the symptoms were so mild that few required the attendance of a physician. This lulled many into a fatal security.

"So general was the prevalence of this disease about the middle of October, that many families had from fifteen to twenty sick at one time; servants could not be hired to do the duties of a family, nor nurses to attend the sick. Relapses were very frequent."—*Med. Repository*, vol. xi. pp. 402–407.

1809. Dr. Samuel Wilson writes to Dr. Mitchell, August 31:—

"Our city has been unusually healthy until lately, owing to the late uncommon fall of rain; for since the first of last month, 22 inches of rain have fallen, which is almost half as much as falls in a year, taking the average of 15 years past. This I can assure you is a fact, as my father keeps an accurate account of the weather, and has done so ever since the institution of the Medical Society in this place; and as the vessel was imported from England, the quantity has been regularly ascertained. From the great fall of rain, we have had a remarkably temperate summer as yet, and were flattering ourselves with the hopes of an exemption from the yellow fever, but this has unfortunately not been the case, for about eight cases have occurred among strangers, and, as is usually the case, the whole have terminated fatally, for I do assure you that very few recover, generally; especially such as are seafaring people, who are seldom taken to the Marine Hospital till the third day from the attack; at least this has been the case this season; and I believe, with one or two exceptions, the disease has been confined to this class of people. As to the origin of the disease, there prevails but one sentiment, and that is of its being engendered here; no better proofs of which can be given than the circumstance of its being confined exclusively to strangers, and the sudden disappearance of it when the cold weather sets in."—*Med. Repository*, vol. xiii. pp. 200–201.

This is all we find in relation to the fever of 1809, except that "on the 6th of October the Governor of New York, by proclamation, directed the quarantine laws to be enforced against vessels and persons arriving from Charleston."

1811. Dr. Joseph Johnson, speaking of devastating fires as a cause of fever in Charleston, and the "terribly marked prevalence of yellow fever" in 1796 and 1797 in consequence, says:—

"So was the summer of 1811, subsequent to that fire which commenced on the evening of the 9th of October, 1810, back of St. Phillip's Church, which extended on both sides of what is now State Street, to Broad Street and East Bay."

These are the facts and observations which Dr. Hume wishes to suppress. These are a portion of the facts and observations from which the "results were drawn" by the eminent men of the profession, which Dr. Hume deprecates, and which he says the Medical Society has abandoned; and the doctor "rejoices to believe that we never will learn" from what the results were derived. This is truly liberal for the nineteenth century.

We have finished the two first periods in the History of the Charleston yellow fever, extending from 1699 to 1807, 1809, and 1811. During the first period, from 1699 to 1748, the facts and observations are not so full as in the second; nevertheless, those which have been preserved by Lining, Hewatt, Chalmers, John Moultrie, and others, are important. Dr. Lining

¹ City epidemics are never "sanctioned" by the city journals."

believed in the contagiousness of the disease, but he distinctly admits that it could not be spread in the country by the sick carrying the fever from the city, "not even so much as to one in the same house;" and he could have known nothing of the first three epidemics, in 1699, 1703, and 1728, as he was not in the country; nor does he give a single instance of importation from the West Indies, or any other part of the world. Indeed, how could the disease be imported from the West Indies to Charleston at this early period, when Dr. Chisholm and Sir William Pym declare that contagious yellow fever was first imported into Grenada from Bulam, in Africa, in the year 1793, and that the fever of West Indian origin is not contagious?

The first two or three epidemics in Charleston could not have been imported from the West Indies, for there was no foreign trade, the infant colony being satisfied, according to George Chalmers, during the first years of its existence, to have a precarious intercourse with the lords proprietors in England.

Whoever examines the writings of Dr. Ramsay, and the old maps of Charleston, will find that the site of the new town was cut up by creeks and covered by marshes; and from the necessary general condition of the new settlement in this hot climate, it is not half so surprising that yellow fever should be engendered here, as that it should occur at the settlement of old Massachusetts colony, with its rock-bound coast and raw climate.

According to Dr. Hume, "Charleston is beyond the northern limit of *self-generating* epidemic yellow fever," though he admits that "Charleston does occasionally produce sporadic cases of fever of domestic origin, but they do not extend or infect localities." They ought to do both, if yellow fever is a contagious disease, just as sporadic cases of measles and smallpox "extend and infect localities." The term *self-generating* is objectionable; but if Dr. Hume intends to say that Charleston is too far north to ever have yellow fever produced by causes within itself, independent of importation and contagion, and that it cannot exist unless imported and spread by contagion, then we understand the meaning of the term, and wholly dissent from the opinion. This disease was certainly produced, in Charleston, by local causes in 1852, as it was indubitably produced on Sullivan's Island by similar causes in the same year.

Yellow fever has been produced by local causes far north of Charleston. It broke out in the most decided manner among the convicts of the New York State Prison, in 1805, in a "situation entirely inaccessible to imported contagion." (See Drs. Walker and Quackenbos, *Med. Repos.* vol. x. pp. 58-61.) The disease originated from local causes in the Richmond Penitentiary, Va., in 1806. We have room for only an extract or two; for the remainder, see *Med. Repos.* vol. x. pp. 215-218.

"Where is there a fact more conclusive as to the origin of yellow fever, because so little confused by the operation of contrary causes? It would almost be a miracle had the *imported germ* of the yellow fever visited the penitentiary. Here are a set of men almost completely cooped up from the rest of the world. The turnkeys, the respectable superintendent, his whole family, are healthy.

The guard, who traverse the outside of the building, are too far remote to communicate the contagion. Few persons have access into the building; no one can enter it without a permit from two inspectors; no one who has visited it lately has exhibited symptoms of the fever. The building is too completely cut off from the city to receive the noxious contagion in a tainted stream of air. And whence was this stream to blow? There is not a single corner in the city where the fever has peeped forth—not one person who has fallen beneath it.

“There seems not a deficient link in this chain of proof; everything is complete—everything conclusive. It is scarcely possible that the fever could approach from without; it must, therefore, have been generated from within. But *here* are sufficient causes for its production: desponding minds; the want of exercise; the want of something to exhilarate the spirits, and put the torpid functions into motion; the damp weather succeeding to a season of unexampled drought; the long train of inconveniences, inseparable from a state of confinement and labour. When causes like these exist, why seek for its origin in the West India market, whence no infected vessel has arrived; or in a city not yet visited by a single symptom?

“That the fever of the penitentiary is the yellow fever of our cities is decided by the two *experienced* and ingenious physicians who have attended it.

“The best established opinion is, that the fever is not in the least degree infectious; that it rather passes from the air to the patient than from the patient to the air; that even in that case, it does not immediately operate, but that it requires time to break down the tone of the body, ere it can produce its effect.”

Our limits will permit of only a simple reference to the yellow fever in New London, Conn., in 1798; Norfolk, Va., in 1801; Gallipolis, &c., not forgetting Philadelphia, in 1853. The instances of the two penitentiaries are sufficient to disprove the singular assertion of Dr. Hume about the “northern limit.”

Dr. Hume makes another strange assertion. He says that

“It is the property of epidemics never to die in the place of their nativity. Yellow fever does die in our climate, for it is not renewed each successive summer, while at Havana it flourishes in perpetuity. If the fever is not a native of the West Indies, it is, at least, naturalized, but it cannot be said to be naturalized in Charleston. Our winter extinguishes it effectually.”

It is well known that dysentery, influenza, &c., are often epidemic, and that they as often cease or “die.” Billious fever is endemic to the southern part of the United States, and when unusual or violent causes are superadded, a tremendous endemic ensues, as at Fort Gibson, in 1834, when we saw men as yellow after death as in yellow fever. Yellow fever certainly did “die” at the New York State Prison, at the Richmond Penitentiary, New London, Norfolk, Gallipolis, and Philadelphia, in 1853. There was malignant yellow fever at St. Augustine, Florida, in 1839, which *died*—the winter “extinguished it effectually”—and there was not another case in the town until 1841, when a malignant fever originated there from local causes, and many persons died. There is no dispute but the fever of this year (1841) originated there; there is no sort of apology for considering it as imported; and no one pretends to point out from whence it was imported; yet this fever died out, and there has not been a case since, thanks to a better system of police. There was yellow fever at Tampa, Florida, in 1839; and at Pascagoula, Miss., in 1848, originating there, which ran the same course.

Yellow fever is endemic in the cities of the low country on our southern coast, including Charleston; and "sporadic cases of fever of domestic origin" occur, as in the case of the drummer at the Citadel Academy, Capt. Cale's lady in 1852, Barkelew in 1853, &c.; but if other causes are added to the endemic condition, thus adding epidemic to endemic influences, malignant epidemic yellow fever will result.

Dr. Chalmers's opinion of the origin of the disease is declared when he speaks of the "narrow lanes and alleys, and more are daily laying out," and the "confined situations," which he predicted (truly) would "hereafter prove a nursery for diseases, not of the most tractable kind, when the town becomes large and is more closely built."

Dr. John Moultrie did not believe the disease to be contagious:—

"I cannot believe, with others, that this disease is propagated from one place to another by contagion, unless the atmosphere is in a fit state to produce it. In the year 1745, this disease prevailed to a very great extent in South Carolina, when it was very evident that contagion from some other place had no influence in producing it. For the first person seized with the disease that year was a sailor, who, having been drunk for two or three days in succession, afterwards fell ill of this fever, and the heat continuing excessive during the month, the fever spread widely throughout the city of Charleston. For when the weather is hot, there is danger lest an ardent fever should change into this disease of a more violent character; and I very little doubt that this is also occasioned by improper remedies and bad treatment."

Dr. Hume's Biographical Sketch of Dr. Moultrie is not very flattering, but let us see whether he was really, or not, an ignoramus. Dr. Ramsay says that he was one of the four who obtained a degree from a college "for the first ninety years which followed the settlement of South Carolina." "He was the first Carolinian who obtained the degree of Doctor of Medicine from the University of Edinburgh, when, in 1749, he defended a thesis, *De Febre Flava*." This thesis was translated into several modern languages. Dr. Moultrie had seen plenty of yellow fever in Charleston previous to writing this thesis; his language shows it, though Dr. Hume says that he "could scarcely have commenced the study of medicine in 1745, when the fever prevailed;" and that he did not do more than "any other young graduate would do who had to write a thesis on what he had scarcely seen." Let Dr. Moultrie speak for himself: "In the year 1745, this disease prevailed to a very great extent, &c." (See the foregoing passage.)

I believe that no one will deny the fact of my being present on Sullivan's Island during the epidemic yellow fever of 1852, and that I saw the disease, which I will describe in the language of Dr. Moultrie: "In the year 1852, this disease prevailed to a very great extent on Sullivan's Island, when it was very evident that contagion from some other place had no influence in producing it. The first person seized with the disease was a sergeant, who, &c." Will any candid person deny that Dr. Moultrie was present during the epidemic of 1745, and saw the disease, or that I was present and saw the fever during the epidemic of 1852?

Dr. Hume says: "It appears that Dr. Moultrie did not inquire from whence the sailor came, and it is possible that he was one of those persons, easily traced by Dr. Lining, as coming from some of the West India Islands"—and it is possible that he was not one of those persons, nor is it at all probable, for Dr. M. asserts positively that contagion had no influence in producing the disease, and we choose to believe his positive assertion in preference to Dr. Hume's conjecture; nor is it so very strange that a sailor who had been "drunk for two, or three days in succession," at the same time the "heat continuing excessive," should be the first victim. Who knows that Dr. Moultrie did not "inquire from whence the sailor came?" Is it at all probable that Dr. M. made his statement without inquiry? Did Dr. Lining, or any contemporary, ever deny the truth of Dr. Moultrie's account? Dr. Hume's assertion is wholly gratuitous.

We do not think highly of the practice of disparaging a member of the profession because he does not think as we do. We might assert that Dr. Lining knew but little of the yellow fever in his limited practice, and by a slight perversion of a passage from Dr. Ramsay—"His fame was much more extensive than his practice"—might sustain ourselves; but will we attempt it?

In the second era of yellow fever, from 1792 to 1807, 1809, 1811, we have abundant evidence that it was not contagious, and that there was not a single instance of importation. How could it be imported and spread by contagion? for the fever ravaged Charleston in 1792, and Dr. Chisholm says the disease was not imported into the West Indies until the year after, 1793; and both he and Sir W. Pym have decided that only this imported Bulam fever is contagious, the original yellow fever of the West India Islands being, according to them, non-contagious. In this second period, there is not a single instance given of its being imported and spread by contagion, and Drs. Ramsay, Wilson, and Joseph Johnson, give testimony against its importation and contagiousness. Dr. Ramsay was in active life, and saw those epidemics, from 1762 to 1807 inclusive; and he unequivocally and publicly stated that the fever was not contagious, and no contemporary member of the profession controverted the opinion.

Dr. Hume says that "it is no honour to Charleston to be the parent of such a pestilence; it is no credit to her sons to endeavour to prove her to be." Is it a dishonour to be the "parent of such a pestilence," unless by such measures as were pursued in 1849 and 1852? making excavations and throwing up mud and refuse soil in all parts of the city? Is it no "credit to her sons to endeavour to prove" the truth? Must they give up the investigation of disease, record falsehoods, and suppress facts, for the special benefit of the few? Ramsay, Wilson, Johnson, Shecut, and others, were not the men for this work; they had too much integrity: they recorded facts.

From 1699 to 1811, embracing the first two periods of yellow fever in Charleston, or 112 years, there is not a single well authenticated instance of imported and contagious yellow fever; if one occurred, let it be published, for it has not yet seen the light.

ART. VI.—*On the Treatment of Ununited Fracture by means of Artificial Limbs, which combine the principle of Pressure and Motion at the seat of Fracture, and lead to the formation of an Ensheathing Callus.* Illustrated by the History of *four* cases of False Joint in the Femur, *eight* in the Leg, and *two* in the Humerus. By HENRY H. SMITH, M. D., Consulting Surgeon and Lecturer on Clinical Surgery in the Philadelphia Hospital, and Surgeon to the St. Joseph's Hospital, Philadelphia. (With eight woodcuts.)

THE function of the bones of the extremities being to sustain the weight of the body, or aid in locomotion and prehension, any destruction of their continuity impairs the action of the muscles which are attached to them, and destroys the usefulness of the member. In accomplishing these functions, the bones are passive, and, therefore, resemble levers which are acted on by forces applied throughout their length. As when a lever is broken the splicing of the fragments may restore its usefulness, so, when a bone is fractured, the temporary and artificial union of its fractured ends may enable it to perform its functions nearly as perfectly as before the occurrence of the accident. Under ordinary circumstances, the reparation of the injury is the result of a natural action; but when this fails, no means of treatment seem more reasonable than that afforded by the application of such a constricting band as will afford artificial support at the seat of the injury. Saw a bone in two, and surround it at the point of division with a ferule, and it will again prove a lever of sufficient strength to sustain certain powers. Carry out the same ideas in an ununited fracture, and modify the means of support at the fracture, so that they may act properly on flesh and blood, and the nearly normal condition of the limb will be restored, or the patient, at least, will be enabled to use it for a great variety of purposes.

In the ordinary treatment of fractures, nature furnishes the bond of union, and, therefore, but little aid is required from the surgeon—rest and the apposition of the fragments being sufficient to enable her to accomplish the cure. But, when she has failed in the performance of this important action, surgery has always been ready to step forward and interfere, and, in some instances, has done so with a rudeness that has terminated either in the loss of the limb or the life of the patient.

In inviting professional attention to a successful plan of treating false-joint, which embodies the principles of pressure and motion in the part, I hope to induce surgeons to admit that a less dangerous and inconvenient method than the seton, resection, caustic, or wiring of the ends of a fracture, may do as much, if not more, than either of these well-known surgical means of treating false joint. With a view of showing the defects of many of the plans of treating this affection, heretofore recommended, I shall briefly enumerate

them, and then, after referring to the pathological conditions of the parts, as well as to the more recent physiological views of the formation of callus, cite a few cases, as illustrative of the advantages of pressure at the seat of fracture, when combined with the stimulus of gentle and frequent motion of the ends of the broken bones.

The formation of callus being due, under ordinary circumstances, to increased vascular action in the part, the means of treatment generally suggested for the cure of false joint have assumed the necessity of stimulus as a fundamental point. With the view of hastening nature in the formation of the much desired bond of union, and in some instances without much reference to the difference in the state of the bones in a recent fracture, and in one which has existed many weeks, surgeons have urged the employment of local stimulants to all cases, without specifying the conditions which demanded it. Of course, means which were useful in one period of a fracture, could not be equally so in another; yet, but little distinction has, in many instances, been made between the treatment of a false joint of three, or of eighteen months' standing. Thus, friction might be useful in the first, but of no service in the last; whilst resection might be attempted in an old case, and yet not be justifiable in a recent one. The following plans of treatment, which have been suggested from time to time, sufficiently illustrate the views of surgeons in the pathological condition of false joint, as well as the ingenuity of each surgical period, and its dissatisfaction with the means of treatment previously recommended.

The most ancient of the plans of treatment proposed for the consolidation of a false joint, was friction of the ends of the bone. This was advised by Celsus, and subsequently recommended by John Hunter and others; and when followed by rest, sometimes accomplished the cure of cases in which union was rather retarded than entirely absent. It is generally regarded as especially applicable to the very recent cases of fracture in which the so-called callus does not harden, or is not sufficiently abundant. Next to the direct excitement of the local circulation by irritating the bone, was its development by indirect means, such as blisters and caustic issues, applied to the soft parts around the fracture, as advised by Dr. Joseph Hartshorne, of Philadelphia, in 1805; Walker, of England, in 1815; and subsequently by Wardrop and Brodie, as well as by Velpeau. The use of the tincture of iodine, as an injection into the fracture, and of stimulating liniments and plasters, also belongs to this class; whilst electricity, galvanism, and acupuncture, are only modifications of the same idea. These means, like friction, necessarily develop a certain amount of vascular action; but can only prove useful in the recent cases of delayed or tardy union, rather than in the more ancient forms of false joint. As a more certain and permanent stimulant, we have next the suggestion of the introduction of a seton between the ends of the bones, as advised by Winslow, and urged, with especial reference to the humerus and jaw, by the late Dr. Physick, of Philadelphia. The employment of this means of ex-

citing action was, however, far from being invariably followed by consolidation, though the patient wore it as directed for four or five months; and, in accordance with recent views, does more harm than good.¹ In the case of false joint in the thigh, it generally failed so completely that it was admitted by Dr. Physick to be a very doubtful remedy in the treatment of this condition of the fragments in fracture of the femur.²

In more obstinate cases, it was proposed by White, of England, and subsequently practised by others, to *resect* the ends of the bones which constituted the false joint, establish a new surface, and then, by rest, attempt a cure; but so dangerous has this operation been deemed, from the severe constitutional excitement it often produced, that the celebrated French surgeon, Sanson, was induced to think that "all prudent surgeons would agree that it was better to leave the patient with his infirmity than to attempt to cure him by this operation." As illustrative of its dangers, I would refer the reader to the details of a case in which I performed it on the bones of the leg,³ and in which the patient barely escaped with her life. Where the surgeon has not wished to saw off the ends of the bones, he has endeavoured to create a new surface by the application of caustic or the actual cautery; whilst, in order to insure the rest and apposition of the fragments, when thus freshened, either by caustic or the saw, they have even been wired or tied together, and yet the operation has failed in accomplishing a cure. The development of a new action in the part, by inserting ivory pegs into the bone, near the fracture, as suggested by Dieffenbach, or by subcutaneous drilling of the ends of the bone, as recently advocated by Dr. Brainard, of Chicago, together with a variety of similar means, are additional instances of the difficulties and defects of preceding plans, and of the efforts of surgeons to remedy the evils of the other methods which have been suggested.

In all these varied plans of treatment, it is important to remember that the patient has either been confined to bed for many weeks, at the risk of losing his general health, or, after suffering from the performance of an operation, been exposed to excessive suppuration, phlebitis, hectic fever, or purulent absorption, especially when the bones were resected. They are, therefore, not only attended with great inconvenience, but often with great danger, whilst frequently their failure has necessitated the amputation of the limb.

In most of the means of treatment thus briefly alluded to, it is easy to notice the effect of the pathological description of the condition of parts in false joint or ununited fracture, as furnished by Boyer, to wit, that, in ununited fracture, a true joint, or a condition approaching it, is usually found, in which may be noted the rounded surface and smooth face of an articulation, as well as a capsular ligament with synovial fluid. The fact that this descrip-

¹ See Transact. Am. Med. Assoc. vol. vii. p. 557. Prize Essay of Dr. Brainard, of Chicago, 1854.

² Dorsey's Surg. p. 135.

³ Am. Journ. Med. Sci. vol. xv. N. S. p. 84. 1848.

tion has been so generally received as correct, might be supposed to be proof of its exactness; yet more detailed and extended observation has shown that, though frequently existing, this condition is not invariably seen, the bones being sometimes joined by a fibrous tissue, and often deficient in anything like a capsular ligament, synovial membrane, articular cartilage, or fluid.

As it is essential to a correct plan of treatment that we should possess a correct knowledge of the pathological condition which is to be relieved, a reference to the varied changes of structure found in an ununited fracture, and often modified both by time and position, will prove useful; whilst an allusion to the ordinary steps of reproduction in bone, may tend to elucidate the *modus operandi* of certain plans of treatment.

The most ancient theory of the formation of "callus" was that the new bond of union was due to an exudation of a gelatinous or viscid fluid (like glue) around the seat of fracture; that it was the product of effused blood, and that this viscid fluid kept the bones in contact, and gradually hardened into an osseous condition. This doctrine seems to have prevailed from an early period, and to have continued in an indefinite state until the experiments of Duhamel, Dupuytren, Breschet, and others, showed the agency of the periosteum in causing the formation of the mass, and the analogy existing between the repair of injury in a bone and that seen in the repair of a wound in the bark of a tree.

After following for many years the opinions of the French school in relation to the growth of bone and the formation of callus, physiologists have recently had their views materially changed, and their ideas enlarged by the developments of the structure of bone, as made by (that great modifier of physiological doctrine) the microscope. The existence of the Haversian canals, spaces, and ossicles, as well as of the osseous granules and cartilaginous cells, having all been thoroughly demonstrated, the study of the formation of the bony tissues has been pursued with an accuracy that has done much towards the advancement of a more correct knowledge of osteogeny, and with it of the process pursued by nature in the production of callus.

As the results of a minute and extended observation by microscopists, and especially of the opinions expressed by Mr. Paget,¹ it is now admitted, "that in the fracture of a long bone the injury is seldom limited to the bone, but that the adjacent tissues participate in it; that the periosteum, though rent, is rarely much damaged, and seldom stripped off the broken ends; that the extravasation of blood about the fracture is not only uncertain, but unequal in the different tissues, being often very slight in the periosteum; that the cellular tissue in and near the seat of the injury, is soon infiltrated with lymph, or a serous-looking fluid, which contains cells like those of granulations; and that the so-called *callus*, in its early stages, does not differ materially from the material formed for the repair of other subcutaneous injuries; that the

¹ Paget's Lect. Surg. Phathol. Phil. edit. p. 160, *et supra*.

bones are ultimately united by the formation of granulations, in which are found nucleated cells as the embryo forms of fibrous tissues, and that this structure becoming subsequently the seat of granular osseous deposit, leads to the formation of an intermediate bond of union, rather than an "ensheathing callus," the latter being, in Mr. Paget's opinion, rarely found in the fractures of the human subject, though often seen in animals. When seen in man, it has been in the ribs, clavicle, and humerus, and was due, in his opinion, to the disturbance of the proper mode of repair by motion.

In a case of ununited fracture of the femur in a hale man of 20, recently placed under my care, the condition of the ends of the bones exhibited in a very marked degree the effect of motion in inducing the formation of this provisional or ensheathing callus, there being a mass around the end of each fragment, as felt through the soft parts, which created a tumour nearly of the size of a child's head at two years of age. In this case, there had been imperfect rest throughout the treatment, and nature had, therefore, apparently acted in this man in the manner often seen in the experiments on animals, and thrown out an amount of new material capable of sustaining the bones under these circumstances.

It is an inquiry worthy of note in passing, whether, in the treatment of fractures, very perfect rest may not be one cause of the deficiency of the new bond of union, and consequently lead to the formation of a false joint. In the cases of false joint, hereafter reported, moderate motion has certainly induced union, and this question should therefore receive attentive consideration, especially in treating cases of tardy union not actually resulting in false joint, lest it prove that rest in the treatment of fractures has been too strongly insisted on.

In studying the formation of the "intermediate," or, as it is more frequently termed, "the definitive callus," Mr. Paget says, that the course of nature in its formation is well exemplified in the condition of the end of a bone after amputation. In this case, as well as in compound fractures, granulations, he remarks, "are always seen to form within and on the medullary canal, and gradually to protrude like the rounded head of a mushroom;" a description which every surgeon has doubtless verified in the delayed union of stumps, and there is, I think, no doubt that in old fractures, as in amputations, the ends of the bone are disposed to take on a similar action, and that the granulations formed on the ends of the fracture lead sometimes to the development of bone in the cancellated portion of the fracture without the formation of any fibrous or cartilaginous deposit. At the same time, the vascular action in the lacerated edge of the external periosteum creates granulations on the edge of the compact matter of the bone, so that the granulations of the centre and of the circumference of the bone being gradually approximated, a bony convex cap is formed, which closes the surface of the bone whether as left by the saw in an amputation, or by nature in an ununited fracture; thus creating a round, solid extremity, devoid of external periosteum,

yet having this membrane extending as far as its circumference—that is, up to the original point of division of the fibres of the compact structure. Such, at least, is the condition which I have more than once seen upon a careful examination of the ends of the bone removed in an old and well-marked case of false joint. Of course, in this state of the parts, we cannot anticipate union from the capped extremities of the bone, though we may, as will be hereafter shown, obtain it from the periosteum on each side of the fractured ends. When, instead of the formation of granulations and osseous granules upon the fractured extremities of a bone, there is only the fibrous or cartilaginous cell, it will be found that the bone will be closed by the attachment of a fibrous or cartilaginous structure, which, as Mr. Paget suggests, is the ordinary state of the parts in fracture of the patella, olecranon and intra-capsular neck of the femur, and which, it is well known, is the condition seen in certain recent forms of false joint, the union being either fibrous or cartilaginous, as in the false joint of the radius, and some of the other bones. As the deposit of osseous granules and the formation of bone is hardly probable in this form of ununited fracture, except in very recent cases, or those which are more correctly instances of retarded union, we must here look to some other point than the ends of the bone for a bond of union, and again find it in the periosteum which surrounds the bone up to the seat of the fracture, this periosteum being capable, under proper excitement, of uniting the ends of a fracture.

To prove this, I now urge the employment of the following plan of treatment in ununited fracture, believing that when, in any false joint, whether recent or ancient, the two fragments are permitted to overlap moderately, whilst, at the same time, such action is excited by the pressure of one periosteal surface against the other, as may be obtained by gentle and long-continued motion in using the limb, a growth of callus will be often induced and accomplished with much less local and constitutional disturbance than it can possibly be by friction and rest, pressure and rest, the seton, resection, drilling, or the use of ivory pegs, as heretofore recommended; whilst the patient will escape confinement, free suppuration, phlebitis, hectic fever, and the serious effects which have often resulted from these plans of treatment.

That pressure and friction of the surface of the external periosteum will lead to the formation of bone, has long been known to surgeons, and exemplified in the formation of a new acetabulum in cases of luxation of the head of the femur upon the pubis, two specimens of which are now in the Wistar and Horner Pathological Museum in Philadelphia; and other instances from the head of the humerus are doubtless well known, as the result is not a rare one. If, then, a rounded, smooth bone like the head of a femur, can, by pressure, lead to the absorption of the edge of the iliacus internus and psoas magnus muscles, then of the periosteum on the ilium and pubis, until it exposes the compact matter of the surface of the pelvic bones, and then induce an osseous deposit around the free edge of the ulcerated or absorbed periosteum, why should the same results not follow the application of similar

forces when the two periosteal surfaces are in apposition near the seat of a fracture? It most certainly does so, and leads to the development of an amount of provisional or ensheathing, as well as of intermediate or definitive callus, fully equal to the support of the body.

How this pressure and motion, acting on the external periosteum, lead to the growth of new bone, or callus, is fully explained by the recent observations of histologists. From the remarks of Mr. Kolliker on the histology of the osseous system,¹ it appears that in the growth of bone, the elementary processes in the layers of the periosteum are as follows: The periosteum of the primary cartilaginous bones being proportionally very thick and vascular, consists, as early as the fifth month, of common connective tissue and fine elastic ligament. On the inner aspect of this fully formed periosteum there is deposited an ossific blastema (4), firmly adherent to the bone (see Fig. 1),

Fig. 1.

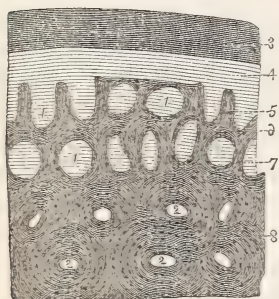


Fig. 1. Transverse section from the surface of the shaft of the metatarsus of the calf, magnified forty-five diameters. 1, 1. Rudiments of the Haversian canals of the compact substance. 2, 2. Area of the canals. 3. External periosteum. 4. Ossifying blastema. 5. Young layer of bone. 6. More developed layer of bone.—After Kolliker.

so that, when the periosteum is removed, it remains upon the bone as a moderately thick, soft, whitish-yellow lamella, in which the microscope shows fibrous tissue, and granular, oval, or round nucleated cells, measuring 0.006.001 of a line, from which, after a time, the bone cells are formed, out of the formative cells of the blastema (page 325), the soft bone lamellæ passing into the blastema. The development of the ossifying blastema Mr. Kolliker regards as due to the numerous vessels of the young periosteum, the origination of its fibres being from fusiform cells, as he has frequently observed it in man and animals. The formation of bone in this blastema occurs, he thinks, wherever it is in connection with bone.

Although these observations of Mr. Kolliker are chiefly limited to the action of the young periosteum, there is reason to suppose that the adult periosteum continues to act in the same manner, although not so rapid in its formation. If, then, pressure of the two sides of the fragments of an ununited fracture lead to an absorption of the outer layer of the periosteum, and thus expose the blastema or structure described by Mr. Kolliker, as between the compact structure and the inner face of the external periosteum, it is easy

¹ Manual of Microscopical Anatomy. Phil. edit. p. 324.

to understand how the periosteum may, under the stimulus of this pressure and motion, be enabled to create a sufficient amount of irregular bony deposit, or intermediate and provisional callus, to serve as the bond of union for the ends of a false joint.

Without, however, enlarging on these physiological points, it may suffice to say, that it is evident from the action of the periosteum in old luxations, as well as in certain diseases and injuries of the bones, that it is capable, under stimulus, of throwing out a mass of new bone, and that it is only necessary to excite it properly to lead to the formation of such an ensheathing structure as will restore the continuity of the bone, and consequently of the action of the muscles attached to it; thus reproducing the usefulness of the limb. Such means, I think, will be found in the use of the apparatus hereafter described, the formation of which is based on the following proposition. If a patient, after amputation of the leg or thigh, can sustain the weight of the body upon an artificial support, which is only a series of hollow cones, so adjusted as to play upon each other (at the joints), and thus facilitate progression, why may he not obtain the same support with his limb on, from some similar mechanical contrivance, made sufficiently light to be moved by wasted and weakened muscles? Surely, but little mechanical ingenuity is necessary to the development of such means of progression; and, in calling attention to those subsequently employed, I wish to lay no claim to originality. It is the surgeon's duty to suggest the principle for the construction of an instrument or apparatus, and the business of the cutler, or other mechanic, to develop it in the material most capable of furnishing the qualities required of the apparatus, and the credit of manufacturing a light and neat apparatus for the treatment of false joint, either for the upper or lower extremity, in accordance with my views and suggestions, is due to Mr. Rohrer, cutler, of North Sixth Street, Philadelphia. The application of the principle of pressure and motion is the result of considerable observation of the defects of the treatment of false joint by the ordinary plans, as well as of several dissections made by me of uncured cases.

The results of my plan of treatment are shown in the histories of the following cases, which I present as furnished to me, or as found in my notes of the patients as they were presented to me:—

CASE I.—*False Joint in both Bones of the Leg, of ten months' standing, cured whilst walking about, by the use of the Artificial Limb.*—Account furnished by Dr. WM. WATERS, of Frederickstown, Md.: "You request me to give my experience of an apparatus introduced by yourself in the treatment of pseudarthrosis.

"In the first place, permit me to tender you my acknowledgments for the cases reported by you;¹ as that principle has been completely successful in one case with me, of an aggravated character, and from report is acting very bene-

¹ American Journal of Medical Sciences, vol. xxi. N. S. p. 106, 1851.
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ficially in another, which has only been wearing for about a month the apparatus made by your ingenious cutlers, Messrs. Rohrer & Sons.

"Mr. S——, æt. 35, had received a compound comminuted fracture of the tibia and fibula, on the 4th of September, 1849. The accident occurred from the wheel of a tender to a locomotive passing over the right leg obliquely, about two and a half inches below the knee, comminuting somewhat the tibia and the fibula, and piercing the soft parts nearly opposite the injured tibia. The fracture of the tibia was obliquely inwards, about two and a half inches below the insertion of the ligamentum patellæ. In passing my finger into the wound (it could be introduced opposite the spine of the tibia and carried around the face to the under surface of the bone), some comminution of the fracture could be ascertained in the lower shaft, but there was very slight displacement of the ends, as one slab or piece, which could be felt depressed slightly beneath the lower shaft, was raised with my finger to its place. The foot was of the natural temperature five hours after the accident; the dorsalis pedis and posterior tibial arteries were very feebly felt; but his pulse was pretty good and firm, though somewhat enfeebled. Drs. Gerry and Strong, of Harper's Ferry, near where the accident occurred, yielding in an effort to save the limb, he was brought, by return train of cars, to this place, where his family resided. He had some fever for a week or more, and suppuration, and a slough about the size of a large pear occurred over the fractured shafts of bone, which resulted in necrosis, exfoliation of the surfaces, and caries of the ends of the bone until April, 1850. In February, the immovable apparatus was applied, and subsequently a carved splint fitting the outside of his leg was fastened by a roller on the entire limb. Injections of nitrate of silver to the tibia were also tried. About twelve months after the accident, a seton was passed between the shafts of the tibia, the fibula having united in the early stage of the case. The leg was splintered for months; the seton was withdrawn, or rather it cut itself out in four weeks, on the principle of Lourme's modification of Physick's plan; this also failed. During November and December, 1850, acupuncture was also used to excite inflammation in the part, the patient's health being good at this time, from previous generous diet and tonics. My patient now became anxious for excision of the fibro-cartilaginous ends. But the pseudarthrosis being near the knee-joint, the loss of bone from the block to be removed would have been at least from one and a half to two inches in width, and would have encroached upon the ligaments on the inside of the knee. I therefore prevailed upon him to try an apparatus like yours in the case reported in Vol. XV., New Series, of the *American Journal of Medical Sciences*, except that it extended some six or seven inches above the knee. As soon as this was applied, our patient, on the 26th of March, 1851 (about eighteen months after the accident), immediately, with a cane, walked across the floor with a pretty firm step. The mobility of the artificial limb at the knee and ankle had otherwise a twofold desirable purpose, as it permitted the exercise of the knee and ankle-joints, the latter having become considerably impaired in motion, from an injury he had sustained about a year previously. In from four to six months, the tibia became sufficiently strong to walk without the apparatus, and his ankle also improved. The fractured bone firmly united, and in six months after wearing it, he resumed his situation, in the employ of the Baltimore and Ohio Railroad Company. I have not seen him for some time; but am credibly informed by his family, that he has had no occasion to wear the supporter for upwards of a year. Indeed, he might have dispensed with it earlier, but I advised him to wear it as a precautionary measure.

"I feel every confidence in the success of the other case under treatment,

who is a respectable physician of our county. I saw a brother of his to-day (April 28, 1853), who informs me that the leg is becoming more stiff, although the doctor has only been wearing the artificial limb about a month. This fracture was oblique in the lower third of both bones; the fibula readily united, but the tibia did not. The case is only of five months' standing."

CASE II.—*Pseudarthrosis of the Bones of the Leg, of five months' standing, cured by the Use of the Artificial Limb in nineteen weeks.*—Reported by Dr. WM. WATERS, of Maryland. "On the 28th of November, 1852, Dr. J. W. J. fractured his left tibia and fibula, about the lower third of the leg; the fracture was simple, but oblique. His leg was placed in Roe's apparatus, by Dr. R. H. Thompson, of Urbana, and in a few days was changed to another; but, in three or four days, the shafts of the tibia were thrown out of position, and the spine of the tibia was rather more than half an inch out of line. He suffered considerably from the irritation of the fracture. I saw him with Dr. T., the attending physician, ten weeks subsequent to the fracture, and found the provisional callus only of a ligamentous character, and very soft, so much so that we endeavoured to reduce the shafts of the tibia to a rectilineal position, but failed, in consequence of the ligamentous union. We therefore proposed breaking up the slight union, and resetting the limb. This was not consented to by Dr. J. The knee of this leg had been ankylosed for many years, as it was injured during his boyhood, and had very properly been fixed at a slight angle to facilitate his walking with that limb. We now applied to the ununited fracture, the immovable apparatus of Suetin, and also advised tonics and exercise in his carriage, or upon crutches, with generous diet, as his general health had suffered from confinement to bed. About the twelfth week, the limb was examined, when but very slight, if any, improvement was noted, and the bone is still flexible. In consequence of erythema, and occasionally neuralgia in the limb, the apparatus had to be removed, and his sufferings were considerable for a simple fracture. This may be ascribable to two causes; first, the overlapping of the bones to some extent; and, secondly, his nervous temperament, he having suffered from dyspepsia previously; and, indeed, his great toe had some gouty thickening around the first joint. On the 6th of April, 1853, upwards of four months, we exchanged the apparatus of Suetin for that employed in the previous case. After wearing Dr. Smith's apparatus about five months, the tibia became firm, and since then our patient has been walking about with only a cane."

February 20, 1854.

CASE III.—*Ununited Fracture of the Femur, of five months' duration, cured in seventeen weeks by the Artificial Limb, so that he could walk with only a cane.*—Jno. K——, aged 40 years, was admitted into St. Joseph's Hospital, in the fall of 1852, under the care of the late Dr. WM. E. HORNER, labouring under a recent fracture of the lower third of the left femur. After being treated by Dr. Horner, with Dessault's long splint slightly modified, the bone was discovered to be ununited, the amount of callus being very small and permitting free motion at the seat of fracture. Friction of the ends of the bone being freely practised, blisters were applied around the limb at the seat of fracture, and the patient took cod-liver oil freely, with full diet and porter; when, after a few days, the splints were again applied and the limb kept at rest for four weeks. A second examination yet showing no improvement, the limb was carefully bandaged from the toes to the groin, and splints of binders' board applied around the thigh, when he was placed on crutches and directed to walk about as much as possible. When I took charge of the wards, a short

time after this, I found him unable to move the limb or bear his weight upon it, but continued the treatment of Dr. Horner for some time longer, whilst the artificial limb was being made. On the 17th of April, the dressing was removed and the motion at the fracture found to be free in nearly all directions, the limb being shortened nearly three inches, except when extension was practised. The new apparatus being applied, he was at once placed on his feet with crutches, and walked across the floor, putting his foot to the ground and bearing some weight upon it. The apparatus requiring alteration, he was kept in bed for five days, at the end of which time he again applied his artificial limb and walked about the grounds freely, spending much time in the open air as warm weather came on. When the apparatus was removed, after six weeks' trial, there was an evident firmness in the fracture, and on October 3, 1853, seventeen weeks after its second application, the bone was so firm that he was permitted to walk without the splint, and in a few days walked freely with only a cane, and left the hospital. In December, he left the artificial limb at my office, being well able to walk with a high-heeled shoe, the shortening measuring by accurate measurement two inches.

CASE IV.—Ununited Fracture of the Femur of six months' standing; Suppuration; Great exhaustion; Hectic; Cured in nine weeks by the use of the Artificial Limb.—The history of this case is given as furnished me in the following letter, dated May 22, 1854, from Dr. G. Dock, of Harrisburg, Pa.:—

"The bearer of this letter, Mr. Neelan, is desirous of having his brother placed in St. Joseph's Hospital, for the treatment of an ununited fracture of the femur. As a short account of his case will be of interest to you, I will give it to you, as far as I can. He is twenty-eight years of age. It appears his thigh was fractured by the falling of a bank of earth on him, whilst excavating a cut on the railroad. The accident occurred last fall. He was placed under the charge of a physician of this place; but, after he had been under his treatment for *three months*, the physician was discharged, and I was called in to attend him.

"I found him in the following condition: first, his general health much impaired; hectic fever; foul tongue and breath; pulse 115 to 120; irritable; slept but little; no appetite; and a torpid, depraved condition of the excretory system. The leg was in a long poplar box, in a state of extension; the bandaging from the foot up to the thigh very tight, and he suffering much pain. I next took a survey of the limb (thigh), and at once saw it was crooked and much too short. I then applied a tape and measured the injured limb from the anterior superior spinous process of the ilium to the centre of the patella; then measuring the sound one, I found the fractured limb nearly three inches short. I then removed a mass of sheet-iron splints, about a peck of bandages, &c. &c., from the thigh, and found the bones, as I anticipated, the upper fragment drawn up by the muscles of the pelvis, the upper end of the lower fragment being drawn backwards by the strong flexor muscles of the thigh, thus causing it to pass up under the upper fragment to the extent I have mentioned. I took everything off, placed a nice Scultet's bandage on a padded *double inclined plane*, and flexed the thigh and leg. I then coaptated the bones (which I found ununited), reducing the limb to its proper length very easily, as the muscles were so soft and debilitated as to offer but little resistance. I then adjusted the bandage and placed splints on each side with an extending point and a buckskin perineal counter-extending band, all working pretty well for some days.

"As I then discovered a spot of *blood* oozing through the bandage, under the calf of the *leg*, I removed the bandage and found a gangrenous ulcer

about as large as a half dollar, and three or four lines deep at this point, the foot and leg being purple and with a low degree of vitality, several spots existing on top of the foot and ankle, and threatening to slough soon. I was therefore obliged to loosen everything about the thigh that could obstruct the free circulation, and favoured the suppuration of the leg by gentle dressings and warm fomentations.

"The next morning, the foot and leg were enormously swollen and œdematous, and sloughs were open on the upper and lower sides of the ankle and top of the foot. To these I applied warm poultices of flaxseed meal and yeast, gave him a grain of opium at night and wine-whey through the day, with an occasional dose of solution of quinine, and a little beef tea. But it is unnecessary to detail my treatment. I succeeded in saving his leg and improving very much his general health, and as his thigh was a secondary matter then, and, of course, no deposit of plastic or osseous matter expected under the circumstances, I just retained the limb in as favourable a *position* as possible, and could apply no retentive means on account of the enfeebled condition of his system, his skin bearing scarcely the pressure of the weight of the limb itself, much less any firm splinting, etc. It is now about eight weeks since I first saw him. Any attentions he may receive from your hands in your official capacity will be gratefully appreciated by his friends, and be of no little satisfaction to me."

The following history was furnished by Dr. Hoyt, the Resident Surgeon of St. Joseph's Hospital, after his admission:—

"Michael Neelan, aged 29, was admitted into the hospital on the 23d of May, with an ununited fracture of the femur. He was at the time in an exceedingly debilitated condition; indeed, was almost in the last stages of hectic. He had little appetite, was extremely emaciated, and had decided night-sweats. His pulse was feeble, and ranged considerably above a hundred. His tongue was clean but pale.

"On examining his leg, I found a small ulcer on the under side of the knee, which discharged healthy pus. The leg and foot were enormously swollen; the ankle was nearly ankylosed, and the toes possessed little motion. The muscles of the thigh were excessively attenuated. The bone was fractured at about the junction of the upper with the middle third. The upper fragment overrode the lower, and at the same time the two fragments formed an angle outwards of about thirty degrees. The limb was shortened three inches. There was a complete false joint at the seat of fracture. Moving the limb in any direction gave little pain unless carried to a considerable extent. He was put on good diet, with the use of tonics. His leg was bandaged every day with a flannel bandage; at the same the joints were freely moved.

"On the 28th of May, as the leg had been much reduced in size, the bandage was discontinued, and Dessault's apparatus for fracture of the femur was applied in order to maintain extension until the appropriate apparatus should be prepared. Dessault's apparatus was continued until the 13th of June, when it was removed and the flannel bandage reapplied. The foot had become considerably swollen during its discontinuance.

"The suitable apparatus (artificial limb) having been prepared, was applied for the first time on the 19th of June. He was able to walk with a little assistance on the same day. Though still weak and emaciated, his health and appearance have much improved. From the time he commenced walking his improvement was rapid. He acquired new strength and vigour. A mass of callus was gradually formed, uniting the two fragments, until, on the 9th of August, it had acquired sufficient hardness to render it safe for him to walk with the assistance of crutches without his apparatus, which had been sent to

undergo alterations. During the whole of this period, he gradually improved in the facility with which he walked. He at first required two crutches, and even then walked with difficulty; but as he grew stronger, a cane was substituted for one crutch, and then for another. The broken limb measured, on the 19th of August, two inches and a half less than the other. We may certainly hope that, should he continue to improve, he will eventually have a highly useful natural leg in the place of the artificial one, which I firmly believe he would have required but for this valuable apparatus."

This patient soon afterwards recovered the use of his limb to such an extent that I returned him to the charge of Dr. Dock, at Harrisburg.

CASE V.—False Joint in the Femur, of twenty weeks' standing, cured in six weeks by the Use of the Artificial Limb.—Report furnished by Dr. R. J. Levis, of Philadelphia. "The case in which I advantageously applied the apparatus, proposed by yourself, for the relief of pseudarthrosis, was that of an ununited fracture in the upper third of the thigh-bone, occurring in a heavy muscular man aged about fifty-five years, of general healthy appearance, but of rather irregular habits. The fracture had been treated with the long, straight splint; the apposition of the fragments seemed quite correct, and with no apparent shortening; but yet, at the end of about four months, there had evidently no bony union taken place, and there was an unimpeded mobility of the limb in all directions at the seat of fracture. No favourable change, for a month after, taking place in the condition of the parts, and the patient suffering much and becoming intolerant of the confinement, it was deemed advisable to make use of mechanical support, and, if possible, to place him on his feet. This indication was answered admirably by the appliance constructed by Mr. Rohrer, similar to others which had been previously formed after your direction by him, and fitted to the limb. The patient was at once placed in a walking attitude, and continued for some time moving about with the assistance of a crutch or cane. At this time the fragments had taken a permanent position, overlapping each other, and projecting somewhat outwardly. The limb continued to acquire more firmness, until, after five or six weeks, he was able to dispense with the support of the apparatus entirely, and has now a somewhat deformed and slightly shortened, yet substantially useful limb."

CASE VI.—False Joint in the Humerus, of six months' standing, relieved, and permitting the use of the Arm in "type-setting."—From Dr. George Dock, of Harrisburg, I have recently received the following notes of a case of false joint in the humerus:—

"A man, about fifty years of age, has been recently presented to me for treatment for a false joint in the left humerus, just above the insertion of the pectoralis major. The history of the case showing that the dressing of the fracture had been very imperfect, I was not surprised at finding a false joint of its character, and doubt whether anything short of a seton or Dieffenbach's pegs will bring about union; but if you can devise an apparatus that will suit him, be kind enough to write to me." The cost of the limb, \$15, being beyond the patient's means, Dr. Dock writes as follows, October 24, 1854: "My man with ununited fracture of the humerus was unable to purchase an apparatus. I consequently made one for him myself, and he is now at his daily work (type-setting), and bids fair to recover with an united bone."

CASE VII.—False Joint in the Humerus.—Treated by Dr. Ashby, of Alexandria. Another case of false joint in the humerus, of long standing,

has been for some time under treatment by my friend, Dr. C. W. Ashby, of Alexandria, by means of an apparatus forwarded in accordance with the measurements sent. Although a combination of difficulties has retarded the treatment, yet Dr. Ashby in a very recent note says: "K—— wore the splint nearly three months, and although it was rather too short, it afforded him so much comfort and support, that at one time I anticipated a favourable result." This patient was subsequently started by Dr. Ashby for Philadelphia, to be treated by myself, but for some unexplained reason never got beyond Baltimore.

Although these cases in the humerus cannot be cited as cures, they are sufficient to show, as Dr. Ashby has remarked, "the correctness of the principle," and may, I hope, induce others to try the same means (see Figs. 2, 3) when a false joint is presented in the humerus.

In order to present a summary of the cases already treated by my method, I add the following recapitulation of those published in this and a preceding paper in this journal. (See vol. xxi. N. S. 1851, p. 108):—

Case 1. Under the care of Dr. Wm. Waters, of Fredericktown, Md. False joint in both bones of the leg, of eighteen months' standing, cured in five months.

Case 2. False joint of both bones of the leg, of four months' standing, cured in nineteen weeks, by Dr. Waters, of Maryland.

Case 3. False joint in the femur, of five months' standing, cured in seventeen weeks, under my own care.

Case 4. False joint in the femur, of six months' standing, cured in nine weeks, under my own care.

Case 5. False joint in the femur, of twenty weeks' standing, cured in six weeks, under the care of Dr. R. J. Levis, of Philadelphia.

Case 6. False joint in the humerus, of six months' standing, under treatment, and at present relieved, by Dr. G. Dock, of Harrisburg.

Case 7. False joint in the humerus, under the care of Dr. C. W. Ashby, of Alexandria.

In addition to these, I had previously noted a case of false joint in the femur of a man of upwards of seventy years of age, which failed to unite, but which was made so firm by the apparatus that the patient walked only with the assistance of a cane. One case of false joint in the leg failed, but though the bones did not unite, the patient is yet able to walk; and I have now from Mr. Rohrer, the cutler, reference to two other cases in the leg, cured by surgeons at a distance, who have failed to respond to my communication respecting them. We have, therefore, a total as follows:—

	Cases.	Cured.	Relieved.	Failed, but able to walk.
False joint in the femur	4	3		1
“ “ “ leg bones	8	7		1
“ “ “ humerus	2		2	
Total	14	10	2	2

The apparatus employed in these cases is represented in the Figures 2, 3, 4, 5, 6.

Fig. 2.

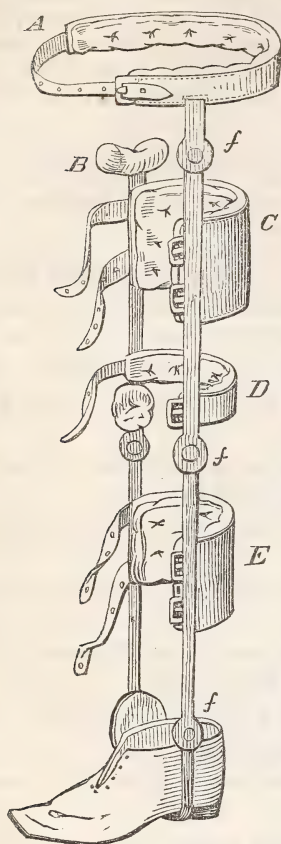


Fig. 4.

Fig. 3.

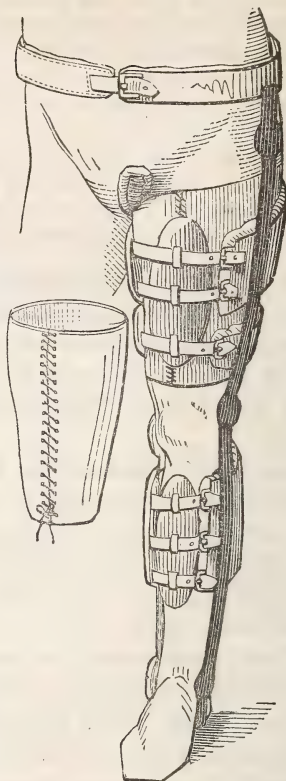


Fig. 2. A three-quarter view of the artificial limb applied in the treatment of false joint in the femur, and worn in the cases detailed above. *A*. A band and belt to surround the pelvis. *B*. A padded crutch-head to support the weight of the body upon the perineum. If the portions of the apparatus are made so as to form a cone, the apex of which is towards the knee, this head may be dispensed with, as the support on the thigh will act as it does in the artificial limb worn after amputation. *C*. The back and upper half of the cone or splint which supports the thigh. The front half buckles around the limb, is formed of sheet-iron, well padded, and, by forcing the limb against the rest of the splint, gives firmness to the fracture, and acts as a ferule. *D*. Another portion of the thigh support, which is sometimes continuous with that at *C*. *E*. A conical piece for supporting the leg. The front splint is here also to be buckled to the limb, and keeps the tibia directly in the line of the femur. *f, f, f*. Joints in the side-pieces of steel, to favour the motion at the ankle, knee, and hip, thus obviating the ankylosis so apt to result from long confinement and rest in one position. As there is always some shortening of the femur (say two inches), a false heel is placed *inside* the shoe. When the limb has united, the patient's shoe, if thus filled, will enable him to walk without attracting attention.

Fig. 3. A buckskin piece to surround the thigh, and prevent chafing of the skin.

Fig. 4. A three-quarter view of the artificial limb as applied. McNeelan walked with this apparatus whilst there was no union at the seat of fracture. In measuring a patient for this or any other of these artificial limbs, moderate extension and counter-extension is necessary in order to prevent subsequent shortening. When the artificial limb is well fitted, subsequent shortening is prevented, the chief support being above the seat of fracture, and the resistance below it.

Fig. 5.

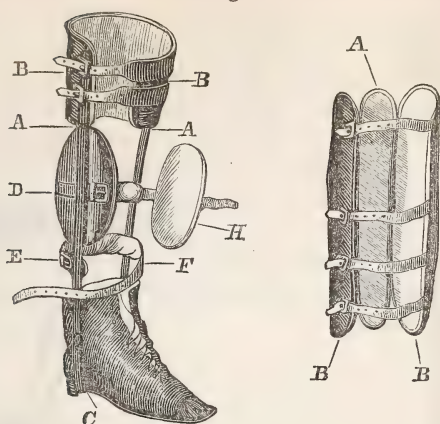


Fig. 5. A three-quarter view of the artificial limb employed in false joint of the bones of the leg. *A, A.* Steel splints applied at the side of the leg and connected under and below the foot at *C*, so as to sustain the weight of the body. *B, B.* A conical-shaped support attached to the side splints and made to embrace the head of the tibia, thus sustaining the weight of the body as in the artificial limb worn after amputation of the leg. *D, E, F, H.* The support furnished at the seat of fracture, and acting like a ferule upon the broken bones. This support steadies the fragments, and generally leads to the development of sufficient definitive and ensheathing callus to unite them. This shoe does not require a false heel, as the support is furnished by the ends of the side splints at *C*. Very little shortening has supervened on the use of this apparatus in false joint of the bones of the leg.

Fig. 6.

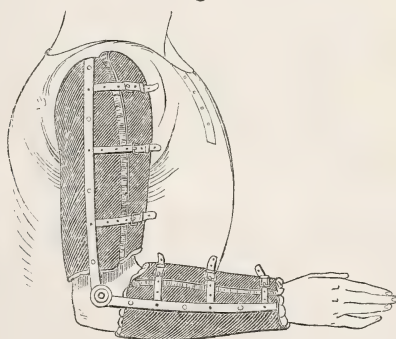


Fig. 6. A side view of the artificial limb applied to false joint in the humerus. In measuring the limb for this apparatus the humerus should be drawn as nearly as possible to its original length, and the artificial limb should rise well over the shoulder and fasten by straps to the opposite axilla if the false joint, has followed a fracture of the surgical neck of the bone.

Having thus shown the means of treatment and the results in the cases just related, it remains to exhibit the specimens which illustrate the method in which nature has accomplished the cure; and in the following drawings, taken from preparations in my possession, I think this will be readily seen to be due entirely to an "intermediate callus" produced at the expense of the external and internal periosteums, as exhibited in Figs. 7, 8.

Fig. 7.



Fig. 8.

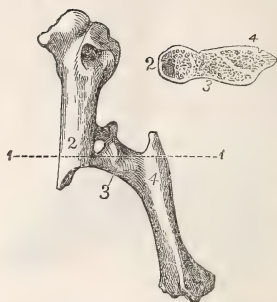


Fig. 7. A front view of the mode of union accomplished in an old fracture of the femur. The callus formed between the fragments appears to be chiefly due to the action of the outer surface of the bone, or rather to the inner surface of the external periosteum after its outer layer has been absorbed.

Fig. 8. A front view of the union obtained in a comminuted fracture of the humerus of a turkey, showing the fusion of compact with the cancellated structure of bone. 1, 1. Line of the saw in making a section. 2. Upper fragment of fracture. 3. Intermediate fragment. 4. Lower fragment. An end view of the section of the upper half of the bone is seen at the side of the drawing, and shows the perfect fusion of the cancellated structure of the intermediate fragment 3, with the compact structure of the fragments 2, 4. This union has evidently resulted from the absorption of the outer layer of the periosteum, without which it could not have taken place.

With the results thus given, I hope that others will be induced to repeat the treatment just advised, and give the profession the results of their experience; and that the more severe modes of treatment heretofore advised will be avoided until after the failure of the application of the artificial limb for six months. As the patient will have considerable use of the member during this period, the duration of the treatment and its inconvenience (if any) will hardly be complained of, whilst if it fails to accomplish the union of the ends of the bones, the patient will yet have a very good and useful member.

The hopeless condition of the limb, after the trial of some of the means heretofore employed, has more than once been illustrated, both in the United States and in Europe, by the willingness of both patient and surgeon to consent to amputation. Very recently¹ we notice the fact of an amputation of a limb for false joint, performed by Mr. Stanley, of London, whose work on *The*

¹ Med. News, Oct. 1854, from Lond. Times, July, 1854.

Diseases of the Bones is so well known. In this case, the use of ivory pegs, as advised by Dieffenbach, and even wiring of the fragments, had failed to induce a cure. A case is also reported where Mr. Lawrence, after employing the immovable apparatus without success, has resorted to the seton, the case being under treatment at the last account; and amputation has more than once been performed in the Pennsylvania Hospital, after the failure of the seton in the femur. When a comparison is made between the use of a seton with confinement to bed for six months, or between the other means requiring confinement, and this, which permits free exercise of the limb in the fresh air, few will, it is thought, fail to give the preference to the treatment by *pressure and motion*, as accomplished through the artificial limbs.

PHILADELPHIA, October, 1854.

ART. VII.—*The Relations existing between Urea and Uric Acid.* By WILLIAM A. HAMMOND, M. D., Assistant Surgeon U. S. Army.

THE origin of urea is still somewhat undetermined, though numerous observations and experiments have been made by physiologists with the view of ascertaining the source or sources whence it is derived. That it is furnished, in part at least, from the metamorphosis of the effete nitrogenous tissues of the body is beyond doubt, and is admitted by all; but that it is also derived from the oxidation in the blood of the albuminates taken as food, without their previous conversion into tissue, is not so generally received.

Liebig, Bischoff, and others, hold that urea can have no other source than the metamorphosis of the worn-out tissues, and entirely reject the theory which ascribes its origin in part to the oxidation of the albuminates of the blood; whilst this latter view is supported by Lehmann, Frerichs, and Bidder and Schmidt, who adduce many experiments in confirmation of their opinion.

I do not intend, however, at this time, to discuss the question of the origin of urea. My object now is, to give the results of some experiments undertaken with the view of ascertaining whether the theory of Liebig, in regard to its formation from an intermediate substance (uric acid), could or could not be sustained by further investigation.

This hypothesis of Liebig, and the facts and inferences hitherto relied upon to sustain it, may be stated generally in few words. He assumes, as the groundwork of his theory, that uric acid is one of the primary products of the metamorphosis of those tissues, which, being worn out, are unable to resist the chemical action of oxygen.

The following formula shows how this conversion may be effected from the oxidation of protein, a substance entering essentially into the composition of all fibrous tissues:—

	C.	N.	H.	O.		C.	N.	H.	O.	
1 atom protein	48	6	36	14	}	15	6	6	9	1½ atom uric acid.
91 atoms oxygen				91		33			66	33 atoms carbonic acid.
								30	30	30 " water.
	48	6	36	105		48	6	36	105	

The uric acid thus formed is, according to Liebig, resolved, by further oxidation, into urea and carbonic acid, as follows:—

	C.	N.	H.	O.		C.	N.	H.	O.	
1 atom uric acid	10	4	4	6	}	4	4	8	4	2 atoms urea.
4 atoms water			4	4		6			12	6 " carbonic acid.
6 " oxygen				6						
	10	4	8	16		10	4	8	16	

According to this hypothesis, urea should be the predominant principle in the urine of all animals which lead active lives, and consequently inspire a large proportion of oxygen, more than is required for the formation of uric acid; and this latter substance, being first generated, should be converted by the surplus oxygen into urea and carbonic acid, and consequently should not be found in the urine of these animals, or, if present, should be but in small quantity.

On the other hand, in animals of sluggish habits, breathing but feebly, and consequently taking in but little oxygen, we should not expect to find urea, for uric acid would probably be the highest degree of oxidation the effete tissues or albuminates of the blood would be capable of assuming.

It also follows, from this theory, that the character of the food must exercise a very important influence in determining the formation of urea from uric acid. A non-nitrogenized diet, from containing a large proportion of carbon, should abstract oxygen in such quantity as to prevent the conversion of all the uric acid into urea, and consequently the former substance should exist in excess; whilst, from a highly-nitrogenized ingesta, the conversion ought to proceed under more favourable circumstances, and urea be found in greater abundance than would otherwise be the case.

Now, in fact, we find that these conditions do exist to a considerable extent. The urine of lions, tigers, dogs, &c., animals of active habits, inspiring a large quantity of oxygen, and eating highly-nitrogenized food, contains scarcely an appreciable amount of uric acid, though urea is found in abundance; and the urine of serpents, whose slothful disposition and sluggish respiration and circulation are well known, consists of little else than compounds of uric acid, no urea being present.

There are several other facts on record which tend to support the view of Liebig. Thus, Wöhler injected urate of potash into the veins of rabbits, and uniformly found an increase in the amount of urea eliminated by the kidneys. Dr. Frick has noticed that, in the convicts of the Maryland Penitentiary, uric acid was in excess in those who were allowed but little exercise, and that in

the same persons the quantity of urea was proportionably diminished. In those, on the contrary, who, from the nature of their occupations, underwent more muscular exertion, he found a larger amount of urea, and a corresponding diminution of uric acid. He also found that in those to whom cod-liver oil (a substance containing a large amount of carbon) was administered, the quantity of urea was diminished in proportion to the amount of oil taken. (*On Renal Affections*, p. 113.) Dr. Frick does not state, however, whether under this last condition the amount of uric acid was augmented or diminished.

In opposition to the hypothesis of Liebig, the experiments of Lehmann are to be taken into consideration. This eminent physiologist found, from observations made upon himself, that the uric acid was in excess at the same time with urea, and *vice versa*. Thus, whilst living on an animal diet, the quantity of urea daily excreted was 821.37 grains, and of uric acid 22.82 grains. On a non-nitrogenized diet, the urea amounted to 237.90 grains, and the uric acid to 11.34 grains; these substances bearing a more opposite relation to each other than should obtain, provided Liebig's theory be correct.

The fact that birds excrete no urea—their urine, like that of serpents, consisting of compounds of uric acid, whilst in their habits and physiology such a wide difference otherwise exists—is also opposed to Liebig's theory, and, in the present state of our knowledge, is absolutely irreconcilable with it.

Without, therefore, pretending to decide a point upon which there is so much conflicting testimony, but with the view of contributing towards its elucidation, I instituted the following experiments, the first series being performed upon myself:—

1. The object of the first experiment was to ascertain the normal quantities of urea and uric acid excreted by the kidneys in twenty-four hours, an average amount of food and exercise being taken.

I reckoned from 7 o'clock A. M., having first passed the urine which had accumulated in the night. During the day, I ate sixteen ounces of animal food, and twenty ounces of vegetable food, consisting of bread, potatoes, and turnips; drank twenty-two ounces of water, and eight ounces of strong coffee; walked two and a half miles, and rode about three miles on horseback; took no other exercise. The mean temperature for the day was 74° Fahrenheit. I passed thirty-one ounces and two drachms of urine, of specific gravity 1.021. The total amount of urea, as determined by Liebig's method with the nitrate of mercury, from 1,000 grains of the whole quantity of urine, was 682.09 grains. The total amount of uric acid, as determined from 1,000 grains of the whole quantity of urine, was 13.72 grains.

These, then, may be regarded as the average normal quantities of these substances excreted by the kidneys in my own person, and as standards for the ensuing experiments.

2. The object of the second experiment was to determine the influence of exercise on the secretion of urea and uric acid, the diet being as in the first experiment.

As previously, commenced at 7 o'clock A.M. During the day, ate and drank the same articles and quantities of food as in the first experiment. Walked briskly eight and a half miles, over a hilly country; rode ten miles on horseback; pitched quoits for two and a half hours; slept six hours. Mean temperature for the day 76°. Passed thirty-four ounces and one drachm of urine, of specific gravity 1.024. Quantity of urea (determined as in the first experiment) 864.97 grains; uric acid 8.21 grains; showing an increase in the amount of urea of 182.88 grains, and a diminution in the uric acid of 5.51 grains.

3. To determine the quantities of urea and uric acid excreted, the food and drink being the same as in the other experiments, but the body kept as nearly as possible at complete rest.

Commenced at 7 o'clock A.M. Immediately laid down on a sofa, and remained there continuously the ensuing twenty-four hours, with the exception of rising four times to urinate. Food and drink the same in every respect as before. Mean temperature for the day 73°. Slept nine and a half hours. Total amount of urine, twenty-four ounces and seven drachms. Specific gravity 1.018. Quantity of urea 487 grains; uric acid 24.86. A diminution of urea from the normal standard of 125.09 grains, and an increase in the quantity of uric acid of 11.14 grains.

These results may be embodied in tabular form as follows, the diet being the same in each experiment:—

	Quantity of urine.	Specific gravity.	Quantity of urea.	Quantity of uric acid.
Moderate exercise,	31 oz. 2 drms.	1.021	682.09 grs.	13.72 grs.
Increased exercise,	34 " 1 drm.	1.024	864.97 "	8.21 "
No exercise,	24 " 7 drms.	1.018	487 "	24.86 "

From the above, it is, I think, conclusively shown that exercise, while it increases the amount of urea, diminishes very materially the quantity of uric acid; and that inactivity, though it diminishes the amount of urea, exercises a contrary effect upon the uric acid. As far, then, as experiments of this nature can do so, these decidedly tend to support Liebig's doctrine. It must, however, still be admitted that there are great difficulties to be overcome before this view can be regarded as actually proved.

In further reflecting upon this subject, it occurred to me that if I could, by providing a greater supply of oxygen than ordinary, induce the formation of urea in animals in which it does not naturally exist, it would almost demonstrate conclusively that the hypothesis advanced by Liebig is correct.

I therefore procured a young blacksnake (*Coluber Constrictor*), and confined it in a large jar. It was fed, *ad libitum*, on flies, grasshoppers, and other insects. At the end of a week, I examined the solid excrement which had collected in the bottom of the jar. As I expected, it contained no urea; but, on dissolving it in warm water, and adding a few drops of hydrochloric acid

to the solution, a large quantity of uric acid was in a short time precipitated. I next fitted to the jar an air-tight stopper, through which passed a glass tube. Through this tube, I introduced, three times a day, for a week, about two hundred cubic inches of oxygen, which was retained in the jar for two hours at a time. By its influence, the snake was rendered excessively lively; his eyes sparkled, and he darted from side to side with surprising agility. This state of activity continued during the whole time the jar remained closed. When atmospheric air was admitted, he soon relapsed into his usual sluggish state. During these experiments, his food was the same as previously mentioned, and was devoured with increased voracity.

At the end of a week, I removed the excrement from the jar, and dissolved it in warm water. A drop of the filtered solution was suffered to evaporate on a glass slide. On viewing it under the microscope, I observed most beautiful crystals of urea, mingled with those of amorphous urate of ammonia. Not being willing to rely upon the crystalline form alone as a test, I submitted the remainder of the solution to chemical examination, as follows. The process employed was that recently proposed by Liebig as a modification of his method of testing for urea with the nitrate of mercury. Solutions of corrosive sublimate and bicarbonate of potash were prepared and mixed together. On adding this mixture to the solution containing the excrement of the snake, the white precipitate of urea and protoxide of mercury were immediately thrown down. This examination I regarded as showing conclusively the presence of urea, and that in no inconsiderable proportion.

From the several experiments recorded in this paper, and more especially from the last series, I consider that the theory of Liebig, accounting for the formation of urea, is probably correct. True, there are many facts difficult to account for on the supposition of its truth, and many well-founded experiments to set aside, but surely the evidence is not altogether opposed to it; and, though much still remains to be accounted for before it can be universally received, there is ground to expect that future investigations will do much towards its establishment.

It is my intention to pursue this subject further, by submitting other animals to experiment. It is only by continued investigation that correct ideas of so important and difficult a subject can be obtained.

ART. VIII.—*Amputation of the Thigh in Civil and Military Practice; The Comparative Success of Secondary Operations.* By RICHARD McSHERRY, M. D., of Baltimore.

THE singular fatality which attends upon amputation of the thigh after gunshot wounds, is enough to restrain the hands of the surgeon wherever his patient

is not subject to removal, for experience shows that, as a general rule, amputation in such cases adds nothing to, if it does not diminish, the chances for life. Such, at least, was my own experience, acquired during the campaign in Mexico, under General Scott; such must also have been the general experience of the medical staff of our army, for, so far as I could learn, there were no recoveries after this operation. Whether there may have been one or more exceptions, I am not prepared to say; but the rule is, unquestionably, that amputations of the thigh were followed by fatal results.

This mortality is by no means peculiar to our army or to the climate of Mexico. M. Ribes examined four thousand invalided soldiers, among all of whom he did not find one single instance of injury of the femur by shot, nor one who had undergone amputation of the thigh. They had all been lost. And so Malgaigne tells us that he lost all of his cases of amputation of the thigh after gunshot wounds during the Polish campaign.

These being familiar facts to me, I was surprised and gratified to find the statistical table of Dr. Norris, published in the July number of this Journal, giving results so widely different from those of military surgeons. The patients had, it is true, the best attentions of one of the best hospitals in this country, but that does not fully account for the success exhibited, which is as follows, viz:—

Sixteen amputations of the thigh, resulting in *two* deaths and *fourteen* recoveries. This was during a period of ten years.

Now, in the Paris hospitals there were, in ten years, according to a table given by Malgaigne (from 1836 to 1846), forty-four amputations of the thigh, resulting in *thirty-four* deaths and *ten* recoveries. We may readily admit that these patients in the French hospitals had all proper attentions, as well as the patients in the Pennsylvania Hospital.

The prominent causes of differences of result must be sought in the causes of operation. All of the Parisian cases just cited were amputations after traumatic lesions; and so were all in the various armies. The *two* fatal cases at the Pennsylvania Hospital were after fracture of the thigh and compound and comminuted fracture of the leg, respectively. Of the *fourteen* remaining and successful cases, *seven* were fractures (*one* ununited fracture of thigh), *one* tumour on the knee, and *six* caries of knee-joint.

In this brief comparison between the results of civil and military practice, we find a strong illustration of the fact that patients who undergo great operations, in full robust health, are more liable to perish than those who are subjected to them for chronic disease. Now, this fact has been used by surgeons of deserved eminence as a plea for secondary amputations, or perhaps it were better to say as a plea for attempting to save limb and life, holding secondary amputations in reserve as a last resort. But modern surgery is disposed to reprobate the plea; it is now an axiom that, where the femur is broken by a shot, the limb must be removed as early as possible. Is it correct? I do not believe it. If a surgeon, in the exercise of his judgment, allows a patient to

retain his broken limb for the time being, hoping to save it, and ultimately the patient dies, with or without secondary amputation, it is very commonly said that he *might* have lived, or he *would* have lived, if primary amputation had been performed. But where is the proof? To reply that, for well-known reasons, primary are more successful than secondary amputations, is merely begging the question. If the patient has had the ordinary advantages of position, attention, and rest, it can be nothing more than conjecture to say that his life was sacrificed by deferring operation. There is, there can be, no proof that such is the fact. But there is abundant proof that delay brings many advantages. Dupuytren, in 1830, treated *thirteen* cases of fracture of the thigh without amputation; of these, *six* recovered and *eight* died. Malgaigne reports *five* cases treated by himself without amputation, of whom *two* recovered and *three* died. These results may not appear very flattering; yet it must be remembered that in the same hospitals, and at the same time, out of *forty-four* amputations of the thigh, *ten* recovered and *thirty-four* died. Thus, there the chances of life were greatest where the operation was not only deferred, but foregone altogether. With armies in motion, primary amputation is almost uniformly practised, with what results we have seen—all die. It must not be supposed that this is owing solely to transportation, for very commonly the patients are placed in military hospitals, which, measured by the terms of their lives, are permanent.

Dr. Norris's statistics show favourably for secondary amputations, inasmuch as there are *four* cases recorded where the operations, all after traumatic lesions, were performed at periods varying from ten to two hundred and twenty-five days after date of accident (or admission), while the *two* fatal cases were both subjected to primary amputation. *Seven* of the remaining cases, it will be remembered, were operated on for chronic disease; and, finally, *three* cases were cured after immediate amputation.

It is somewhat remarkable that, at the Pennsylvania Hospital, amputations of the leg resulted so much more unfavourably than of the thigh; thus, out of *forty-three* cases, *twenty-two* were cured (both legs removed in one instance), and *twenty-one* died (both legs having been removed also in one of these instances); but here it happens that, except *five*, all of the amputations were consequent upon severe injuries. So far as primary and secondary operations are concerned among these, of the *twenty-two* cured, *twelve* were primary and *ten* secondary; and out of the *twenty-one* deaths, *thirteen* followed the primary and *eight* the secondary operation.

I offer this brief summary of facts to show that the rule, now generally considered so absolute, that the thigh must be amputated immediately upon fracture of the femur by shot, is a bad rule, inasmuch as it requires certain destruction of the limb, without adding appreciably to the chances of ultimate recovery. When there is thorough disorganization of the limb from injury, the case, of course, is different; but the *exception* should not be made the *rule*; and, finally, whoever looks over the records of surgery will find numerous

instances of preservation and cures, after the most formidable and unpromising injuries of the thigh involving the femur, quite as many, perhaps, as there are recoveries after primary amputation.

BALTIMORE, October 6, 1854.

ART. IX.—*Case of Necrosis of the Lower Jaw.* By G. R. HENRY, M. D., of Burlington, Iowa.

APRIL 21, 1854, I was requested by Dr. Garner, an eminent dentist of this city, to visit, with him, John McCallon, four years old, who was suffering from some disease of the face.

I found a boy, born of healthy parents, who had always enjoyed good health himself, had consequently taken no medicine, and had *been well* until last October, when he was troubled with toothache, from which time he has been more or less ailing. As far as I could gather the history of the case, it was this: An abscess formed in the alveolus of the root of the first molar tooth, which was not opened, and the tooth had not been removed as soon as it should have been; from that time, a fetid discharge of matter has been kept up, destroying the appetite of the boy, and affecting his general health.

The tumour of the jaw had been gradually increasing, until the left side of the face was at least three times as large as its fellow; the swelling had extended to the temple, so that it became a question whether it was not an encephaloid or other cancerous affection of the bone; the matter was discharging from the rest of the bicuspid and first molar teeth, which had been removed, and also from a sinus at the angle of the jaw. I found that, by strong traction with forceps, slight motion could be perceived between the upper and lower portions of the tumour, which made the impression upon my mind that the disease was limited to the alveolar process; but, on cutting down to the jaw, through the alveolus, we found that the bone itself was involved, and, on completing the section of the maxilla, that there was healthy bone beneath it, closely approximating to it—in fact, forming a shallow groove for the reception of the old maxilla. After removing the remaining teeth upon the left side, and several splinters of bone, we concluded to await the efforts that nature was making to throw off the old bone and replace it with a new formation. I prescribed chloride of soda and other detergent washes, to destroy the fetor and to cleanse the parts. This they partially accomplished; and, on the 9th of June, we concluded to remove the diseased portion, and, if necessary, the entire jaw.

The mouth being held open with cork, we were enabled to proceed without any disfigurement of the face from external section, by cutting down upon the bone from within, and grasping it firmly with forceps just in front of the angle, lifting it cautiously and steadily upwards until the maxilla was detached from the groove it was in, and then, using forcible traction forward, the whole bone from the articulation, with condyle and coronoid process, was removed as far forward as the prior section. The bone was bathed in a thin, unhealthy pus; the suppurating process had entirely denuded it of periosteum, and every other attachment to the soft parts, except the portion of gum and mucous membrane which lay over it, and through which the section had been made. We then removed

the portion of jaw in front, including the symphysis mentis, and extending four-fifths of an inch to the right side; but little hemorrhage followed the operation, and this entirely of a venous character; the secretion of pus diminished immediately, and ceased entirely in a few days.

At this time, the boy has perfect use of the jaw; the articulation is a little larger and higher than usual, *but a new bone has formed from the articulation on the left side to the point on the right side where the necrosed portion was separated from the healthy bone*, with the exception of the want of teeth. The new bone is as serviceable, and, apparently, as strong as the old one, and having as perfect motion in the articulation; within a week, a tooth has shown itself on the left side, corresponding to the lateral incision, but it is small, and not very firmly fixed, and has the appearance of being an appendage to the periosteum, rather than that of a tooth with a regular socket.

I exhibited the removed maxilla, at the annual meeting of the Iowa State Medical Society, to many physicians and surgeons of the State. It is a perfect jaw, not splintered or atrophied at all, excepting slight absorption of the condyle; it had very much the appearance of a bone just taken from the macerating tub, after six weeks' exposure to heat and moisture. The new bone, I presume, was secreted by the periosteum of the old, during its struggle between life and death; and, in this beautiful manner, a most admirable substitute has been provided for a necrosed portion of the body.

The horizontal position of the ramus in the young subject facilitated very much the extraction of the bone without external section, and permitted it to be withdrawn in a manner that would have been impracticable with the adult.

The boy is now in good health, and masticates his food without difficulty; in fact, the only inconvenience that he experiences is from being compelled to chew upon one side alone.

BURLINGTON, IA., August 18, 1854.

ART. X.—*Strangulated Inguinal Hernia, the Testicle on the ruptured side never having descended from the Pelvis; Operation; Cure.* By F. M. ROBERTSON, M. D., Lecturer on Obstetrics in the Charleston Summer Institute.

THE subject of this case was a negro man of Mr. H. W., of this city, stout, well made, muscular, and aged about thirty years. Had previously enjoyed uninterrupted good health. The strangulation occurred on the morning of September 12, 1853. He was not aware that the intestine had ever descended before, as he had never observed it previous to the present occasion. Violent symptoms exhibited themselves about daylight. The pain was intense, and the vomiting incessant. When my friend, Dr. Fitch, the family physician, saw him, there was extreme tenderness over the tumour, in addition to the general agony caused by the strangulation. This local tenderness was so great, that the tumour could only be handled while he was under the anæsthetic influence of chloroform. Every effort was made by Dr. Fitch, assisted by Dr. Davega, to reduce the hernia by the taxis, but to no purpose. He was kept in a profound state of anæsthesia for more than an hour, and, although the relaxation of the muscular system was complete, yet the strangulation was not relieved in the slightest degree.

About half-past 10 o'clock I received a message from Dr. Fitch, requesting me

to meet him at 12 o'clock M., for the purpose of operating, in case the symptoms continued to increase in violence, and all efforts at reduction, by the usual procedures, should fail. I saw the case at 12 o'clock. On examination, I found a large painful tumour occupying the left inguinal canal, increasing in size as it emerged from the external ring, and mounting up over the ring. It did not descend into the scrotum, but reached only a short distance below the pubic bone, towards what appeared to be the rudiments of the left scrotal cavity. It was now ascertained that no testicle had ever made its appearance on that side. There was no development of the left side of the scrotum. Whatever existed primitively, had become completely atrophied. He stated that the testicle on that side, to his knowledge, had never made its appearance even in the inguinal canal, as he had never observed any protrusion or swelling in that region before the sudden seizure on that morning. The tumor was tense, and extremely painful—prominent, and had the appearance of being partially divided into three parts, by transverse bands.

Feeling satisfied that no good could result from further delay, as all reasonable and appropriate means had been judiciously and faithfully used, and the symptoms of fatal strangulation still persisting, we resolved to operate.

The patient was placed upon a table, in the usual position, and brought completely under the anæsthetic influence of chloroform; and, assisted by Drs. Fitch, Kinlock, and Davega, I proceeded to operate. The integuments over the tumour were pinched up, by Dr. Kinlock, between the index finger and thumb of each hand, in a direction transverse to the axis of the inguinal canal, which I transixed with a straight bistoury, with the back to the tumour, and cut from within outwards, in the direction of the canal. After enlarging the incision, both above and below, I commenced a careful dissection, as the relations of the parts were necessarily anomalous, in consequence of the non-descent of the testicle. After dividing the tissues down to the sac, the tumour presented a multilocular appearance, in consequence of the transverse bands. Those which seemed simply to adhere to it, were divided; but several digital projections, resembling the end of the finger of a glove, arose from the sac—all, however, communicating with the main cavity. In colour and appearance, the sac resembled the tunica vaginalis, as seen in the operation for hydrocele. A small point was pinched up with the forceps, and divided with a pair of scissors. A considerable quantity of blood-coloured serum escaped. The sac was laid freely open, which exposed a knuckle of the small intestine, occupying the canal and extending below the external ring, highly congested, and of the colour of a half-ripe mulberry; but no spots of mortification could be observed. The stricture at the external ring was divided, and the finger passed into the canal, and, with some effort, above the internal ring; no testicle was encountered in the exploration, though the finger was, with some difficulty, passed above the internal ring, which was dilated without the use of cutting instruments, and the protruded intestine returned without further difficulty. The sac was left in *situ* (in fact, it appeared to be kept in its position by adhesions), and the edges of the wound brought together by three points of interrupted suture and adhesive straps. A compress and spica bandage completed the dressing.

The patient had been kept in a profound state of anæsthesia during the operation, and, as he appeared to be in a good condition, as evidenced by his respiration and circulation, he was placed in a comfortable position on the operating table, where he remained, as though nothing had occurred, in a pleasant slumber. Ordered that he should not be aroused, but, in case he awoke, to have two grains of opium.

6 o'clock P. M. Continued to sleep for some hours after the operation. Took the opium, but threw it up, and was disposed to vomit all liquids that were taken, even cold water. Complained of great soreness in the region of the hernia, but no severe pain. He was not aware that he had been operated on, as he had not been informed of the fact since awaking to consciousness. He was under the impression that a simple compress and bandage had been placed on him during his sleeping condition. Directed perfect quiet, and a full anodyne.

13th, 9 o'clock A. M. Rested well during the night. No more nausea or vomiting; passed urine during the night; no action from the bowels; pulse 62, and regular; skin moist, and of the normal temperature. Complains very much of soreness around the wound. No tympanitis, and very little tenderness on pressure, except in the immediate vicinity of the inguinal canal. Could take a full inspiration without any increase of pain or impediment in the play of the abdominal muscles. He had a troublesome cough, which had existed some days previous to the accident. It was now annoying and painful. Ordered an anodyne expectorant, gruel, if desired, and iced water; an enema at 4 o'clock P. M., if the bowels do not act before. Appears cheerful, and expresses himself greatly relieved, in comparison to his sufferings yesterday morning.

6 o'clock P. M. Had a comfortable day. Vomited only once after the morning visit. Abdominal exploration gave the same condition as at last visit. Enema acted well. Pulse 70, and regular; skin moist, and of the normal temperature. Directed a tablespoonful of castor-oil at bedtime; to be repeated at daylight, if it does not operate by that time.

14th, 9 o'clock A. M. The oil acted three times, without producing any pain; fecal discharge natural in character. Skin moist, and normal in temperature; pulse 66; tongue moist—white fur in the centre, natural around the edges; no tympanitis or tenderness of the abdomen on pressure; soreness in the vicinity of the canal less; cough better; upon full inspiration, free play of the abdominal muscles, without pain. Is anxious to sit up. Ordered to retain the recumbent posture. Continue expectorant, gruel, and iced water.

6 o'clock P. M. Has had one more feculent operation. Pulse 61; other symptoms the same as at morning visit. Gruel, arrowroot, and iced water.

15th, 9 o'clock A. M. General condition the same as last evening. Removed the dressings. Union by the first intention complete, except a small portion of the lower part of the wound, which was designedly left open. Some tenderness on pressure along the course of the canal. Reapplied the dressings, compress, and bandage. Allowed to assume the sitting posture occasionally during the day. Chicken-soup.

16th, 9 o'clock A. M. Condition the same as yesterday.

17th, 9 o'clock A. M. Dressed the wound. Adhesion complete. Patient improving in all respects. From this time, his recovery was rapid. No supuration whatever from the wound, which was completely cicatrized on the 27th of September. He has been permitted to take gentle exercise from day to day, wearing a compress and bandage. From appearances around the external ring, I am under the impression that there is a complete obliteration of that opening. The tumefaction, in the direction of the canal, which was considerable for some days after the operation, has gradually diminished, and now presents a slight indurated ridge along the course of the canal.

On the 3d of October, as far as it is possible to judge, the inguinal canal appears to be completely obliterated; but, as a judicious precaution, a truss with a soft pad was applied. This was soon thrown aside, and no inconvenience was experienced afterwards.

ART. XI.—*Case of Mitral Regurgitation, with great Hypertrophy of the Heart; Systolic Murmur, audible at a little distance from the surface; Open Foramen Ovale; Absence of Cyanosis.* By D. MACGIBBON, M. D., Visiting Physician to the Charity Hospital, New Orleans.

ELLEN LALLY, aged 5 years, was admitted into the Charity Hospital on the 17th of February, 1854, suffering with typhoid fever. After remaining under treatment for about three weeks, she got well, but was permitted to remain about the house for some time after, as her mother, who left her and a sister a little older on their arrival in this city from Ireland, had proceeded up the river without them, leaving them as a legacy to the institution.

She was observed to be languid in her manner, seldom speaking to, or mixing with, the other children in the house; but, as far as I can learn, nothing particular beyond had been noticed by any one.

In the beginning of April, I resumed charge in the female department, and the nurse one morning, soon after, brought this little girl to me, stating that she had caught cold, and coughed at night. The peculiar shape of the chest at once arrested my attention; it was much enlarged in front, giving to it a pigeon-breasted aspect. The action of the heart was very rapid and heaving, but there was no intermission in the beats. A bellows murmur, accompanying the systole, was heard all over the chest—indeed, it could be heard while the ear was within a little distance of the surface—but the point where it was heard most distinct was the apex.

When placed in bed, the action of the heart was such as to perceptibly shake her whole frame. She was quite anemic-looking, and there was some dropsical effusion in the abdomen and lower extremities.

Judging from all the circumstances of the case—the period and location of the murmur, the strength of the impulse, with the shape of the chest and extent of dulness on percussion, and insufficiency of the mitral valve, with much consequent hypertrophy, was the diagnosis made.

From this time she kept her bed, preferring a partially elevated posture. It was difficult to get her to take any medicine. Her breathing became gradually more and more difficult. There was also frequent slight hæmoptysis observed to occur. The action of the heart became more violent and rapid, but it never intermitted in any of its beats.

On the 21st of May she died. A *post-mortem* examination of the body was made, and the following results obtained:—

On removing the sternum, with the cartilages of the ribs, the pericardium was observed to reach far over into the right side of the chest, and to extend as high on both sides as the clavicles. It occupied, in short, the whole anterior of the chest, which, as already mentioned, was abnormally large. The sac was thin, and partially translucent. A small portion of clear serum was found in it when opened. The heart itself was as voluminous as that of an adult. Both auricles and both ventricles partook of this enlargement. These cavities were much distended with blood. The mitral valve was found in a fibro-cartilaginous condition, with its margin much corrugated, leaving the left auriculo-ventricular opening in a state of patency, so as freely to permit of regurgitation taking place, as diagnosed. Besides this, another important organic lesion, not suspected, was found to have existed. The foramen ovale was open, leaving an aperture large enough to receive the end of the thumb. The opening had a thickened, ring-like margin. The semilunar valves of the aorta, as well as

those of the pulmonary artery, were in a perfectly normal state. The caliber of both these vessels seemed out of keeping with the hypertrophied ventricles from which they respectively originate, being no more than sufficient to receive the little finger; but, I suppose, large enough to distribute the blood of so young a subject. The tricuspid valve was a little thickened, and probably permitted of some regurgitation taking place into the right auricle. Both lungs were found to have been much encroached upon; their substance was abnormally dense, and much congested with blood. The liver was enlarged, and it also was much congested. A good deal of serum occupied the abdomen. The heart I have preserved as a pathological specimen, which, with its complication of rare organic lesions and history, to me at least, is interesting.

The point, in the above case, of greatest interest, is certainly the absence of cyanosis, notwithstanding the free communication proved to exist between both sides of the heart. The insufficiency of the mitral valve—diagnosed rightly, as proved by the *post mortem*—was at the time considered sufficient to account for all the symptoms present. Disturbed action of the heart, hypertrophy of the organ, dyspnoea, and hæmoptysis are all circumstances which we so frequently meet with as resulting from that lesion, that no other cause having been suggested in the present case need scarcely be wondered at. Certainly, up to the last, as far as I am aware, nothing occurred to impress on the mind of any one who examined her a suspicion that the foramen ovale was open, and complicated the case, as it proved to be. Of course, had there been any blueness of the surface, this lesion, from the fact of its giving rise to that colour, would not have gone unsuspected; but of this there was not the slightest trace; on the contrary, the lips and other parts usually affected were abnormally pale. Cyanosis is such a very frequent concomitant of open foramen ovale, even when this exists in but a slight degree, that its absence in this case would seem all the more remarkable. Nevertheless, cases are recorded where, as in the present, that did not follow; but these are admitted to be rare. Out of seventy-one cases, for instance, collected by Dr. Stillé, and reported in this Journal (Vol. viii. New Series), where communication existed between both sides of the heart, cyanosis followed in all but five, exhibiting thus such a close connection as to justify the inference of cause and effect. Dr. Walshe, in his work *On the Heart and Lungs*, suggests “that, where a widely-open foramen ovale has been formed (as it certainly occasionally has) without previous cyanosis, some corrective condition, either organic or dynamic, has existed to prevent the intermixture.” Influenced by his remarks, and reflecting on the above case, I cannot help conjecturing that the constantly distended condition of the left auricle with arterial blood, consequent on the insufficiency of the mitral valve, may have prevented that egress of venous blood through the opening in the septum of the auricles, which otherwise might be expected. The presence of the current on both sides of the opening being thus somewhat equalized, we can see a reason for the absence of much commingling of the two bloods, and, consequently, for the absence of the cyanosis in this and similar cases. A pretty free admixture is thought necessary in order to produce this. Whether the opening was congenital, or whether it closed in the usual way and was reopened by the increasing distension of the left auricle, as the lesion of the mitral valve became developed, are questions which our short acquaintance with, and total ignorance of the early history of, the case will not permit of our answering. I am inclined, however, to adopt the latter of these suppositions, as being most consistent with the conditions met with. That the organic disease of the heart must have been far advanced at the period when she entered the hospital with typhoid fever, is more than probable; that it was not detected, may be explained on the supposition that

the physician in attendance had his attention specially directed to the cure of the disease with which she entered, and that nothing occurred, while under his charge, to call his attention to the condition of the chest, as mine had been at a later period, and that no auscultation had therefore taken place. Without this means of informing ourselves, but little positive can be learned of these cases; with it, much, but not everything.

ART. XII.—*Annual Report of THOMAS LAWSON, Surgeon General, for the fiscal year ending 30th June, 1854, to Hon. Jefferson Davis, Secretary of War.* (Communicated by the Author.)

SIR: I have the honour to lay before you a statement of the fiscal transactions of the Medical Department, and a report upon the sickness and mortality of the army, for the year ending on the 30th day of June, 1854.

The amount of the appropriation for the Medical and Hospital Department remaining on the 30th of June, 1853, was—			
In the hands of disbursing agents . . .		\$1,528 80	
In the Treasury of the United States . . .		33,842 70	
Amount appropriated per act approved 3d March, 1853		52,000 00	
Amount refunded by Col. H. Whiting . . .		1,025 92	
“ “ Col. T. F. Hunt . . .		584 67	
“ “ W. Williams . . .		31 89	
			\$88,977 98
Of this sum, there has been expended on account of—			
Pay and other claims of private physicians contracted in 1847	\$123 57		
“ “ “ “ “ 1848	597 03		
“ “ “ “ “ 1849	1,253 18		
“ “ “ “ “ 1850	2,217 65		
“ “ “ “ “ 1851	3,619 31		
“ “ “ “ “ 1852	1,771 06		
“ “ “ “ “ 1853	5,712 40		
“ “ “ “ “ 1854	8,129 31		
		\$23,428 51	
On account of medical supplies, &c. &c. contracted in 1847		5 50	
“ “ “ “ “ 1848	517 59		
“ “ “ “ “ 1849	402 08		
“ “ “ “ “ 1850	382 98		
“ “ “ “ “ 1851	674 35		
“ “ “ “ “ 1852	912 40		
“ “ “ “ “ 1853	1,721 61		
“ “ “ “ “ 1854	24,621 63		
		29,238 14	
Total amount expended		\$52,666 65	
Leaving in the hands of disbursing agents .	4,911 66		
And in the Treasury of the United States .	31,399 67		
		36,311 33	
			\$88,977 98

It appears, from the foregoing exhibit, that the total expenditure on account of the medical department of the army, during the year ending on the 30th of June, 1854, was fifty-two thousand six hundred and sixty-six dollars and sixty-five cents (\$52,666 65). Of this sum, fifteen thousand two hundred and ninety-nine dollars and twenty cents (\$15,299 20) were paid on account of the claims of private physicians, for services rendered, and four thousand six hundred and sixteen dollars and fifty-one cents (\$4,616 51) for medical and hospital supplies purchased, both accounts originating prior to the commencement of the last fiscal year; leaving as the expenditure *proper* of the Medical Bureau, for the service of that year, the sum of eight thousand one hundred and twenty-nine dollars and thirty-one cents (\$8,129 31) for pay of private physicians, and twenty-four thousand six hundred and twenty-one dollars and sixty-three cents (\$24,621 63) on account of medical supplies; making a total of thirty-two thousand seven hundred and fifty dollars and ninety-four cents (\$32,750 94).

The annual report of the sick and wounded of the army, as consolidated in the tabular statement herewith transmitted, exhibits the following results:—

The number of officers and men remaining sick on the 30th of June, 1853, was 658, and the number of cases of disease which occurred during the succeeding year was 24,998; making an aggregate of 25,656 cases that have been under medical treatment within the twelve months ending on the 30th of June, 1854.

Of the whole number of sick, 24,570 "returned to duty," 25 were placed "on furlough," 181 were "discharged service," 51 "deserted," and 258 "died;" leaving 271 "sick" and 300 "convalescents" still under medical treatment.

The mean strength of the army, for the year ending June 30, 1854, was, according to the returns in this office, 8,095; and, as the number of cases of sickness reported for the same period was 24,998, the relative proportion of cases of disease to the number of officers and enlisted men in the army was 3.08 to 1; so that, on an average, each individual was sick three times during the year.

It also follows, from the foregoing data, that the ratio of deaths to the number of officers and enlisted men was 1 to 31.37, or 3.18 per cent., and that the proportion of deaths to the number of cases of disease treated during the year was 1 to 99.44, or 1.005 per cent.

The increased ratio of mortality exhibited by this report, as compared with those for the two preceding years, arises from epidemic cholera, and also from the prevalence of yellow fever, which so terribly ravaged a great portion of the Southern and Southwestern States in the summer and fall of 1853. Of the former disease there were thirty-one cases, and eighteen deaths; and of the latter, three hundred and forty cases are reported, of which eighty-three died.

The mortality in the army, *generally*, during the last year, as has been shown, was greater than the ordinary average in time of peace; but the number of deaths among the officers of the medical staff, within the last sixteen

months, has been extraordinarily great, the proportion being 1 in 12, or 8 per cent., a mortality among officers seldom exceeded except in war.

Although we have been called upon to mourn the loss not only of many of our number, but of some of our best and most distinguished officers, it will be doing no injustice to others, whether of the living or of the dead, to render a richly-merited tribute of respect to the memory of Surgeon Thomas G. Mower, for many years the senior surgeon of the army, its chief medical purveyor, and the presiding officer of its Boards of Medical Examiners. During a service of forty-one years, he had frequently confided to him the highest and most responsible duties, all of which were invariably performed to the satisfaction of the department. To the judgment and discretion with which he exercised the power delegated to him as President of the Army Boards of Medical Examiners, the medical staff owes much of its present efficiency and reputation, and it is hoped that the influence of his example will not be lost.

The meteorological observations, commenced under the auspices of this Bureau in 1819, have been steadily continued at all the military posts occupied by troops, and monthly registers thereof have been regularly transmitted to this office. The results of these observations, from the commencement to 1842, inclusive, have been published, and those from that period to 1850 have been arranged and tabulated, and are now ready for the press.

The observations taken at the stations in Texas, New Mexico, California, and in Oregon and Washington Territories, which have been established since the war with Mexico, have proved of special value to the science of meteorology, by affording the necessary data for determining the mean temperatures, fall of rain, and other climatic peculiarities of the region lying between the Western and Southwestern States and the shores of the Pacific. In order that these observations might be made of as much practical benefit as possible, the meteorological registers have been freely loaned to the Smithsonian Institution, and to officers of the army in charge of the recent surveys for a railroad route to the Pacific; and it is to data obtained from this office that the paper on "Agricultural Climatology of the United States," appended to the *Report of the Commissioner of Patents* for the year 1853, owes much of its completeness.

The Army Medical Board, for the examination of assistant surgeons for promotion, and of applicants for appointment in the medical department of the army, to which reference was made in my last annual report, convened in the city of New York, on the 1st day of December, 1853, in obedience to your orders, and remained in session more than two months.

By this Board, *twelve* assistant surgeons were examined; all of whom having been approved, they were recommended for promotion.

Of the *forty-nine* candidates for appointment in the medical staff of the army, invited to present themselves for examination, *thirty-three* reported in person, and *one* by letter; *two* withdrew voluntarily, without an examination; *five* withdrew at a more or less advanced period of their professional examina-

tion; *three* were found physically disqualified, and *twenty-four* were fully examined; and of these last, *fifteen* were found qualified for appointment.

All the candidates approved by the Medical Board of 1851 have been commissioned, as have also *four* of those who received favourable reports from the last Board, leaving *eleven* candidates legally qualified for the appointment of assistant surgeon in the army.

The numerical strength of the medical staff being inadequate to the demands of the service, a number of private physicians have been, for some years past, necessarily continuously employed at the different arsenals and military posts in the vicinity of towns; and, although I have concluded to refrain from recommending at this time any addition to the present number of medical officers, an increase will be absolutely necessary in the event of new regiments being added to the present military establishment. I am the more particular in making this statement, for the reason that in the several bills before Congress at its last session, having for their object the creation of an additional military force, no provision was made for a corresponding increase of the medical department.

I cannot, consistently with my sense of duty, close this report without earnestly inviting your attention to, and renewing, the several recommendations made in my last annual report, and which have for their object—

1. That soldiers acting as nurses and attendants in hospitals be allowed the extra pay authorized to other soldiers on fatigue duty, by an act approved March 2, 1819, the provisions of which were extended by “an act to increase the pay of the rank and file of the army,” &c., approved August 4, 1854, by which soldiers employed on “constant labor of not less than ten days” are entitled to receive 25 cents per day at stations east of, and 35 cents per day at stations west of the Rocky Mountains.

2. The enlistment of a certain number of qualified persons to serve specially as hospital stewards.

3. That the assistant surgeons of the army be relieved from the operation of the proviso to section 1 of “an act making appropriations for the support of the army for the year ending 30th June, 1846,” approved March 3, 1845, by which their allowance of forage ceased to correspond with that of cavalry officers of the same grade, as established by the 24th section of “an act to increase the present military establishment of the United States, and for other purposes,” approved July 5, 1853, and which is still unrepealed.

Annual Report of the Sick and Wounded of the Army of the United States for the year ending 30th June, 1854—CONTINUED.

QUARTERS.		THE BRAIN AND NERVOUS SYSTEM.										THE URINARY AND GENITAL ORGANS.										THE SEROUS EXHALANT VESSELS.		THE FIBROUS AND MUSCULAR STRUCTURES.		ABSCESSSES AND ULCERS.		WOUNDS AND INJURIES.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																										
		Apoplexia. Cephalalgia. Chorea. Delirium tremens. Epilepsia. Mania. Melancholia. Meningitis. Neuralgia. Paralysis. Tetanus. Congestio cerebri. Cerebritis. Calculus. Cystitis. Diabetes. Enuresis. Gonorrhoea. Ischuria et dysuria. Nephritis. Orchitis. Strictura urethrae. Syphilis primitiva. Syphilis consecutiva. Ulcus penis non syphiliticum.										Anasarca. Ascites. Hydrocele.		Pernio. Podagra. Rheumatismus acutus. Rheumatismus chronicus. Fistula. Phlegmon et abscessus. Ulcus. Ambulatio. Amputatio. Concussio cerebri.		Contusio. Fractura. Luxatio. Punctio. Subluxatio. Vulnus incisum. Vulnus laceratum. Vulnus punctum. Vulnus sclopeticum. Vulnus contusum. Anchylosus.		Phlegmon et abscessus. Ulcus. Ambulatio. Amputatio. Concussio cerebri.		Contusio. Fractura. Luxatio. Punctio. Subluxatio. Vulnus incisum. Vulnus laceratum. Vulnus punctum. Vulnus sclopeticum. Vulnus contusum. Anchylosus.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																		
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Annual Report of the Sick and Wounded of the Army of the United States for the year ending 30th June, 1854—CONTINUED.

[illegible]

All of which is respectfully submitted.

TH. LAWSON, Surgeon General.

SURGEON GENERAL'S OFFICE, November 8, 1854.

ART. XIII.—*Exostosis of the Orbit*. By MARK STEPHENSON, M. D.,
Surgeon to the N. Y. Ophthalmic Hospital.

INASMUCH as exostosis of the orbital bone is a rare affection, the following case may be worthy of record:—

Mr. Walton, in his admirable treatise on operative ophthalmic surgery, remarks, that few surgeons, even in large ophthalmic practice, have met with the disease in this location.

Dr. Buck, one of the surgeons of the New York Hospital, who has had extensive opportunities of seeing a vast amount of disease, informs us that he had never seen a similar case.

Mrs. S. W., aged 18 years, of good constitution, three years preceding the date of consulting me, perceived for the first time a slight enlargement of the bone immediately beneath her eyebrow, which continued gradually to increase year after year, but without any pain or constitutional disturbance. When I first saw her, it was over two inches in diameter at its base, and hung down like an inverted cone over the eye.

It completely counteracted her vision, excepting to a very limited extent; she could discern objects at her feet, or anterior to her, when the head was thrown backwards. No satisfactory cause could be assigned for it, and she has never experienced any inconvenience from it excepting its mechanically obstructing vision.

I at once informed her that nothing short of an operation could remedy her difficulty; she gave her consent, and fixed upon a day for its removal.

The operation was performed on the 30th January, 1850, in which I was assisted by Drs. Buck, Stone, and Macfarlain.

The patient was put under the influence of chloroform, after which a straight incision was made over the superciliary ridge extending from the internal to the external angular processes of the frontal bone; and from the centre of which a vertical incision was made at right angles with the first, and nearly of the same length—dissecting up these gave us a triangular opening.

The next step in the operation (after denuding the bone of a portion of its pericranium) consisted in removing the bony tumour. This was done by the use of Hey's saw, but, owing to the hardness and thickness of the bone, it required some time to reach the frontal sinus, after which, with the aid of a pair of strong cutting bone-forceps and an elevator, the bony prominence was removed, exposing to view a portion of the lachrymal gland.

The lining membrane was found partially diseased, and a very small fungus attached to it. The parts were then cleansed and the flaps of the wound secured by interrupted sutures, adhesive plasters, &c. The integuments united by the first intention, excepting at the internal angle of the wound, which

remained a fistulous opening for eighteen months, during which time it continued to emit a muco-purulent discharge. This, doubtless, was owing to a chronic inflammation of the mucous membrane lining the inferior surface of the sinus.

Although the patient was *enceinte* some four months, and was under the influence of chloroform for nearly one hour, yet no unpleasant consequences resulted from it, or the operation; and in due time she was delivered of a healthy living child.

The ligament through which the tendon of the levator palpebra superioris passes being attached to the orbital portion of the frontal bone, I was fearful that the functions of the muscle would be destroyed, but the movements of the superior palpebra is as perfect as it ever was.

It is now over four years since the operation was performed, and there is not, at this time, the slightest evidence of disease remaining, and little or no deformity; nor has there been for the last three years.

NEW YORK: 167 East 14th St.

REVIEWS.

ART. XIV.—*A Systematic Treatise, Historical, Etiological, and Practical, on the Principal Diseases of the Interior Valley of North America, as they appear in the Caucasian, African, Indian, and Esquimaux Varieties of its Population.* By DANIEL DRAKE, M. D. Edited by S. HANSBURY SMITH, M. D., formerly Professor of Theory and Practice of Medicine in Starling Medical College, Ohio; and FRANCIS G. SMITH, M. D., Professor of the Institutes of Medicine in the Medical Department of Pennsylvania College, Philadelphia. Second series. 8vo. pp. 985. Philadelphia, Lip-pincott, Grambo & Co. 1854.

THIS second volume, or series, as it is improperly termed, of Dr. Drake's great work, comprises the history, etiology, pathology, and therapeutics of the individual diseases which prevail among the motley population of the immense interior valley of North America—so far, at least, as the materials were accumulated and partially arranged by the lamented author. Of a large number of the diseases embraced in the plan of the work as originally designed, he had written no account; and even in those treated of in the volume before us, several of the accounts are deficient in that fulness and completeness of detail which they no doubt would have presented had the life of the author been spared.

The editors to whom the manuscripts of the author were intrusted to prepare them for the press, have very successfully accomplished their laborious and difficult task; for which they deserve the thanks of the profession. From the materials left by Dr. Drake, they have prepared very full histories of the several fevers, and nearly so of the majority of the diseases embraced in the first five classes of the author's list of the phlegmasiæ.

The account of the *Autumnal Fevers*, with which the present volume commences, is the same as that contained in the former volume. It occupies the eleven chapters of the first part. It is reprinted here, we presume, in order to bring together, in the same volume, the entire portion of the work devoted to the consideration of the pathology and treatment of the diseases of the immense intermontane region, the topographical, hydrographical, physiological, and social etiology of which was the leading subject of the first volume.

The second part of the present volume is devoted to the Yellow Fever; the geography, chronology, local history, etiology, symptoms, pathology, and treatment of which are very fully considered.

In regard to his sources of information in relation to this disease, Dr. Drake remarks:—

"I have never seen a case of yellow fever; but am not precisely in the condition of a historian, who might have only printed works from which to compile. I have visited and studied the topography, hydrography, and climate of a number of the most infested localities; and, what is of equal or greater advantage, enjoyed the opportunity of conversation with a great number of physicians, who had become familiar with the disease in all its aspects. A large part of the facts about to be presented was thus obtained, while the remainder

have been drawn from publications by physicians resident in the region embraced in the plan of this work.

"Thus, it is our own yellow fever, that of the Gulf of Mexico, and tributary rivers, that will be here described; and if the description should not be true of the disease as found in all countries, it will be because it is true of that which prevails in our own."

From the history of the disease in the different localities where it has prevailed throughout that portion of the Valley of the Mississippi lying below the thirty-fifth degree of latitude, Dr. Drake presents certain general deductions in reference to its etiology.

The question of its origin in, or propagation by contagion, is very fully discussed. That the yellow fever is not capable of being propagated from the sick to the well in any locality to which the disease is not endemic, and where it is not at the time prevailing, Dr. Drake believes to be a fact very fully established, both by direct testimony, and various analogical considerations; nor does he consider the hypothesis which maintains that the predisposing impress of impure air is a necessary condition to the action of the contagion, as one that has a foundation in either positive facts or probability. Another very favourite hypothesis with a few pathologists is, that the contagion of yellow fever acts upon the impurities suspended in the atmosphere of places where and when the disease prevails, and excites in them an intestine or fermentative action, which transforms them into its own nature. This notion of contagious fermentation, Dr. Drake rejects as one resting solely on assumptions, and liable to objections that are unanswerable.

Intimately connected with this doctrine of the morbid contamination of an atmosphere by the introduction into it of a specific ferment, is that of the importation of the yellow fever by infected vessels or cargoes arriving from places where the disease prevailed at the period when they left.

"If," Dr. Drake remarks, "we can show that the disease has arisen independently both of contagion and of ferments, under circumstances which deteriorate the atmosphere with the material on which the ferment is said to act, we have made out a case identical with that which originally produced the fever, and proved that it is still, as at its primitive appearance, a disease of local origin; the offspring of certain conditions, and a necessary occurrence whenever and wherever those conditions exist.

"What, then, are the proofs of this origin?

"1. When treating of the origin of the fever at Pensacola, I have given the history of the generation of the fever on board of three national ships, the Natchez, Vincennes, and Hornet, altogether apart from any external influence; and when giving its history at Donaldsonville, I stated an instance of its being generated in a small boat on a voyage from Baton Rouge to that town, the disease not existing in either. I might greatly extend this catalogue if I chose to introduce cases of a less rigid character; for the number of instances of alleged spontaneous origin on ships, in the Gulf of Mexico, is very great, but I do not wish to employ apocryphal data.

"2. Passing by New Orleans, where the local and commercial conditions are too much mixed for a satisfactory estimate of the influence of either, I may refer to Mobile, as having several times been invaded, in such parts of the city, and under such circumstances, as to convince the whole of its medical men, and all its observing population, that the fever did, indeed, arise independently of importation. I may also refer to Natchez, some of whose epidemics began under circumstances which seem utterly adverse to the hypothesis of importation; likewise to the country around Washington, where we have the authority of Dr. Branch and Dr. Monette, the latter a powerful advocate of importation, that the disease has occurred from local causes; to Appalachicola Bay, where, according to Dr. Hart, several cases occurred when no ships had arrived from a distance; to Fort Smith, where the disease appears to have prevailed in

1823; to Memphis, where it was epidemic in 1828, and, as far as observation could guide those who witnessed it, arose from conditions existing on the spot; finally, in our examination of the origin of the fever in all the times and places where it has prevailed in the northern basin of the Gulf, we found that in almost every case which admitted of a strict examination, the asserted importation of the disease could not be made out; but if it were not imported, it was of local origin.

"3. It does seem to me, then, that the fact of a local origin of some cases is as well established as medical facts generally are, and that we are at liberty to reason from it as a *fixed datum*. But does this prove that the disease is indigenous? The answer must be in the negative. For it is *possible* that in all of those cases a portion of the ferment may have been introduced long before, and lain dormant for want of that element in the atmosphere on which it is said to act; and it is also possible, that when such an atmosphere was generated, the ferment took effect. We cannot disprove this; and, therefore, admitting all the facts which have just been cited, they do not demonstrate that the fever arose from causes entirely local. But, although that grade of proof is not attainable, we may certainly approximate to it; for, in the first place, the very existence of such a ferment is, as we have shown, an assumption; in the second place, as it is represented to have been a product of the South, and as cold suddenly arrests the fever, it seems highly improbable that it could survive the action of several winters; and, in the third place, it is contrary to all analogy that it should neither undergo spontaneous decomposition through so long a period, nor evaporate, nor be absorbed by the bodies to which it adhered, through a series of years, and thus be lost. If these reasons do not annihilate the claims of the doctrine of a ferment, it must, I think, be admitted that it reduces them to the very lowest degree of probability.

"4. The occurrence of the fever from local conditions, independent of any foreign agency, being established in even a single instance, the controversy is, *de jure*, at an end; for if it can be thus generated in one case, it may in all, and to say that it is not, is to superadd a hypothetical to a known cause, in violation of the rules of philosophizing. He, indeed, who admits its production in this manner in one instance, and denies it in others, is bound to support his denial—he brings upon himself the *onus probandi*. When a phenomenon has been once proved to arise from a certain cause, true logic requires us to refer it, whenever and wherever it may afterwards appear, to the same cause. If, then, yellow fever on board the ship *Natchez*, or in the vicinity of Washington, or in the village of Memphis, arose from local causes only, it has arisen in every other town through which we have travelled, from like causes; in other words, is indigenous.

"Having made this generalization, let us proceed to inquire whether in its history it conforms to the laws of the endemic fevers with which we propose to compare it.

"1. It prevails in a certain season of the year, setting in about the time the surface of the earth has attained its maximum heat, and ceasing when the temperature is reduced below a certain point.

"2. While, like autumnal fevers, it extends indefinitely to the South, it has, like them, shown itself limited to the North; but the limits of the two diseases are not the same, the boundaries of the former being much higher than those of the latter.

"3. Within these limits, it invades certain localities, and is never seen in others, and the same is true of autumnal fever. From some, it is scarcely absent any year; while others are invaded only now and then, and the same is true of them.

"4. In some seasons, as those of 1819, 1827, 1829, and 1843, but above all, in that of 1839, it appeared as a wide-spread epidemic, while in others it has been limited to a few places, or even to New Orleans; and autumnal fever varies in a similar manner.

"5. It is often seen to be influenced in its aspect by the cause of autumnal fever. The cause of typhoid fever has now and then produced modifications in its symptoms. In the year 1811, at *Terre aux Bœufs* and New Orleans, in

the army, it was modified by scorbutus. During the prevalence of epidemic cholera, in the latter, it assumed many of the characteristics of that epidemic. The whole of which is true of autumnal fever.

"6. In successive years, at the same place, it presents diversities which cannot be explained; and in the same season, in different localities, variations of character have been noted, which is also the case with autumnal fever.

"Thus conforming to nearly all the laws of our non-contagious, endemic, epidemic fevers, it presents additional claims to be regarded as such. It meets, in fact, all the requirements of the theory of indigenous origin.

"Is yellow fever, then, it may be asked, merely the remittent autumnal fever with which we are all familiar? The answer, according to these views of its origin, is that it is one of the varieties, as a tertian fever is another. That it springs from a peculiar modification of the remote cause in the localities where it prevails; but that this modification is produced by local conditions, not by an imported ferment. That this variation in the character of the agent which produces it is progressive. That the fever in June and July may wear the aspect and possess the intrinsic pathological nature of remittent autumnal fever, but in August, September, and October, display the more malignant character of yellow fever, returning in November to the milder form of an ordinary remittent.

"Such is the malarial theory of this formidable disease; that which is held by a majority of the physicians of the South; a theory which inculcates cleanliness and free ventilation in houses, yards, alleys, streets, boats, ships, and wharves, to obviate the development of civic malaria, which being prevented, the fever, according to the theory of an importation of ferments to act on that malaria, would equally fail to appear.

"But I cannot regard this theory as finally established, although obnoxious to fewer objections than the theory of ferments; for—

"1. The whole pathological history of yellow fever *seems* to constitute it a specific fever, differing from an autumnal remittent too far to admit of its being a mere variety of that disease.

"2. It has occurred in localities where civic malaria can scarcely be admitted to exist, as in Washington and its vicinity, in the State of Mississippi; in Opelousas; and in the navy-yard on Pensacola Bay, where a white sand-drift constitutes the surface of the ground.

"3. It has been produced by exposure to the air, which has escaped from goods sent from a city where the disease prevailed [? Ed.]. But I know of no facts which go to show that autumnal fever is ever produced in that manner. It is scarcely necessary here to repeat that there is no well authenticated example of its spreading from this source.

"4. While the condition of a town, to all visible appearance, continues the same, the disease will appear and disappear, precisely as it does while the commercial intercourse with a place continues unchanged.

"Until these objections are removed, the cause of this disease should be kept *sub judice*. We are not compelled to choose among conflicting theories, and declare that which is best supported to be established. It may be more probable than any other, and yet not be true."

We have presented the foregoing extract, as well to show the manner in which the leading questions in reference to the diseases, the histories of which are comprised in the volume before us, are treated by Dr. D., as from the fact of the clear and logical refutation set forth in it, of certain erroneous doctrines in regard to the etiology of yellow fever, which, we regret to find, are becoming popular with a portion of the American medical profession.

A very minute and accurate history of the peculiar symptoms of yellow fever—or those which seem best to demonstrate its specific character—is given in Chapter VII.

Under the head of "Modifying Influences," Dr. D. remarks that many of the modifications which the symptoms of yellow fever present, in different epidemics, are due to four or five eccentric tendencies. Thus:—

"1. Many epidemics are characterized by the phenomena of ardent inflammatory fever. They occur when there is a general atmospheric constitution of the phlogistic kind, which imparts to all febrile affections a more intensely inflammatory character.

"2. When a typhous epidemic constitution is prevalent, yellow fever presents, in its symptoms, a tendency the opposite of the last.

"3. In a highly malarial state of the atmosphere, it shows, in its milder attacks, a leaning to the type of simple autumnal remittent fever; in the more violent, a simulation of congestive or malignant remittents.

"4. When epidemic cholera appeared in New Orleans, yellow fever was prevailing, and soon put on so many symptoms of that disease, that many cases were only recognized as such by the fatal hemorrhage and black vomit of the closing stage.

"An attention to these and facts of a similar kind, is necessary to a reconciliation of the different histories of the disease, and the great diversities of treatment which physicians of equal skill and veracity have reported as successful or prejudicial."

After presenting an account of the pathological anatomy of yellow fever, drawn chiefly from the observations recorded by the historians of the disease as it has prevailed in the Valley of the Mississippi, Dr. D. proceeds to consider its pathology. His remarks, under this head, embody no views of sufficient interest or novelty to demand any particular notice or comment.

In regard to the treatment of yellow fever, after noticing the fact that the disease has "a *quasi* definite duration," "a character of *self-limitation*," in consequence of which many cases would terminate favourably if nothing were done, as others end fatally under the most vigorous treatment, Dr. D. examines, in succession, the applicability and value of the several remedies that have been employed in the different stages of the disease.

Bloodletting he considers not to be a specific for yellow fever—as not *the* remedy, but still *a* remedy—not applicable to all, but yet to many cases—improper in some epidemics, demanded in others—not to be resorted to as a matter of course, but with discrimination—chiefly beneficial in the early, often pernicious in the latter stages—not employed to cure the disease, but to produce the mitigation of excitement, and create a predisposition for the beneficial effect of other remedies, and through these effects, combined with the effects of other measures, causing the fever to run its course without destroying life.

Local bleeding, Dr. D. considers, may, in many cases, when vigorously employed, be even preferable to venesection; but in others it is less to be relied upon, unless, indeed, it be carried so far as to produce a constitutional in addition to a local effect, when probably it would be quite as well to rely on the lancet. In hospital practice, local bleeding may, to a certain extent, be found beneficial as a means of abstracting blood from particular localities, and as many patients are brought in when the disease is too far advanced to permit of venesection. It must be borne in mind, however, that in the latter periods of the fever the bleeding from leech-bites cannot always be restrained, and has sometimes proved dangerously exhausting to the patient.

In regard to *emetics*, Dr. Drake cannot but think that in the forming stage of the fever, when the stomach happens to be loaded with undigested food, or, which sometimes happens, with bile, as is evinced by its spontaneous ejection, an emetic might be of service; while there cannot then be any active inflammation to contraindicate its use. In very malignant cases, such as will not in any stage admit of bleeding, an emetic might promote reaction; but, administered in such a state of the system, it should be of a stimulating character.

Purgatives.—Of the early necessity of evacuating the contents of the stomach and bowels, which, retained, would prove a source of irritation, Dr. D. thinks there can be no doubt. The following are the restrictions which he lays down as those which theory imposes on the use of purgatives, and the rules which it suggests for their administration :—

"1. Those which are least offensive to the stomach, and least likely to irritate the bowels, should be chosen.

"2. When the reaction of the system is feeble, and when the excitement is declining, they should be exhibited sparingly.

"3. When the excitement runs high, copious bleeding should precede their operation, if not their administration.

"4. Such as increase the secretions from the liver and the mucous membrane should be preferred.

"5. When the head is greatly affected, and the excitement of the system considerable, their operation may be pushed further than under other circumstances.

"6. As a means of relieving the congestion of the abdominal viscera, not of producing revulsion from distant parts, their action should be moderate, otherwise, as we have seen, they will invite blood into those organs.

"7. As evacuants from the lower bowels, when costiveness exists in connection with an irritable stomach, injections should precede the administration of purgatives."

Calomel, as a cathartic in yellow fever, receives the sanction of Dr. D.; the general abandonment, however, of the use of this and other preparations of mercury, with a view to their specific effects on the system, by the physicians of yellow fever regions, is to be received, he considers, as *prima facie* evidence that, when thus administered, they exercise no beneficial influence on the disease. The objections to the mercurial treatment are :—

"1. It generally fails to re-excite the functions of the liver. 2. The stage of excitement is too short to permit the establishment of a constitutional effect. 3. When that effect has been produced, it has not prevented the stage of collapse with its petechiæ, passive hemorrhages, and black vomit, but, in the opinion of some physicians with whom I have conversed, seemed to aggravate them. That this is not altogether imaginary, may be inferred from the well-known injurious effects of calomel in scurvy, a disease in which the blood, as in the third stage of yellow fever, is deficient in fibrin. 4. Those who have recovered under the constitutional impress of calomel have often experienced a tedious convalescence, in connection with a vexatious ulceration of the mouth. 5. By relying on calomel, other things of less equivocal value have sometimes been neglected."

On the efficacy of the *sulphate of quinia* in the treatment of yellow fever, Dr. D. presents the following remarks :—

"It is undeniable that the quinia has proved beneficial in the practice of a number of our physicians; but can it be regarded as a specific, even in the modified sense in which it is a specific in autumnal intermittent fever? I think not; for, in the hands of several physicians, it has not produced any decided benefit, and all who have used it extensively have seen patients die with black vomit after their systems had been thoroughly brought under its influence. Indeed, of its value in yellow fever, unmodified by any autumnal intermixture, we have not any satisfactory proof. Nevertheless, so many epidemics present a mixed character, that, even limited to them, its importance is very considerable. When no alliance of this kind exists, I am disposed to believe that it may shorten the stage of excitement, if exhibited in large doses immediately after copious depletion. Its narcotico-sedative effects on the nervous system of animal life, and the centrifugal determination of the blood which it favours, as is shown by the diaphoresis which follows its exhibition, are well fitted to extinguish the morbid excitement. But, in *pure* yellow fever, is this the subdual of

the disease? It would seem not; for after the patient has lain for two or three days under its influence, in what Dr. Lewis calls the 'state of calm,' collapse, black vomit, and death may happen. There is, then, an insidious lesion of the system, which it does not reach; a root of malignity it cannot eradicate."

Sudorifics.—After spontaneous diaphoresis has occurred, Dr. D. considers that no doubt can be entertained as to the propriety of maintaining and even promoting the secretion of the skin; great care, however, being taken not to irritate the stomach by the means employed nor to induce excessive sweating. Of the diaphoretics that have been used, the best, perhaps, Dr. D. remarks, are the spiritus mindereri; the common saline mixture; laudanum and the spirit of nitrous ether, to which, when the stomach will bear it, a little wine of ipecac. may be added; small doses of Dover's powder; and an infusion of sage, serpentaria, or orange leaves, the last of which is a favourite ptisan.

We cannot follow Dr. D. in his account of the treatment of the third stage.

The rules to be then observed, whether in regard to the avoidance of causes calculated to impede recovery, or for the removal of whatever pathological lesions may still continue, are those which would suggest themselves to every well-instructed and experienced practitioner. Happily, in favourable cases, but little positive treatment is required. Even in those cases, in which the third stage is attended by symptoms of an unfavourable character, it is a question whether it is not better to leave them, in a great measure, to nature, than to irritate the stomach by medicines.

A very interesting comparison of yellow fever with autumnal fever, in respect to the geography and chronology, the subjects, symptoms, treatment, pathological anatomy, sequelæ and mortality, of the two diseases, is given. A contrast exhibiting such well marked diversities between the two as would appear to very clearly mark autumnal fever and yellow fever as distinct diseases, depending on different remote causes. These causes would sometimes seem to act on the system simultaneously, producing a hybrid disease—intermittent yellow fever.

The third part of the volume before us, is devoted to a consideration of Typhous Fevers; under which denomination the author groups the synchus, adynamic, typhus mitior, typhus gravior, and typhoid fever.

"Modes or varieties of fever," Dr. D. remarks, "which resemble each other so closely in the circumstances under which they prevail, their symptoms, and the treatment they require, as to render a distinctive diagnosis difficult, can scarcely be regarded in any other light than as mere varieties; and although, when well characterized, it may be very proper to designate them by different names, we should not forget that the terms employed express mere varieties, as the words quotidian and quartan indicate varieties of intermittent fever, and inflammatory and congestive, varieties of remittent autumnal fever."

After some very interesting facts in reference to a general typhous epidemic, and the histories of the various sub-epidemic and sporadic visitations of typhous fevers, and of the Irish immigrant fever, Dr. D. presents the general etiological conclusions deduced from these histories, and such additional observations furnished by other countries, as were necessary to supply deficiencies.

As to the relation of continued fevers to the seasons, Dr. D. finds that they make their appearance in every season, but begin more than twice as often in fall and winter as in spring and summer; prevailing much oftener in autumn and winter than in the other seasons, and most frequently of all in winter; that they sometimes cease with the access of summer, reviving in winter, again to cease when summer returns, and that the same remark is conversely true of winter. On the whole, that they prevail more in cold than in warm weather.

In regard to their climatic relations, Dr. D. finds that, in our interior valley, continued fevers have their chief prevalence between the 32d and 48th parallels; in western Europe, between the 44th and 60th. Where the mean annual temperature rises above 62°, or falls below 40°, they prevail but little on either continent. The isothermal curves which best represent them in Western Europe are 48°—52°. In this country, they have not prevailed sufficiently to justify a conclusion on this point.

As to the influence of the annual range of temperature on the prevalence of typhous fever, Dr. D. believes it to be inversely to the range of winter and summer heat, being least in the Interior Valley, greater on the islands, and intermediate on the continent of Europe.

He compares this climatic law of continued fever with those of yellow fever and autumnal fever. Thus:—

“American summer temperature of 80°, or above, with a yearly temperature of 65° or upwards, is necessary to the prevalence of yellow fever in our Valley, and an average summer heat of 60° or more, with a yearly heat of 40° or upwards, is necessary to the production of autumnal fever. Thus, as a general law, yellow and autumnal fevers are in direct proportion to the heat of summer, while the continued fevers are in reverse proportion. Yet there is one point of view under which they conform to the same law. They all prevail most where the range between winter and summer is least. Thus, at St. Louis, the range is 42°, and yellow fever is unknown; at Natchez, it is 29°, and the fever prevails occasionally; at Havana, only 10°, and there the yellow fever is as constantly present as typhus fever in Dublin. In the latitude represented by Cincinnati, the range is 40°, and the prevalence of autumnal fever is moderate. In that represented by Baton Rouge, the range is 28°, and the fever prevails every year as certainly as continued fever prevails in Glasgow or Edinburgh.

“It appears, then, that while climates which present but a limited range between their winter and summer temperatures, may, on that account, be exempt from some diseases, they are proportionably more subject to others; in the lower latitudes to yellow and periodical fevers, in the higher to typhous fevers. But how is it that the warm winters and cool summers—the moderate annual range of Great Britain and Ireland, favour the production of continued fevers? This question cannot be answered without the introduction of another meteorological fact, viz: that where the range between winter and summer is small, the diurnal range is correspondingly small. Thus, in those islands it is much less than in the zone of the Interior Valley with which we are comparing them. Now, sudden and violent changes, and extremes of atmospheric heat, tend directly and powerfully to generate a phlogistic diathesis, which favours the production of phlegmasial diseases, but not of typhous fevers. Such a diathesis may, indeed, be regarded as a preventive of those fevers. But the moderate cold and heat of the British Isles may act indirectly by favouring atmospheric humidity.”

No accurate observations have been made to determine the relations of these fevers with humidity, but Dr. D. thinks, from an examination of the places where they most prevail, it may be inferred that a cold humid atmosphere favours their production.

In his section on the topographical relations of continued fevers, Dr. D. remarks, that most of our sub-epidemic and sporadic cases of typhous fever, have occurred on rolling or mountainous surfaces, and not on the banks of the larger rivers or the coasts of the northern lakes. As the mineral character of the subjacent rocks afford no explanation of this fact, Dr. D. believes that we must refer to the organic matters which rest upon or mingle with the soil.

“In the mountainous and hilly regions, these are washed away by the rains to be transported and deposited in the lower valleys. Now, those portions of the country *from* which this transportation is made, are most infested with con-

tinued fevers, those to which it is made are least infested—the reverse of which is true of the periodical fevers. Hence, to the east of a median line, drawn parallel to the mountains and the Mississippi, the people suffer but little from periodical fevers—to the west as little from those of a continued type. Here, then, is a species of antagonism, which suggests that the conditions favourable to the production of the former, tend to prevent the latter.”

An interesting section on the physiological and domestic influences productive of continued fevers follows.

In the absence of statistics, Dr. D. expresses the belief that a large majority of those attacked are under thirty years of age. Early life is then a predisposing cause.

“In regard to sex, males are more liable than females, and a greater proportional number of them die. The hospital returns of England, of Ireland, and Scotland, accord with the latter, but reverse the former statement, for more females are admitted than males.”

Whites and blacks appear to be equally liable to continued fevers. Dr. D. is disposed to think that Irish immigrants are, of all the races inhabiting the interior valley, the most liable to their attack.

“It is not uncommon,” he remarks, “to see Irish immigrants taken down with the disease many months after they have reached the interior of the Valley, and the reports of the British army in Canada, give an account of cases so far beyond that of the United States army in the same latitudes, as to suggest a predisposition to continued fever in the royal troops. This may possibly consist in a national peculiarity of constitution, but more probably depends on impressions made on the system before leaving their native country.”

Dr. D. adduces the more favourable conditions of the poor and labouring classes in this country in regard to food, clothing, fuel, and lodging, when compared with that of the same classes in Europe, as one of the probable causes of the less prevalence of continued fever in America than there.

“On the whole,” says Dr. D., “it may be affirmed, that in the Interior Valley, continued or typhous fevers have repeatedly prevailed among those who were adequately nourished with healthy food; but this does not prove that an insufficient or insalubrious diet may not favour the production of these fevers. It would be more logical to say that the diet on which we live is one reason why they prevail so little; and that when they do arise, it is from other causes which our full feeding cannot avert.

“In both Europe and America, the geographical and climatic limits of the continued and typhous fevers are nearly the same with those of the glutinous cerealia and the potato. Some one or more of these plants enter largely into the diet of the whole people, and a single one frequently constitutes nearly the whole sustenance of the poor, who are everywhere the chief victims of these fevers. That the grains of some of the cerealia are liable to deterioration during their growth has long been known. Thus wheat varies greatly in its qualities in different seasons; and sometimes affords a flour which produces nausea and gastric distress, procuring for it the epithet sick wheat. It is also liable to become ergotized, though in a far less degree than rye. The ergot of this grain is well known to possess active, and, in large doses, poisonous properties. The deterioration of the potato during its growth, and even the development in it of a deleterious principle, are matters of history. I refer to these facts, as indicating that the nitrogenized amylaceous grains and tubers may perhaps undergo changes of which as yet we know nothing, rendering them injurious to the health.

“The connection between famine and fever is generally received as a fact. But may not the causes which diminish the quantity of the articles just named, transform some of their normal and nutritive elements into heterologous compounds, and thus combine insalubrity with scarcity, in the production of the typhous fevers? In times of famine, the worst articles of diet inevitably fall

to the poor; and they are the greatest sufferers from the fevers which then arise."

The question of the contagious propagation of continued fevers is very fully discussed. Dr. D. admits that, in various parts of the Interior Valley, sporadic cases of typhous fever occur, which cannot be referred to contagion, and do not propagate themselves by it; yet he supposes that we have also typhous fevers which do propagate themselves in that mode. He also thinks that we may be allowed to give some weight to the almost universal opinion of the medical world, that there are typhous fevers, of an epidemic character, which originate from local causes. Now the question comes up, are there two distinct species of typhous fevers; the one always contagious, the other invariably non-contagious, or can a fever which originates in local causes become contagious?

Dr. D. declares that all attempts to prove the existence of two species of typhous fever, distinguishable by the presence or absence of contagion, have hitherto failed. In favour of the supposition of a fever originating from a miasm or malaria, elaborated in the atmosphere, causing the secretion of a contagion, and thus extending and perpetuating itself, no positive and conclusive facts have been adduced.

In comparing the course of reasoning pursued by Dr. D., and the admissions made by him, in reference to the contagiousness of typhous fever, with his arguments to disprove the contagiousness of yellow fever, there will be found, as it seems to us, some little discrepancy; the points conceded in the one case, as at least probable, being, in the other, rejected as destitute of either positive evidence or probability.

In the section on the classification of continued fevers, we meet with the following remarks in reference to the presumed distinctive characters of typhus and typhoid fevers. After noticing the several cutaneous affections that may occur in continued fever, Dr. D. observes:—

"Now, the celebrated Louis and his followers, including two distinguished American observers, Dr. Jackson and Dr. Gerhard, have seized upon one of the varieties of this cutaneous affection, and under the name of the rose-coloured lenticular spots, endeavoured to make it the sign of a fever specifically distinct from that which presents a different aspect of the skin. I cannot concur in this classification. We do not understand what pathological condition in the continued fevers tends to generate the various dottings of the skin, which a part of the cases present, but I cannot doubt, that it is essentially or specifically the same in all, whether the cutaneous affection appears in the form of rose-coloured capillary congestions, purple extravasations, or miliary exantheas, followed by exfoliation of the cuticle. Indeed, if two species were to be formed by a reference to the maculæ of the skin, they should not be of the kind proposed, but founded on the difference between the rose and purple spots on the one hand, and the pseudo-morbillous rash on the other; for the latter affection manifestly differs more widely from both of them than they do from each other. The difference between a passive, punctate congestion of red blood, in the capillaries, and petechiæ formed by its escape into the cellular tissues, is not as great as that between those conditions and the papulæ which rise so high as to roughen the skin and detach the cuticle. I am far from believing, however, that such a division into species should be made; for in the same epidemic, and even on the same patient, all these forms of maculæ are sometimes commingled. The value of this means of classification is still further reduced by the fact that even in the hospitals of Paris, where the rose-coloured spots have been sought after with keenest vision, they have not been seen in more than three-fourths of the cases, while in the Interior Valley they are seldom seen at all. It has rarely happened to myself to see them in our indigenous fevers; and Dr. Sutton, who has described an epidemic which in other respects corresponds very well with the typhoid affection of Louis, and who made diligent search for them, declares

that they were 'an extremely rare occurrence.' On the whole, we may say that as yet the attempt to divide the continued fevers into species, that shall be designated by the appearance of the skin, has been a failure."

In relation to the division of continued fevers into species, by pathological anatomy, Dr. D. remarks:—

"In constructing species, we must have reference both to the symptoms and the structural lesions. There must be a constancy of connection between them, as there is between loss of muscular power, insensibility, coma, slow pulse, and stertorous breathing in apoplexy, and the sanguineous engorgement or extravasations in the brain, as found on dissection after death, or the hard and frequent pulse, hot skin, headache, contracted pupil, delirium, and coma vigil, of cerebritis, and the capillary hyperæmia, lymphatic adhesions, and serous effusions revealed by autopsy.

"Now we may take it as a fact that, in continued fevers, this association of symptoms and lesions is far more loose and uncertain than in the diseases which have been named; for when the cerebral symptoms have been well developed, the brain after death has often presented but few lesions, and the abdominal lesions have quite as often falsified the predictions made from the symptoms during life. Moreover, the organs of both cavities are often found in a state of lesion in the same subject, while in different members of one family, in the same epidemic, the chief lesions have been found in the head of one victim, in the abdomen of another. It is equally well known that the lesions in continued fever are not limited to the cavities which have been named, but are found likewise in the chest, and these are so frequent that if we constitute as species the other forms, we might consistently establish a third, the thoracic or pulmonary. We should then have the cerebral, or typhus mitior of the British writers, the abdominal typhus, or typhoid affection of the French and German, and the pneumonia typhoides of the American. These designations are founded in nature and may be retained, but they should be understood to express only varieties or modifications of a single species. In certain countries or classes of people, one variety may prevail for a time, in others, another; then, again, all may be present in the same time or family. Even the whole of these lesions are not unfrequently found in a single *post-mortem* examination. The truth is, that we must admit a constitutional, typhous diathesis, with varying localizations; and not specifically distinct, primary local inflammations, generating fever, as in the phlegmasiæ. If this be admitted, it follows as a corollary, that the labours of the Parisian school, have simply made us acquainted with a form of typhus not peculiar to their city, nor ever epidemic there, but sporadically prevalent among them for some time past; not absolutely, but relatively to other forms, more prevalent in that city than elsewhere."

In the ensuing two chapters, the symptoms of continued fever, throughout the period of incubation, the stages of development, of excitement, and of innervation; the particular symptoms in the digestive system and respiratory apparatus, the state of the calorific function, the hemorrhagic and petechial phenomena, with the progress and termination of the disease, are minutely detailed.

The pathological anatomy of typhous fevers is given in Chapter XIII.; it is, as the editors remark, imperfect; evidently not having been finished for publication by the author, and much of the original manuscript being probably lost or mislaid.

The very able chapter on the pathology of typhous fever will be read with deep interest and instruction. It does not admit of an analysis, adapted to the limits within which we are necessarily restricted, that would do full justice to the views of the author, and the manner in which they are developed by him:

We must pass over the chapter on the treatment of typhous fever, occupying some forty-eight pages. It is mainly occupied with an examination of the

several plans pursued and remedies recommended by the physicians practising at different points within the Interior Valley, with judicious remarks as to their relative value, and their adaptedness to meet the leading indications of cure, based on the pathology and general tendencies of the disease.

An interesting but incomplete chapter, a considerable portion of it not having been written out by the author, follows, on the relations of typhous fevers with yellow, remittent, and other febrile diseases, on secondary typhus, and the typhoid stage. In it Dr. D. presents the differences and identities or analogies between yellow and typhous fevers, and remittent, autumnal, and typhous fevers, as follows:—

"Yellow and Typhous Fevers. I. Differences.—1. Yellow fever is a disease of hot climates, and in our Valley has not yet ascended beyond the 35th degree of latitude, where the mean heat of summer is about 80°, and that of the year 70°.

"The typhous fevers prevail more in the higher than the lower latitudes, and, as indigenous diseases, are almost unknown where the heat of the year and the summer rises above the degrees just given.

"2. Yellow fever, in our Valley, prevails most on low levels near the sea, is littoral, and has not occurred at an elevation of more than 400 feet.

"The typhous fevers are essentially continental, and prevail in the Appalachian Mountains, and those of the territory of Santa Fe.

"3. Yellow fever occurs almost exclusively in our cities and towns; typhous fever, on the other hand, prevails equally in town and country.

"4. Yellow fever generally occurs as an epidemic, and in the northern portion of its zone is never sporadic; typhous fevers, on the contrary, are both epidemic and sporadic in every region which they infest.

"Yellow fever within our Valley scarcely ever sets in till after the summer solstice, and as constantly ceases before the winter, being at its height about the autumnal equinox. Typhous fevers occur sporadically throughout the year, and have been epidemic in every season.

"6. The onset of yellow fever is generally sudden and violent; that of typhous fevers slow and insidious. The ordinary duration of one is five or seven days; of the other, three times that period.

"7. A cutaneous maculation or efflorescence is a frequent event in typhous fevers, but is absent in yellow fever.

"8. Prolonged yellow fever is almost unknown, but a short and fatal typhous fever is not very uncommon; it does not, however, display the symptoms characteristic of yellow fever.

"9. In the closing stage of yellow fever there is suppression of urine; in that of typhous, retention, with broad exceptions to both.

"10. Typhous fevers are sometimes contagious; yellow fever never.

"11. The closing stage of yellow fever is attended with the black vomit; that of typhous not. The closing stage of yellow fever is generally unattended with delirium; that of the typhous fever always with delirium or impassibility—abolition of intellect.

"12. The most striking morbid appearances in yellow fever are found in the stomach, duodenum, and liver; in the typhous fevers, in the brain, lungs, ileum, and spleen.

"13. Yellow fever is followed by quick and perfect recovery; typhous fevers have a slow and imperfect convalescence.

"II. Identities or analogies.—Both are continued fevers; both are accompanied with hemorrhages in the latter stages; both confer an imperfect and uncertain immunity from a second attack; both originate and become epidemic in certain, but not the same, localities; both appear at times to be introduced; both originate in ships at sea, but not often when they are sailing in the same latitudes; finally, both seem to run, respectively, a destined course, in despite of medical interference."

"Remittent, Autumnal, and Typhous Fevers. I. Diversities.—1. A mean annual temperature below 40°, with a summer mean heat below 60°, extinguishes remit-

tent autumnal fever, but is not incompatible with a general prevalence of typhous fevers. The former is epidemic only in autumn; the latter, in every season.

"2. Extensive dryness of surface, the absence of decomposable organic matter, and circumstances characterizing the densely populated portions of our larger cities, greatly diminish, or prevent autumnal fever, but not typhous.

"3. Typhous fevers are sometimes contagious, sometimes introduced; autumnal remittents never.

"4. The natural termination of remittent fever is into intermittents; but not so with typhous.

"5. One attack of remittent fever predisposes to another; but it is otherwise with typhous.

"6. The copious secretion of bile and derangements of the biliary function, so characteristic of remittent fever, are absent in typhous.

"7. The maculæ in typhous do not occur in remittent fever, and hemorrhages are rarer.

"8. The coma, subsultus, sordes of the teeth, and dry red tongue of typhous are not necessarily present in remittent fever.

"9. When both assume a malignant character, and prove fatal within a few days, the assemblage and succession of symptoms are not the same.

"10. The sulphate of quinia arrests remittent, but not typhous.

"11. The pathological appearances in remittent fever are mainly in the stomach, duodenum, spleen, and liver; those of the typhous fevers more in the ileum, brain, lungs, and spleen; though there are many exceptions to both rules.

"II. *Resemblances or identities.*—In many cases, the access or forming stage is nearly of the same length in both; and, sliding into an intermittent type, the remittent may endure as long as a protracted typhous fever.

"1. Both may have a full epidemic development in the same climates, whether on land or sea.

"2. In the early periods of the stage of excitement the remittent may approach so closely to a continued type that the two fevers may be confounded.

"3. Both attack males more than females.

"4. When remittent fever terminates fatally in one or two weeks, a certain amount of subsultus, a dryness of the tongue, and intestinal hemorrhage are sometimes present, although no typhous fever may be prevailing in that locality; and this brings us to inquire, not into the distinctive peculiarities of these two forms of fever, but into their combination, into the hybrid or mongrel diathesis which results from the joint impress, in ever-varying proportions, of the causes which produce true typhous and true remittent fevers."

This inquiry Dr. D. pursues in the next section, where he describes this hybrid fever under the denomination of "*Remitto-Typhous*, or *Secondary Typhous Fever*."

Part IV. of the volume before us is devoted to the consideration of the "*Eruptive Fevers*, as they occur in the Interior Valley."

Under the head of "*Vaccination as a Prophylactic of Smallpox*," Dr. D. enumerates the leading causes that have conspired to prevent its universal or very general practice throughout the population of the Valley.

He expresses his firm belief in the prophylactic efficiency of vaccination, and presents some judicious suggestions as to the means best calculated to render the practice of it more general. They are as follows:—

"1. As the militia are enrolled for the common defence, and as the smallpox, introduced into an army liable to it, might at any time defeat the object for which it was taken into the field, it would seem to be not inconsistent with the principles of constitutional liberty to require that all should be vaccinated. 2. Wherever public schools are established, it might be required, as a condition to the admission of children of both sexes, that they shall have been vaccinated. 3. All charters for academies, colleges, and universities might require the same condition. 4. To aid in the execution of such laws, it might be enacted that

the guardians of the poor, and the governors of all hospitals authorized by the States, should cause gratuitous vaccination to be performed in all cases where, through poverty, it might be demanded.

"It may be asked, to what good purpose would these regulations tend, if individuals are liable to smallpox after vaccination? I answer that, since the discovery of the vaccine disease, smallpox remains a scourge of society, not because the former is a fallible preventive, but because it is not universally employed. If all who are born were carefully vaccinated, smallpox would *probably* disappear, and certainly lose its mortal character—a result that should satisfy us.

"There are some, disheartened by the occasional occurrence of smallpox in the vaccinated, who would return to inoculation, as affording a more infallible protection; but against this backsliding there are insuperable objections, for—
1. Inoculation, although a better protection than vaccination, is not infallible; and the mortality among those who subsequently contract smallpox is greater than that among the vaccinated. 2. Smallpox from inoculation is, on the whole, a more fatal disease than smallpox following vaccination. 3. The practice of inoculation tends to keep alive in the world the disease which it is the object of vaccination to annihilate by depriving it of subjects."

The histories of the eruptive fevers, as given by Dr. D., although restricted to these diseases as they occur within the Interior Valley, based upon the personal observations and experience of the author and the recorded observations of physicians practising in the different sections of the Valley, present a mass of valuable and instructive facts, adapted to throw no little light upon the true pathology and appropriate treatment of the several febrile exanthemata.

The chapter on erysipelas, especially in its epidemic form, is particularly interesting. The question of the contagiousness of epidemic erysipelas is examined at some length. After adducing the evidence bearing directly upon it, Dr. D. remarks:—

"The facts presented in this section seem to me to place epidemic erysipelas in the same category with the typhous fevers. Originating sometimes, if not always, from some other cause than contagion, yet in many instances spreading by that means."

At the close of the section which treats of the symptoms of epidemic erysipelas, Dr. D. makes the following important remark, which is fully borne out by the almost universal tenor of the accounts that have been published of the disease, as it has occurred in different portions of the United States:—

"The peritoneum in men and non-parturient women was obnoxious to the inflammation, but not in as high a degree as the pleura. Pregnant, and especially lying-in females, were, however, peculiarly liable, and the most fatal cases were the puerperal. I say nothing, however, on the history of these cases here, as they can be best studied under the head of 'Puerperal Fever.'"

Puerperal fever, agreeably to the classification adopted by Dr. D., belongs to a chapter of his work which, unfortunately, he was not spared to write.

The fifth part of the present volume is devoted to a consideration of the Phlogistic Fevers—the Phlegmasiæ.

The subjects discussed in the first five chapters are, a comparison of the phlogistic fevers with the previous groups; their classification, etiology, pathological and modifying causes; the rise and establishment of the simple or common phlegmasiæ; the state of the blood in them; their progress, terminations, and anatomical lesions; and the indications and means of cure adapted to conduct them to a favourable termination.

These chapters constitute a most admirable introduction to the study of the individual diseases included in the general group. The whole of the subjects

embraced in them are treated of with great ability by the author. The facts and suggestions he has presented will be read with deep interest by the American physician, who cannot fail to derive from them much valuable instruction.

The first of the phlegmasiæ whose histories are presented, are those of the central organs of innervation, the brain and spinal cord, with their membranes.

It was not our intention, when we sat down for the purpose of presenting a notice of the work before us, to enter into any discussion as to the correctness of the views advanced by the author, either pathological or therapeutical. It is proper, however, to remark, in reference to the section presenting an anatomico-physiological introduction to the history of the phlegmasiæ of the brain, etc., that we entirely agree with the able editors of the work in the opinion expressed by them in a note appended to this section, that "the progress of experimental physiology has rendered it necessary that some of the positions of the lamented author" advanced in it "should be modified."

Dr. D. maintains that there can never occur either increase or decrease in the quantity of blood in the brain; consequently, that the latter can never suffer compression from hyperæmia. But, as his editors correctly remark, "it may be advanced that, even when the amount of blood in the vessels of the brain is unchanged, the pressure upon their walls may vary considerably. Moreover, the quantity of cerebro-spinal fluid is variable, and this, taken together with the softness of the cerebral substance, would admit of considerable variations in the quantity of blood contained in the cerebral vessels, variations necessary, as it would seem, to the ever-changing functional activity of the cranial viscus, or at least bearing a direct relation to that activity. Indeed, it will be found that the author himself, in the next chapter, leans to this view of the case, and even mentions experiments of his own as corroborating it."

In treating of the prevalence and causes of the phlegmasiæ of the brain and spinal cord, Dr. D. remarks:—

"While thoracic inflammations prevail more in the North, and abdominal in the South, the cerebral appear to occur with considerable uniformity in all our latitudes; showing that, of the whole, they have least connection with climate. My own observations lead to the conclusion that they are more frequent in town than country; but as the *supposed* causes abound in the former more than in the latter, the mind is, of course, busied in favour of a conclusion, which, until established by statistics, should not be regarded as final."

The following positions, as a mere statement of occurrences, may be received as correct; some of them are stated, however, in language involving certain pathological doctrines to which not a few of Dr. D.'s readers will refuse their assent. Of secondary cerebritis the author remarks:—

"1. It is frequently developed, as we have seen, in the course of our typhous, exanthematous, erysipelatous, *yellow*, and autumnal remittent fevers, when it generally proves fatal.

"2. In the form of duramateritis, it occurs as a metastasis of rheumatism from the joints; a more manageable case than those just mentioned.

"3. Active or eccentric hypertrophy of the heart, by over-injecting the brain, frequently originates inflammation.

"4. In advanced stages of phthisis it is not uncommon; and I have seen a fatal case which supervened on extensive tuberculization of the lungs, before the suppurative stage had set in. In these cases, there is doubtless a deposit of tubercles in the brain; and they may be classed with the cases of original tubercular cerebritis (or incurable hydrocephalus) of children.

"5. I have seen many metastases of inflammatory cholera infantum to the brain. It seems probable that the organ was tubercular; for they have gene-

rally proved refractory and fatal. Sometimes they begin almost as soon as the gastric affection. In other cases they arise insidiously and slowly, the cholera having become chronic. I have not seen the cholera morbus of adults undergo this metastasis.

"6. The cerebritis following on the collapse of epidemic cholera, is but too well known to us all.

"7. Apoplexy is often followed by inflammation, especially when hemiplegia has been induced; and its superadded lesions are doubtless one of the causes which render the latter infirmity protracted, and occasion the imbecility so often associated with it.

"8. Inflammation of the dura mater and its associated arachnoid membrane sometimes follows on otitis, and suppuration of the internal ear.

"9. Cerebral inflammation has followed on the drying up of ulcers of the scalp. There was probably an extension of inflammation to parts within, and a consequent cessation of that without. But the current opinion in the profession that powerful applications, in certain chronic diseases of the scalp, are followed by metastasis, deserves to be respected."

Very excellent accounts are given of cerebritis, cerebro-meningitis, chronic encephalitis, tubercular encephalitis, myelitis, and cerebro-spinal meningitis.

The section devoted to tubercular encephalitis is the least full in reference to symptomatology and pathological anatomy, than to any of the other particulars included by the author in his, confessedly, imperfect account of this disease of childhood; nevertheless, the section will be found replete with important interest.

We agree with Dr. D. in the following remark as to the part the tubercular diathesis plays in this form of cerebral inflammation:—

"It is, I think, undeniable that this diathesis gives a predisposition to inflammation, and that children having it are more obnoxious to various causes of inflammation than those from whom it is absent. Thus it plays a twofold part—inviting that form of morbid action, and also rendering it more difficult of cure."

We cannot, however, coincide in opinion with the author when he adds, that—

"With a tubercular diathesis, this form of inflammation may be developed without the agency of any exciting cause, and is then said to arise spontaneously. It seems probable, however, that in such cases tubercular deposits have gradually taken place in the brain or its membranes, and, as in the lungs, irritate the tissues into inflammation."

We doubt very much the production of inflammation, in any instance, in either the brain or lungs, simply from the deposit of tubercular matter.

Rheumatism and ophthalmia are the subjects of Chapter X., both of which diseases are treated with great ability.

The next chapter is devoted to the consideration of the phlegmasiæ of the respiratory organs.

Dr. D. is strongly in favour of an active antiphlogistic treatment in croup. We present all that he says as to the method of cure in this disease. It will be perceived that he is entirely silent in regard to the subject of local applications to the throat.

"When by slow ingravescence or a sudden invasion the disease has taken on the character of a phlegmasia, an active treatment is imperatively demanded." "The treatment, in this stage of the disease, should be antiphlogistic, and whenever the fever is developed, the first remedy is bloodletting. In children of a year old, the application of two, three, or four leeches to the larynx and trachea, down to the sternum, will be sufficient. In want of these, scarifications and small cups may be applied to the nucha and the region of the larynx. If this

be not practicable, venesection should be attempted, and from one to two ounces of blood drawn. At a more advanced stage (age?) that mode of depletion is preferable to the others, and without reference to quantity, the flow of blood should be continued till perspiration of the face, yawning, or nausea, indicates the approach of syncope, beyond which it should never be carried. *Such a bleeding ought never to be repeated, and we should seldom resort to the lancet a second time, unless there be a conjoined bronchitis or pneumonia.* Leeching or cupping may, however, be employed when a repetition of the phlebotomy would not be proper. Immediately after the bleeding, the child may be immersed in a tepid bath, where it should remain from fifteen to thirty minutes, a much longer time than is usually thought necessary. Instead of *lying* buried up in the water, it should stand or sit in the tub, and have the water poured over its body by two or three assistants working at the same time. This method is much more soothing and sedative than immersion." "While this is going on, an emetic should be given. Of any medicine, tartarized antimony is the best, and most easily administered. Full vomiting should be effected, and after the operation the little patient may be moderately covered, and, provided the room be not too hot, allowed to rest. In many cases, convalescence now begins, and advances with free catarrhal secretion. Sometimes, however, the child remains anxious and restless, with dyspnœa, when a portion of laudanum and calomel should be administered, under the action of which, his nervous irritability will be allayed, while the calomel, by its action on the liver and bowels, will produce or prepare the way for those alvine evacuations which will carry off the existing contents of the digestive canal and make salutary revulsion from the inflamed organ. If this simple but energetic treatment should not arrest the disease, the fever will soon begin to rise, and its intensity will show the violence of the inflammation. Without recurring to what has been said on the repetition of bloodletting, I may now insist, with the best authorities, that the continued administration of tartarized antimony and calomel is our great resource. The former may be given in nauseating doses, even to occasional vomiting; and the latter in one, two, or three grain doses, according to the age of the child, every two hours; and both must be continued till the fever abates, or the signs of exhaustion or carbonization of the blood appear. A good vehicle for the tart. ant. is a syrup of polygala senega, the *modus operandi* of which was first pointed out by Dr. Archer. If the child have great restlessness, tepid bathing may be again employed, and small doses of laudanum or paregoric be administered. If the medicines should not act on the bowels, a gummy, oleo saccharum (?) should be administered, which, in the mucous inflammations of children, generally has a soothing and happy effect. If the bowels should be too active, and especially if they should throw off watery discharges, laudanum should be given. Blisters have been both commended and condemned. I have certainly seen them useful. They should be applied over the lower cervical vertebræ, or over the trachea below the larynx. They should be removed as soon as vesication begins, and the surface covered with an emollient poultice.

"In more advanced stages but little can be done. The spasmodic action of the glottis may be moderated by an emulsion of assafetida and laudanum; and to promote the expectoration of the false membrane, a strong infusion of the senega root may be administered. The dyspnœa still continuing, with a haggard expression of countenance, and the appearance of a smoky hue of the lips, the question of tracheotomy comes up. The point then to be decided, is, whether bronchitis or pneumonia has disorganized the lungs, and thus contributed to the asphyxia which is now going on. If they have not, there is no objection to the operation; but I confess that, in this country, I have no great reliance upon it as a resource in such an emergency, seeing that parents generally oppose it, and that the greater number of our physicians are not fully prepared for its performance under the difficult circumstances which then exist."

One of the most interesting chapters in the present volume, is that on tubercular pneumonitis, or pulmonary consumption. By no means as complete as could be desired in respect to the pathology and morbid anatomy of

the disease, it is, nevertheless, replete with important facts and researches bearing upon many important points of its etiology—without correct views of which no successful prophylaxis of tubercular consumption can be established. And in a disease where medical treatment avails so little to arrest its fatal course, the means of prevention become of paramount importance.

It was for a long time supposed by the members of our profession, as well as by the public generally, that consumption is the especial endemic of cold and variable climates, while it is of rare occurrence in warm and equable climates. From the investigations of Dr. D., it is shown, as it had already been by other authorities, 1. That consumption prevails in every climate, cold, warm, and temperate. In those marked by great equability, as in those noted for great variability of temperature. 2. That, comparing the extreme northern and southern climates, the prevalence of consumption in the latter, will be found to exceed that in the former. 3. That, in regions where periodical fevers prevail, consumption is also a prevalent disease. 4. That a hot and humid climate is more favourable to the production of consumption, than one very cold, or even cold and humid. 5. That throughout every portion of the temperate zone, consumption prevails without our being able to determine, from the statistics at our command, the relative agency of the local modifications of climate this zone presents, and the different natural and social conditions to which it gives birth, in the production of the disease.

“From all that has been said,” Dr. D. remarks, “it results that the opinion long current in the profession, and never till lately called in question, that a tubercular diathesis owes its existence mainly to cold climates, is an error. On the other hand, as we have seen, it also prevails, *cæteris paribus*, less in cold than hot climates. But if this be not insisted on, more cannot at most be conceded than as great a prevalence in one as the other, and, therefore, we can no longer call it a disease of the north, or connect it with degrees of latitude or isothermal curves. The hygienic rule which results from this conclusion, is equally obvious and important. He who has a tubercular predisposition, or labours under consumption in any stage, should *not* seek a warmer climate in winter, but a colder in summer; and if he lives far in the south, it might be well for him to remain in a colder climate throughout the winter. Happily, our Interior Valley abounds in eligible retreats for carrying out this requisition. Voyages to the Falls of St. Anthony, with a summer sojourn in Minnesota, now a settled region; a residence on the island of Mackinac, with excursions into Lake Superior; voyages down the St. Lawrence to the cool and wild scenery of the Saguenay; and the Chataouque Mountain platform, at the sources of the Alleghany River, offer as attractive a variety as can be afforded by any country on earth; and it should be recollected, in reference to the lake country, that our army returns present it as more exempt from consumption than the Mediterranean, where one soldier out of every three hundred and eleven dies annually of that disease, while only one out of five hundred and twenty-six dies of it at our lake military stations. In addition to these reports, the consumptives of Florida and Alabama, who may not be able to wander far from home, have a good resource in the beautiful valley of the French Broad, Buncombe County, North Carolina, where, at the elevation of more than two thousand feet, they can enjoy the comparatively cool and invigorating influence of a climate, which, I was assured by its physicians, is but little infested by consumption.

“What has been said, implies a general recommendation to the consumptive and the predisposed, to remain at home in winter; but it equally implies that they should not confine themselves to hot rooms; but, contrariwise, should daily seek the open air. If they cannot bear it without cough and difficulty of breathing, it is because the tuberculation of their lungs has made great advances, although suppurative inflammation may not yet have been set up. That, however, is *inevitable*, and, when established, will advance to a fatal termination, the more rapidly the longer and more enervating may have been their confinement in hot apartments.

"Thus far I have spoken of general climate, and must now refer to local, which is in most cases quite artificial. When a tubercular diathesis is forming under such circumstances, as, for example, in close villages, or dense and damp forests, or in alleys in the depth of cities, or in workshops and factories, where the air is confined, damp, and vitiated from the presence of a great number of operatives or the employment itself, a change of local climate, only to be effected by change of place, is an indispensable condition to the arrest or retardation of the disease; and in making it, the patient should seek a fresh, pure, and invigorating atmosphere, cooler, instead of warmer, than the one from which he escapes."

We have already extended this notice to too great a length to allow of our following the author through the two remaining sections devoted to the consideration of the etiology of consumption. His judicious remarks on the influence upon the development of the disease of insufficient exercise, in-door and sedentary occupations, damp and unventilated habitations, bad clothing, bad or poor diet, alcoholic drinks; crowded, ill-ventilated and over-heated workshops; occupations which throw the body into constrained or unnatural postures, and the constantly breathing an air loaded with minute particles of animal, mineral, or vegetable substances, will be read by every physician with deep interest and equal profit.

The twentieth and concluding chapter of the present volume is devoted to cardiac inflammation. Valuable as is, unquestionably, this portion of the work, as far as it has been written out by the author, it possesses, nevertheless, less of interest than most of the preceding portions; not from the inferior importance of the affections treated of, but from its presenting less of the features of originality which are displayed by the others; fewer evidences of its being based upon the observations and experience of our own practitioners. It is, unfortunately, also, incomplete—the entire subject of the organic diseases of the heart, though referred to by the author, never having been written by him.

The volume before us fully sustains the high reputation conferred upon Dr. Drake, by the publication of the first volume of the work. It redoubles our regret that he could not have been spared to complete it, and thus leave a more perfect monument to his own memory, as a cautious and philosophic investigator of the etiology and character of the diseases prevalent in one of the most important regions of the Western Continent, and, at the same time, a most valuable contribution towards the establishment of a medical literature strictly American.

The histories of the several diseases embraced in the present volume are not all of equal fulness and completeness. Some evidently want the final revision, which the author would have bestowed upon them had he lived; while to others it was, no doubt, his intention to have made important additions, and to have given to the portions prepared by him, greater extension. We are thankful, however, that the editors have given it to us as it is. Incomplete though it be in some of its parts, it nevertheless furnishes an invaluable mass of facts and observations, carefully analyzed, and systematically arranged, from which the American practitioner cannot fail to derive an increased acquaintance with the etiology, pathology, and therapeutic management of some of the more prominent diseases of his own continent.

D. F. C.

ART. XV.—*A Practical Treatise on Foreign Bodies in the Air-passages.*

By S. D. GROSS, M. D., Professor of Surgery in the University of Louisville; Member of the American Philosophical Society; Author of "Elements of Pathological Anatomy," "A Treatise on the Diseases of the Urinary Organs," &c. With Illustrations. 8vo. pp. 468. Philadelphia: Blanchard & Lea.

BUT few subjects in the range of medicine or surgery are more important than that of foreign bodies in the air-passages; and yet, strange to say, there is scarcely a treatise or monograph, up to the present time, in which this subject has received that attention and investigation which it demands. It is to fill this vacancy in the history of our science that the labours of Dr. Gross have been especially directed upon the present occasion. In his preface, he states that his "design originally was, not to write a book, but to compose a short monograph for some medical journal." The author, however, had not proceeded far in his investigations before he found that the subject was far more comprehensive than he had at first imagined, and that, to do it anything like justice, it would not only require more space, but also much more time and study than anticipated. And, in fact, it must be admitted that it has been no easy task to collect, analyze, compare, and, finally, deduce correct general principles from the scattered facts throughout our various periodicals, upon this important subject. The result has been a very respectable volume, consisting of four hundred and sixty-eight pages, and containing, besides much other very interesting matter, some fifty cases of the accident now under consideration, which for the first time meet the public eye. Besides this, an abstract of upwards of two hundred cases, extracted from the medical press of this and other countries, is contained in this work, and forms, as the author most justly remarks, "one of its most important elements."

When we bear in mind the frequency of the accident, the helpless age at which it most commonly occurs, the agonizing suffering of the patient, and the prompt and decided action required to prevent impending death, it is not surprising that the author has devoted so much zeal and industry to the investigation of this subject. He admits that reflections of this character impelled him to write this treatise, and adds, that "if, in the providence of God, the work shall be instrumental in saving the life of one human being, or even in ameliorating the suffering of a single individual," he will feel himself amply remunerated for the time and toil bestowed upon its composition.

The first chapter is devoted to the general consideration of the character of the bodies which may enter the air-passages, the alterations they may undergo during their sojourn in these parts, the situations they most generally occupy, and, finally, the various routes by which they gain admission or may be expelled. The substances may consist of almost every conceivable form, from the vegetable, animal, and mineral kingdoms; but these, of course, are not alike liable to undergo rapid and important alterations from the effects of heat and moisture, which they necessarily encounter in the air-passages. Pills, particles of sugar, or crumbs of bread may readily undergo complete or partial solution, and thus be expelled, or, at most, induce but temporary inconvenience; but Dr. Gross very properly condemns the absurd notions, entertained by some, as to the softening power of the air-passages upon hard substances, such as grains of corn, melon-seed, beans, and even cherry-stones—substances which, indeed, almost defy the solvent powers of the gastric

juice. He points out the great danger of delay from trusting to such unfounded expectations, and states that "we might as well wait for the softening and disintegration of the Rock of Gibraltar by the waters of the Atlantic and Mediterranean, as for the softening and disintegration of a persimmon-stone or a piece of gristle by the heat and moisture of the air-passages;" a strong expression, but perhaps not too much so for so absurd a notion. In this chapter he alludes to a case in which a grain of maize, and to another in which a grain of corn exhibited signs of germination when removed from the lungs. It is well known that most vegetable substances greatly increase in bulk from remaining long in contact with the mucous membrane, and thus greatly diminish the chance of their being expelled through the glottis. A remarkable exception to this rule was recently observed by the writer of this notice, in the case of a little child who had a coffee-grain in the air-passages for more than three years, without its having undergone any perceptible alteration, either in bulk or consistency. Although mineral or other hard substances are not liable to any increased bulk from the above causes, yet it is well known that, when long retained, they are often coated with inspissated mucus, or even incrustated with carbonate and phosphate of lime.

In regard to the situation which foreign bodies most commonly occupy, the researches of the author fully confirm the generally received opinion that it is in the right bronchus. Dr. Stokes, in his short monograph upon this subject, alludes to the fact, as first pointed out by Dr. Goodall, of Dublin, that the septum or ridge which separates the right from the left bronchus, being to the left of the mesian line, necessarily tends to direct all foreign substances into the right. This effect is no doubt promoted by the larger dimensions of the right tube, but not, as has been supposed, by its comparatively horizontal position, since, in reality, the descending body would naturally tend to enter the left, which is most vertical. A small and smooth body, particularly if it be at the same time heavy, as a bullet, pebble, or shot, will be much more likely to descend into the bronchus than one which is light and large, or rough; and Dr. Gross expresses the opinion that many cases of sudden death, which, by a coroner's inquest, have been set down—"died by the visitation of God," have, in reality, been owing to the impaction of some particle of food in the very "door of the windpipe." Although, as a general rule, the foreign body obtains admission through the chink of the glottis, yet occasionally they have ulcerated their way through from the pharynx. An instance of this kind occurred quite recently in the wards of the Pennsylvania Hospital, in the case of a man who had several false teeth lodged in the pharynx, and which finally produced an ulcerated opening into the larynx. At other times, the substance may enter through a penetrating wound of the neck. Dr. Gross also alludes to cases in which the foreign body, having passed into the throat, excited ulceration of the lung, and was finally discharged through a fistulous opening between the ribs.

Allusion is likewise made to numerous cases of sudden death from the impaction of bodies upon the rima of the glottis. "Diseases of the epiglottis, disqualifying it for the due performance of its functions, remarkably predispose to this occurrence." "A person labouring under delirium tremens, and confined so as to be unable to move, may, in an effort at vomiting, instantaneously perish from the introduction of food into the air-passages." (p. 60.) But even where the "immediate effects of foreign bodies" (which forms the subject-matter of Chapter II.) are not so unfavourable, the author proceeds, in the third chapter, to the consideration of the remote or "pathological effects;" and here shows that, although the dangers are not so imminent, they

are nevertheless very urgent. The most usual of these are softening, thickening, and ulceration of the mucous membrane; and, at a more advanced period, pneumonia, pulmonary abscess, pleuritis, tubercle, &c. "It is a singular fact that the pathological changes now enumerated may all occur, to a greater or less extent, in cases where the obstruction is seated, not in the lungs or bronchial tubes, but in the larynx or upper part of the trachea."

Chapters IV. and V. are devoted to the consideration of the symptoms and diagnosis of this accident, and certainly constitute one of the most interesting parts of the work. The introduction of the body, in nearly every instance, gives rise to a severe paroxysmal cough, with impending suffocation; this is followed by a deceitful calm, lasting for several hours, or even an entire day; but, sooner or later, the symptoms recur even with increased intensity. When the case becomes more chronic, a new train of symptoms set in; the cough is less violent, though readily excited by the least motion; the expectoration becomes purulent; and, finally, hectic fever and general impairment of health supervene. By some, much importance has been attached to the sensation of pain as indicating the locality of the foreign body; upon this point, Dr. Gross remarks:—

"The pain does not always follow the introduction and lodgement of a foreign body in the air-passages, and that, when present, it varies much in degree as well as in character. Generally speaking, it is very slight, while sometimes it is excessively violent. It may come on at the moment of the accident, or it may not show itself until some time after, perhaps not until the resulting inflammation has produced serious structural lesions."

As this symptom is sometimes entirely absent, and inasmuch as, when present, it may depend upon so many other causes, it is generally admitted that it is of but little or no importance in a diagnostic point of view. A peculiar "flapping noise" has occasionally been heard, both by the patient and physician, in cases where the foreign body has been movable, occasionally ascending into the larynx, and again descending into the bronchial tubes; but similar sounds may be produced by the presence of inspissated mucus, or pieces of false membrane floating up and down the air-passages, and causing a "flip-flap" sound, which could not well be distinguished from that produced by a movable foreign body. Confessedly one of the most important means of forming a correct diagnosis is that derived from auscultation and percussion; all the others may entirely, or in a great measure, fail. Dr. Gross, indeed, mentions several interesting cases, where, from the absence of well-marked general symptoms, no suspicion of the real nature of the case had been entertained, and was only revealed by an examination *post mortem*. As a general rule, when the body is smooth and round, it will more or less completely obstruct one of the larger or smaller bronchial tubes; and, as a necessary consequence, the respiratory murmur will either be entirely destroyed, or greatly diminished throughout the portion of lung supplied by the obstructed tube. As a result of this, the respiration will be puerile or supplementary throughout the rest of the lung, or even upon the opposite side. When, during a violent paroxysm of cough, the body is driven from the bronchus, the murmur is at once restored, whilst, at the same time, violent spasm and dyspnoea are induced by the irritating substance impinging against the more sensitive membrane of the larynx. Now, it is precisely this alternation of symptoms which proves most characteristic of the accident under consideration. It should not, however, be concealed that many of the above signs may be owing to the plugging of a bronchial tube by a mass of inspissated mucus or lymph, as sometimes happens in "membranous bronchitis." It is also important to bear in mind that, when the body is of an

irregular shape, it will still allow the passage of air, and thus not perceptibly interfere with the respiratory murmur; and, for the same reason, such a body will not be displaced by a paroxysm of coughing, however violent. It will, therefore, be perceived that even the physical signs will occasionally fail to enlighten us upon the real nature of the case; generally speaking, however, in connection with the previous history, they will prove a valuable aid in forming a correct diagnosis. Percussion, although it affords less important indications, still merits our attention. In recent cases, the sound is always clear at first, and that even when the tube is completely obstructed; under the last-mentioned circumstances, the portion of lung from which the air is cut off becomes ultimately collapsed or condensed from an accumulation of the natural secretions, and congested condition of the mucous membrane; the percussion is now necessarily flat over this part of the lung. These various circumstances are fully dwelt upon by the author; but, as it would be impossible to follow him more closely in this short notice of his work, we must conclude this part of the subject by a single quotation, illustrating the manner in which the subject is treated:—

“When the foreign body descends below the larynx, it is usually arrested in one of the bronchial tubes—more frequently the right than the left. Under such circumstances, the respiratory murmur in the corresponding lung is generally more or less affected, as is evinced both by percussion and auscultation. The wall of the chest, however, is not, as might be supposed, always, perhaps not even generally, dull or flat, as in pneumonia and phthisis, in which the parenchymatous substance of the organ is condensed by abnormal deposits; on the contrary, the sound is frequently unnaturally clear and resonant, very much, indeed, as in pulmonary emphysema. This peculiarity is sometimes recognized over the entire lung; while at other times it is limited to particular portions, as one-half, a third, or one-fourth, according to the size and situation of the foreign body. When the extraneous substance is so large as to obstruct the bronchial tube completely, there must necessarily be marked dulness on percussion on the corresponding side of the chest, and great diminution, if not entire absence, of motion in the ribs, as I have repeatedly witnessed in the human subject, and also in my experiments on the inferior animals.”

From a careful analysis of the cases which have fallen under his observation, he concludes that when aphonia exists in connection with pain and tenderness over the larynx, and a peculiar whistling sound in respiration, there can be but little doubt that the foreign body is in the larynx. Allusion is also made to the danger of confounding the symptoms resulting from the lodgement of a foreign body in the pharynx with those produced by a similar accident affecting the windpipe; and mention is made of a surgeon, as reported by Desault, who, after opening the trachea, found, upon the death of the patient, that the offending body had been impacted in the pharynx. The rest of this chapter is devoted to the consideration of the possibility of aneurisms of the aorta, pertussis, spasm of the glottis, croup, &c. being mistaken for the lodgement of extraneous substances in the air-passages; but, as the means of diagnosis in these affections are generally so apparent, it will not be necessary to dilate upon these points.

Under the head of “Spontaneous Expulsion,” forty-nine cases are reported in full, in all of which the patients recovered after the discharge of the foreign body; in eight others, death occurred at longer or shorter periods, after the expulsion. Many of these cases have been extracted from the journals of this country and Europe; whilst not a few have been communicated by the friends of the author, and now appear in print for the first time; nine of these, indeed, occurred in his own practice.

“It has been stated that the extraneous body, when the parts relieve them-

selves spontaneously, is most commonly ejected in a fit of coughing. By a reference to the accompanying table, it will be seen that this occurred thirty-seven times out of the forty-nine; in eight, the mode of expulsion is not stated; in one, it took place in sneezing; in one, during spontaneous vomiting; and in one, in dreaming." "The period during which the body may remain in the air-tubes, after the first symptoms have passed off, varies from a few minutes to several hours, days, weeks, months, and even years. In one of the cases referred to under this head, the substance, a piece of bone, was retained sixty years." (p. 116.)

We now approach the most important part of the whole subject, namely, the treatment, both medical and surgical. From the fact that emetics are very efficacious in expelling mucous and other accumulations from the air-passages, it is not surprising that the same means have frequently been resorted to with the view of dislodging extraneous substances from the lungs. Experience, however, has shown that such hopes are delusive, and that not only much precious time is thus lost, but that even the life of the patient is endangered by the foreign body being occasionally driven into the larynx and provoking violent spasm of the glottis. The practice, too, appears to be based upon false physiological notions, since it is well known that the act of vomiting is attended by a deep and forcible inspiration, which might readily tend to wedge the substance more firmly in the lungs. In the forty-six cases alluded to by the author, in which emetics were used, "the only beneficial effect produced in any, was a slight and transient amelioration of the cough and dyspnoea. In some, their exhibition was decidedly prejudicial, causing a marked increase of the respiratory embarrassment; and in not a few, the patient seemed to have been in imminent danger of suffocation from the foreign body being forcibly impelled against the larynx in the act of vomiting;" from all which he concludes that the only cases in which they should be used, "are those in which there is great respiratory embarrassment, in consequence of the excessive accumulation of mucus in the air-cells and bronchial tubes, or in which the symptoms are of a marked asthmatic character."

Sternutatories, or even accidental sneezing, have occasionally expelled a foreign body from the air-passages; but such an occurrence is so exceedingly rare that no importance can be attached to them as remedial agents. The violent expulsive effort which attends their action, naturally tends to drive the offending body into the larynx; but the spasm excited by its presence, when it reaches this point, prevents its escape through the glottis, and may even endanger the life of the patient. To overcome these objections, Dr. Gross suggests the use of chloroform.

"The proper plan would be to make the patient inhale this fluid until he is nearly or wholly insensible, and to irritate the Schneiderian membrane with snuff, or some other substance, the moment he begins to regain his consciousness. Should sneezing ensue while he is in this condition, with the air-tubes in a state of perfect relaxation, it is easy to conceive how the foreign body might be ejected. Nature would be taken, as it were, by surprise, as she has sometimes been by a dream, as in the remarkable case occurring in the practice of Mr. Cock, of London. As the use of sternutatories, exhibited in the ordinary manner, has hitherto been almost invariably unsuccessful, I merely throw out this hint for the consideration of the reader, without feeling inclined to place much confidence in it."

Allusion is also made to the inhalation of iodine, or other irritating substances, with the view of provoking spasmodic or violent cough, and thus promote expulsion. A successful case, thus treated, is reported; but when we bear in mind that the pulmonary irritation is, in most instances, already excessive, it is difficult to conceive of the propriety of such a procedure.

Most of our readers will recollect the memorable case of Mr. Brunel, where the foreign body—a coin—was removed from the air-passages by inversion, after tracheotomy had been performed; since then, several cases of a somewhat similar character have been reported. The treatment by “inversion” is, however, only applicable to heavy substances, which by their gravity might, under favourable circumstances, escape through the chink of the glottis. Such an experiment, however, is not unattended with risk, unless, at the same time, the respiration should be maintained by an opening in the windpipe, as in the case above alluded to. Dr. Hansford, of Knoxville, Illinois, has recommended that the patient should be placed in the horizontal position upon a table or bench, with the head inclined over the edge, and, at the same time, to apply a sharp blow on the back of the chest during forcible inspiration. By means of the “maul,” as he terms it, he has succeeded in expelling water-melon seed, and other comparatively light substances, at a single blow. Others have recommended forcible inspiration, followed by sudden compression of the chest; but, however ingenious these various means may appear, a more extended experience has not confirmed their utility, and they are certainly not devoid of risk, for reasons already assigned. Before proceeding to the consideration of the surgical treatment, the author directs especial attention to the various inflammatory affections which are frequently induced by the presence of extraneous bodies; he especially cautions against the mistaken idea that the patient is necessarily safe when once the offending substance is expelled; and, after reporting a case illustrative of these views, he concludes with the following remarks:—

“The above instance is not a solitary one; the records of surgery are crowded with them. Had the patient been carefully watched, she would not have perished from an accident, from the effects of which, under proper management, hundreds have recovered. But a spark had been kindled in her lungs, which a drop of water might have extinguished in its incipency, but which, having once spread, nothing could afterwards subdue.” (p. 196.)

Chapter IX., and the seven following chapters, are devoted to the consideration of the operation of bronchotomy, including various interesting anatomical and physiological considerations relative to this subject; the whole illustrated by very numerous cases of the most interesting and instructive character. The author, in speaking of the difference in the sensibility of the mucous membrane of the trachea and bronchial tubes, as compared with that of the larynx, alludes to the experiments of Magendie, showing that if an opening be made into the windpipe, and a probe be passed up into the glottis, violent irritation and coughing are produced, while no such effect follows the introduction of the instrument into the trachea and bronchial tubes. Dr. Gross, in repeating these experiments, ascertained that the sensibility of the bronchial mucous membrane, though less than that of the larynx, was considerably greater than the lining membrane of the trachea—so that the greatest sensibility would appear to be at the two extremes. He proceeds, however, to show that the sensibility of the trachea sometimes becomes exquisite from the effects of disease, whilst, at others, all feeling appears to be destroyed. The following passage will show the estimation in which he holds the operation of bronchotomy:—

“A careful examination of the facts which are comprised in this treatise will, I think, serve to satisfy any one, however prejudiced or skeptical, that the only real safety of a person, labouring under a foreign body in the air-passages, consists in bronchotomy. We have seen, it is true, that various substances, entrapped in these passages, may be ejected, either spontaneously or through

the intervention of art, as the use of emetics and sternutatories, or even by simple inversion and succussion of the body; but no one, surely, acquainted with the subject would adduce such cases for the purpose of establishing a rule of practice."

Next follows a complete history of laryngotomy, tracheotomy, and laryngo-tracheotomy, accompanied with numerous well-executed wood-cuts, illustrating the anatomy of the parts, the necessary instruments, and the manner in which the operations should be performed. Opening the trachea in the adult has, at all times, been considered both an easy and safe operation; but in early infancy, the case is very different, since the dangers and difficulties are greatly increased by the shortness of the neck, and the great depth and small size of the trachea; the restlessness of the child also tends greatly to increase the embarrassment. To overcome this latter difficulty, Dr. Gross strongly urges the necessity of resorting to the inhalation of chloroform; in fact, in such cases, he considers it a *sine qua non*, and alludes to some successful cases in which it had been used by Drs. Kearny Rodgers, of New York, and Wm. Davidson, of Madison, Indiana. In the course of his remarks he states: "I know hardly an operation in all surgery that I would not rather undertake than this," unless aided by the use of chloroform. Never having witnessed the administration of this agent under the circumstances above alluded to, it is not in the power of the writer of this notice to speak authoritatively upon this subject; theoretically, however, there appear to be some objections to the procedure. That the inhalation of chloroform, under all circumstances, is attended with more or less risk, is fully proved by the numerous cases of sudden death from this cause now on record. Its rapid and peculiarly depressing effects upon the nervous centres—as manifested by nausea, vomiting, and feebleness of pulse—would appear especially to preclude its use in a class of patients particularly liable to the development of cerebral symptoms under slight provocations. These objections would not apply with the same force to the use of ether, which an ample experience has now shown to be comparatively safe. But, if the use of this class of remedies greatly facilitates the performance of the operation, does not the very insensibility thus induced remove one of the chief safeguards of the patient, namely, the natural irritability of the air-passages? Despite all the care the surgeon can take, and however mindful he may be of the importance of arresting all hemorrhage before opening the trachea, more or less blood will be drawn into the air-passages, and might readily produce fatal asphyxia, if not at once expelled by the violent expulsive efforts which immediately follow the opening of the trachea. Moreover, all experience has shown that, in a vast majority of cases, the foreign body is driven through the opening immediately, or soon after the operation. Now it is quite conceivable that this fortunate result would be more or less prevented by the sedative effects of anæsthetics. It is true that the surgeon might be enabled to probe the bronchial tubes, and apply the forceps, with less embarrassment; but is there not some danger that this very freedom of action would lead to protracted and forcible manipulations, destructive to these highly organized and delicate structures, to say nothing of the important nerves and bloodvessels in juxtaposition with these parts? It is also highly probable that the inhalation of chloroform strongly predisposes to congestion of the bloodvessels about the head and neck, thus tending to increase the hemorrhage, and, consequently, the danger of the operation. In confirmation of this view, we find the following remarks upon a case in which this operation was performed by the late lamented Dr. J. Kearny Rodgers, of New York:—

"Laryngotomy was then performed at the crico-thyroid space, the patient having been previously placed under the influence of chloroform. This agent caused a good deal of congestion of the head and neck, leading to considerable hemorrhage, and the necessity of tying several vessels." (Chap. XIV. p. 417.)

The further history of this case also goes to show that the patient "suffered severely, for a number of days, from the effects of the chloroform."

The latter part of the volume contains the report of thirteen cases in which laryngotomy was followed by the expulsion of the foreign body and the recovery of the patient, and only four which had a fatal termination. Upon which the author remarks:—

"In my bibliothecal researches, comprising many hundred volumes, I have not found another example of the kind; and hence, although it is very probable that I may have overlooked some cases, it is reasonable to infer that the operation in question is rarely followed by fatal effects."

Sixty cases are reported in which tracheotomy was successfully performed for the extraction of foreign bodies, and only eight are mentioned as having been followed by death. Comparing the successful with the unsuccessful cases, he thinks that we must adopt one of two conclusions: either that the latter are much less numerous than the former, or that surgeons do not report the unsuccessful as frequently as the successful ones. He also narrates ten cases of laryngo-tracheotomy having a favourable result, and but three in which the operation proved fatal, and these were the only ones he was able to find in our periodical and systematized literature.

Although the number of cases above reported may not be considered as fully sufficient to establish any very positive deductions as to the mortality from bronchotomy, still, it must be admitted that Dr. Gross is entitled to the thanks of the profession, for thus systematizing the most important facts to be found on record upon this deeply interesting subject. He justly remarks, in his preface, that, in an inquiry of this kind, individual experience amounts to nothing, and it is only by arranging, analyzing, and tabulating isolated and scattered facts that we can hope to advance the interests of science and humanity. After a careful analysis of all the facts, the most important conclusions arrived at by the author are:—

"That bronchotomy, in none of its forms, is a hazardous operation; and that it rarely proves fatal, except when it has been too long delayed, or when it is not followed by the ejection of the offending body." "Although foreign bodies have occasionally been ejected from the windpipe under the influence of emetics, errhines, and other means, the number of such cases is too small to justify the practitioner, under any circumstances, in confiding in these different classes of remedies. Generally, indeed, their effect is to increase the respiratory suffering, and the danger of the patient, by impelling the intruder against the larynx, where its presence always excites spasm and other unpleasant symptoms."

Many other important deductions are summed up in the last chapter, which, in fact, constitutes a very perfect *résumé* of the whole matter. Appended to the work, there is also a complete index, alphabetically arranged.

In concluding this imperfect sketch of Dr. Gross's very elaborate book upon *Foreign Bodies in the Air-passages*, we feel no hesitation in recommending it to the profession as a work replete with much important and interesting matter upon the subject it purports to treat, and as well calculated to fill a void which has long been felt in our medical and surgical literature.

W. P.

ART. XVI.—*The Transactions of the American Medical Association*—Instituted 1847. Vol. VII. 8vo. pp. 668. New York, 1854.

THE seventh annual session of the American Medical Association, held at St. Louis, Mo., in May, 1854, was opened, in the absence of the President, Dr. Knight, of Connecticut, by a short address from the senior Vice-President, Dr. Usher Parsons. This address, prepared at a very short notice, is very concise, but at the same time neat and appropriate. It presents, however, no prominent points or topics which require especial notice.

The printed transactions of the session, as presented in the volume before us, comprise a fewer number and a less variety of strictly medical reports and papers than those of several of the preceding sessions. They are comprised, in great measure, of three reports on the epidemics prevailing in certain sections of the United States; a report on erysipelas, one on the medicinal and toxicological properties of the cryptogamic plants of our country, and the prize essay of Dr. Brainard, on the treatment of ununited fractures. These, nevertheless, form a volume replete with interesting and valuable matter; a volume that will reflect no discredit upon those who contribute the several papers it comprises, nor upon the Association under whose auspices they have been given to the world.

The first of the reports contained in the present volume is that on Medical Education, from the pen of Dr. J. L. Cabell, of Virginia. It is a well written, sensible paper.

Confining himself to "a recapitulation of the several particulars in respect to which the system of collegiate medical education in the United States has been the subject of unfavourable criticism, and to an inquiry in regard to the extent to which the measures hitherto taken by the Association have had the result of removing or lessening the defects against which they were directed," the author, with a great deal of candour, and in an independent spirit of criticism, discusses the character and sources of the defects referred to, the causes which have heretofore impeded their removal, and the means best adapted to overcome these impediments.

Among the defects in the existing system of medical education in the United States, Dr. Cabell places prominently "the want of a proper preliminary education on the part of a large, we fear a constantly increasing proportion of medical students." In concluding his very pertinent remarks on this topic, Dr. C. has no doubt that—

"The Faculties of all the most respectable schools of medicine in the Union, are truly anxious to conform to the recommendation of the Association in respect to the measure of proposed reform in respect to the preliminary education of matriculants; and would cheerfully submit to some pecuniary sacrifice, if there were a reasonable guarantee that the desired end could be thereby attained. But as the imposition of rigorous conditions of matriculation by a few of the schools will, it is believed, have no other result than to draw a large proportion of their students to such other institutions as will exact from the candidates for graduation but slender requirements of labour and of learning, it seems scarcely reasonable to hope that, under existing circumstances, the principal medical schools should adopt a policy which threatens to prove so ruinous to their interests, while it offers no compensating advantage to the profession at large."

After noticing the fact that, in some of our medical colleges, the Faculty is

not clothed with the authority of prescribing the conditions of matriculation, Dr. Cabell asks :—

“Is there, then, no remedy for an evil which has done so much to dishonour a profession once noted for the profound and varied learning of its members? Your Committee,” he replies, “are of opinion that it would be premature to come to so discouraging a conclusion. They do not yet abandon the position which was ably sustained by the late Dr. Parrish, in a report made to the Philadelphia Convention, that ‘the influence of combined and harmonious action, directed to a special object, by the great body of the profession, is a power more potent than that exercised by legislatures, or by the corporations they may create.’ The operation of such an influence is, however, slow, and we should not be impatient if ample results are not rapidly attained. One of the instrumentalities through which it operates is gradually being brought to bear upon the solution of the difficulties which thwart the attempts that have hitherto been made to introduce this and other measures of medical reform. We allude to the organization of State and County Medical Societies, the manifold usages of which agencies are too obvious to require exposition.”

The next defect noticed by Dr. C. is the shortness of the annual lecture term in our medical colleges.

This subject is examined with much care, and in a commendable spirit. The arguments that have been presented in favour of a continuance of the present term of four months, and the objections that have been urged against any further extension of it, are candidly discussed; while the separate and appropriate duties, in the education of the medical pupil, of the private and professorial instructor are briefly considered. The general conclusions at which Dr. C. has arrived in reference to these important topics will be understood from the following extracts :—

“The weight of education must fall, unquestionably, upon the pupil; and any system, whether that of public lectures or private preceptorship, which aims to dispense with constant and hard labour on the part of the pupil, is mischievous in proportion to its success. The question is not whether teaching by lectures be better than active personal study, since these are by no means incompatible, but whether personal study will be most advantageously aided and directed by a judicious course of instruction by lectures, or by ‘private preceptorship mainly,’ with ‘the school as its complement.’” The Committee “are firmly convinced that ‘the active, practical discipline of the mind,’ is one of the most certain results of the right use of the system of teaching by lectures.” “Under this system, the intelligent student, following in the train of thought pursued by the lecturer in his discussion of doubtful questions, in his analysis of complex phenomena, and in his inductions of general truths from particular facts, learns to perform for himself the works, so difficult to those who have only studied by a mechanical routine of unreflecting labour—of critical analysis, and inductive generalization.

“In order, however, to insure the full success of this system by a right use of it, and by preventing that abuse which is truly obnoxious to the criticism of the medical Faculty of Harvard University, and which, unhappily, is too prevalent, it is indispensable that searching examinations should be given, each day, on the lectures of the preceding day. It is this combination of the two systems which is productive of the most wholesome results. Those who have had experience in this matter know how wonderfully the faculties of mind are stimulated in listening to a discourse on the topics, arrangements, and illustrations in regard to which we expect to undergo a public examination.” “If the teacher propounds his questions, as certainly he should, in such a form as to throw upon the student the burden of analyzing a complex subject submitted to him for elucidation, it is impossible to exaggerate the benefit which the latter may derive, both in giving precision and completeness to his knowledge of the particular subject, and in the mental discipline which is incidental to the process employed in its acquisition. It must be evident that such examinations

will be more useful if conducted by the lecturer himself, than if left to the performance of a third party, especially if the latter did not himself hear and enter into the spirit of the lectures."

"The Committee do not suppose that more than a very few of the existing medical colleges will adopt a system so radically different from that in connection with which they have enjoyed, and continue to enjoy, a large measure of public patronage; but they do believe that new institutions, the incorporation of which, by legislative authority, the profession is utterly powerless to control, will be induced to make an experiment, the success of which, as regards the thoroughness of the instruction given to the pupil, cannot be doubtful. At all events, the Committee feel it to be their duty to recommend such a system of medical education as, in their opinion, may comport with the existing state of the medical sciences, and satisfy the reasonable demand of the profession, rather than to speculate upon the probability of its adoption by the medical schools of the Union."

"For that class of students who reside in our cities, and enjoy the advantages of private instruction by competent and zealous teachers, possessing all the necessary appliances for practical illustrations, a shorter course of public lectures may not only be sufficient, but, in some instances, preferable. But as respects that far more numerous class of students who come from the rural districts, the case is very different. Many of these pass the interval of eight months, between the end of the first course and the commencement of the second, in desultory reading, and without the benefit of suitable instruction or guidance. This course cannot be otherwise than detrimental: and, for the benefit of all who pursue it, an extension of the term is urgently needed. The Committee would therefore suggest the expediency of some action on the part of the Association, by which encouragement will be held out to such schools as may be willing to make trial of a reformatory plan, the general features of which are as follows: 1. The extension of the term to eight months, with not more than four lectures per diem at any period of the course, and not more than three during the dissecting season. 2. The exclusive assignment to such schools of the whole groundwork of elementary medical education, and the subsequent transference of the advanced pupils to schools, whether private or public, in which they should be taught to make practical application of the principles which they had previously acquired."

The third defect in the existing scheme of medical instruction, to which attention is directed, is "the omission, in the curriculum established by many of our medical colleges, of one or two important branches of knowledge." The pertinent remarks of Dr. C. under this head prove forcibly the correctness of his conclusion, that this omission is "an evil which, in the present state of the medical sciences, and in view of the relations of the medical profession to society, imperatively calls for redress."

The fourth defect considered is, the entire neglect, or very imperfect use, of clinical instruction in many of the medical colleges of the United States.

"As respects some of these institutions," Dr. C. admits "the defect is irremediable, owing to their location in villages too small to furnish subjects for hospital practice. And in some of our larger cities, the seats of the oldest medical colleges in the Union, the hospitals are either entirely inaccessible to the student, or are opened to him under such restrictions as to make it a privilege of little value. In such cases, the schools have been driven to the necessity of finding a substitute in what are termed 'college clinics.' But, even where no other impediment hinders the students from visiting the hospitals daily, those who attend six lectures each day in the college, and are also engaged in the study of practical anatomy in the dissecting-room, can find little time or disposition for such clinical observations as will be of any value to them. Hence, it is only the small minority who remain in the city during the summer that truly derive any profit from the facilities for clinical observation which the city hospitals may afford.

"We have almost invariably found that, among even this class of students,

only those who have previously graduated can be induced to read, in connection with their clinical observations, the class of books which constitute the elucidators of disease. The undergraduates feel themselves constrained, by the prospect of a coming examination, to confine their reading mainly to the elementary text-books. In view of such facts, your Committee express their hearty concurrence in the position taken by Dr. Pitcher, in his report, and indorsed by the vote of the Association, 'that a familiar knowledge of the elements of medical science should precede clinical instruction.'"

The last defect noticed by Dr. C. is one which he very properly stamps as "the most flagrant and mischievous of all the defects of American medical schools, the correction of which is essential to the attainment of much or any benefit from any other projected measure of reform." This defect is the "disgracefully low standard of professional knowledge and general mental culture exacted of the candidates for the degree of Doctor of Medicine."

"The evil in question," Dr. C. remarks, "having originated, in a great measure, in the active competition of rival schools, some have contended that it can only be remedied by diminishing the number of these institutions. If this were so, we should be obliged to abandon all hope of relief, and submit to a perpetuity of disgrace; for those institutions now possess chartered rights, which it cannot be expected that they will relinquish. Nor is it probable that more good than harm would ensue from a diminution of the number of the schools. If students could obtain admission into the ranks of the profession *only* after giving satisfactory proof of attainments and mental discipline as would test the thoroughness and the judicious character of the instruction they had received, the rivalry between the separate schools would consist in efforts to elevate the standard of medical education by enlarging the curriculum with the progressive advancement of the medical sciences, and by perfecting the means of illustration and exact demonstration. The only available remedy, then, in the opinion of the Committee, is the establishment of boards of examination, distinct from the Faculties of the schools. It does not suffice to have a committee of the State Society present at an examination conducted by the Faculty of the school. The desideratum is to have a uniform standard throughout the Union, or throughout the limits of each of those States in which such a board may be appointed."

"In those States in which there are no laws regulating the practice of medicine, and where, consequently, the collegiate diploma has no legal character, its value being solely due to the popular appreciation of it as a certificate of professional attainments from those who are supposed to know the character of the graduate, and are presumed to give truthful testimony, it seems reasonable to believe that the united voice of the profession would be as potent in exacting the possession of a license from an extra-collegiate board of examiners, duly appointed in conformity with a general plan sanctioned by the American Medical Association, and adopted in all the States of the Union, as it now is in inducing young men to procure a collegiate diploma which is not necessary to legalize the practice of their vocation."

Dr. C. considers that the reflected influence upon the schools of the operation of this board of examination, in inducing them to elevate their standard lest they should be discredited by the wholesale rejection of their graduates by the licentiate boards, would be of immense service, although a majority, or a large proportion of their graduates might refuse to undergo the additional test. This is precisely the way in which Dr. C. expects the plan to work most favourably for the cause of medical education.

Dr. Cabell concludes his report by presenting for the consideration of the Association the following resolutions, as a summary of the principal views embodied in the report:—

"1. *Resolved*, That the views and recommendations heretofore expressed by this Association, respecting the importance of establishing a uniform standard

of preliminary education, of extending the term of lectures, and especially of greatly elevating the standard of professional attainments requisite to graduation, be hereby reaffirmed.

"2. *Resolved*, That this Association approves and recommends the practice of daily examination by each professor, as essential for securing 'that active, practical discipline' of the mind, which is one of the most important ends of collegiate instruction; and believes, not only that such a system might be easily put into operation, under an extension of the term of lectures, but that the whole groundwork of elementary medical instruction might be most advantageously assigned to the schools which may adopt that system, as a substitute for the very faulty one of private office instruction now in common use.

"3. *Resolved*, That this Association cordially approves of the establishment of 'private schools,' duly organized, for giving that species of instruction which consists 'in demonstrations,' and other practical exercises 'on the part of the student, instead of the instructor, but still under his direction and superintendence,' embracing the whole circle of clinical observation and practice, the use of the microscope, chemical manipulations, and the performance of surgical operations on the dead body; and would earnestly recommend such institutions to the patronage of those graduates who did not enjoy similar advantages during the period of their collegiate pupilage.

"4. *Resolved*, That those medical colleges whose curriculum does not now include full courses of lectures on physiology and medical jurisprudence, be earnestly invited to make immediate provision for supplying the deficiency, and to require the professor of physiology to make an exposition of the outlines of comparative anatomy, to such an extent, at least, as may be necessary to enable the student to appreciate the force of the evidence upon which the modern doctrines of physiology mainly rest.

"5. *Resolved*, That to insure the efficient and beneficial operation of the proposed measures of reform, the Association considers it essential that some uniform system of examining candidates for admission into the ranks of the medical profession, *in addition to the collegiate examinations for degrees*, should be adopted in all the States of the Union.

"6. *Resolved*. That the Association regards as auspicious omens of future progress, the already improved character of our medical literature, and the evidences of an increasing desire, on the part of a respectable number of medical students, for a higher grade of professional education, as exhibited in the patronage extended to extra-collegiate organizations for practical teaching; and that, in view of such encouraging signs, it cherishes an abiding conviction that more thorough and general reforms will be ultimately, though gradually, accomplished."

We have been much gratified by the tone and tenor of the report, of which we have given above a very imperfect analysis. The author does not satisfy himself with merely enumerating the leading defects in the present system of medical education in the United States; and with simply deploring their existence, and the little influence which the action of the Association has heretofore had in effecting their removal. Admitting with perfect candor the difficulties and obstacles which stand in the way of effecting a radical reform in medical instruction, and of elevating it to the desired standard, he is, nevertheless, inspired with full confidence that, by an honest and zealous co-operation on the part of the great body of the medical profession, and a very little patience, the removal of these obstacles and difficulties may, in due time, be accomplished, and indicates the proper means by which so desirable an object may be accomplished, and one of the leading purposes had in view in the organization of the American Medical Association, attained.

We believe that the plan suggested by Dr. Cabell is a perfectly practical one, and demands the serious consideration of the members of the Association. The friends of reform in medical education believe that sufficient time has been spent in discussions and resolutions, and that the period for effective

action has fully arrived. Let then the Association, instead of again appealing to our medical schools for a prolongation of their respective lecture terms, the enlargement of their courses of instruction, and the requirement of a more thorough and complete preparation on the part of those upon whom they confer the honours of the doctorate, take the entire subject in its own hands, and decide at once what shall be the qualifications of those who shall be admitted and recognized as legitimate members of the medical profession. Let the latter refuse to recognize the mere possession of a diploma from a medical college as a sufficient passport to entitle its possessor to enter its ranks. In this manner, a broad line of distinction would be established between the well-instructed physician and the mere medical graduate, and no fear need be entertained that the public would not in time perceive and acknowledge the more solid claims of the first to their confidence in the hour of sickness and of peril.

Passing by, for the present, the report of Dr. Sutton on the Epidemics of Kentucky and Tennessee, we come to the highly interesting paper of Dr. R. S. Holmes, of St. Louis, on Erysipelas.

The views of the author, though somewhat loosely expressed, and thrown together with but slight regard to logical arrangement, are, nevertheless, deserving of close attention. They are, we believe, in the main, correct. Everything connected with the history and etiology of erysipelas would appear to mark it as a constitutional disease. Even when the result of a wound or other injury, it is evidently the condition of the patient's system which gives to the resulting inflammation the erysipelatous character. We are not quite prepared, we confess, to subscribe to the opinion advanced by Dr. H., that erysipelas is a "blood poison—a blood disease"—that "something is in the blood, requisite to be thrown off, nature doing it by mucous, serous, by glandular, or by skin structure."

"Previous to this apparent inflammatory action, however," remarks Dr. H., "the system in general has given signs of suffering; there is a chill followed by fever; or fever sets in from the commencement, pains in the limbs, or body; uneasiness, derangement of biliary, stomach, or intestinal secretion; flushed face; pains in the head; and very often oppression about the throat or salivary glands; in every idiopathic or severe traumatic case of erysipelas, the constitutional precede the local symptoms. There are, doubtless, cases of the disease where no external sign is given of its presence, where the tendency occasionally manifested by it to seize on serous, instead of mucous or cuticular tissue, is in existence from the onset. We hold that the peritoneum, the pleura, or the arachnoid may take on the erysipelatous inflammation as certainly as the lining membrane of the fauces, but is not confined to those parts; it can no more be called a dermal disease than it can be called a peritoneal disease.

"The exudation to which an erysipelatous cause gives rise is of a serous character. This exudation of erysipelas is greater in amount in regard to its serum, and smaller in amount in the nuclei of pus cells, or of fibrin. This may be readily supposed to be its character; for it shows no tendency to the reparative process, is thrown forth in such an amount, at times, as to burrow through the surrounding tissues, and even, as in the case of 'black tongue,' either by its own tendency to decay, or by the severance of continuity of parts, to assume the gangrenous character. We see no reason why erysipelas may not cause death without any structural lesion, as the poison of malaria often does, or the bite of a serpent."

The general peculiarities of the effusion which takes place in erysipelatous diseases, seem to Dr. H. to be:—

"A likeness at first to serum; a want of any tendency in neighbouring parts to absorb it; a subsequent change into a yellowish, turbid, ichorous, or imper-

fect pus formation ; the comparative inability to maintain its life when in contact with living structures for any length of time ; its tendency to cause tissues in contact with it to assume its own state of decay ; the infrequency of its assuming the true pus, or adhesive, or fibrinous type of epigenesis."

The exudation of imperfect serum or pus which Dr. H. describes as the result of erysipelatous inflammation may, he believes, "be viewed as a purely diseased state of the secreting vessels of the part ; which secretion itself acts as an irritant or poison to the part, or neighbouring parts, or to the system at large, betokening an improper state of the system as regards its curative powers."

There is evidently a close relationship between certain forms of phlebitis and the resulting pyemia, and erysipelas. Dr. H. remarks :—

"Concerning what has been called the absorption of pus, and its subsequent deposition in a near or remote part, we hold that this doctrine is not favoured by either physiology or pathology. The fact of pus being secreted in liver, in lung, in cervical, axillary, or lymphatic gland, after operations or wounds on the extremities, or as a sequence to an inflamed uterus, has been long known ; but we find little to countenance the belief that pus is taken up as such, and deposited again in a distant part. The sudden disappearance of abscesses may be accounted for without supposing that the pus globule, in its integrity, has entered the circulation ; but even on the supposition that it has, it is by no means probable that all these globules would be caught up by some particular gland, leaving aside the very doubtful possibility of the size of the globule permitting it to pass through the capillaries. These deposits of pus, too, are found in distant parts ; while the primary offending wound, or inflamed surface, has formed little or no pus. In fact, they are more frequently found under such circumstances, and particularly in inoculated wounds. As well as we can understand the meaning of terms, this is what is generally called the phlegmonous inflammation ; but it is an inflammation of essentially the same kind as the erysipelatous, accompanied by the same symptoms, and cured by the same means. It is the absorption of imperfectly eliminated material ; which material (in a state of decay, after it has been thrown forth by the part without accomplishing ulterior objects) has been absorbed or taken into the circulation, causing in the blood, or in the part where it is set down, for expulsion, its own act of decay—or effort of the system to expel it. We suppose this is the whole idea of the secondary deposition of pus. A decomposing atom, if you choose, a non-laudable deposit of pus, has occurred, having within it the very character of decay. This decay may take place within the blood after absorption, or within a part, as any gland, after deposition, causing an inflammatory exudation about the part, and a pus secretion from this as the readiest means of expelling the intruding substance. We think this the whole process of pus deposits—a true phlegmonous erysipelas of deep-seated parts ; for you will observe about these parts, should the deposition be, we will say, in a neck gland, that about it, is the true erysipelatous inflammation, and the constitution suffers in the way it is wont to do, had the disease displayed itself in superficial, instead of deep-seated parts. The phlegmon undermining the tissues of an arm, or other part, burrowing among the muscles, and dissecting them out by the inflammation of their connecting areolar tissue, and giving exudation to a copious fetid discharge, has received the name of deep-seated erysipelas, phlegmonous erysipelas, phlegmonous inflammation of the areolar tissue, etc. This inflammation has most probably come on at first with an erysipelatous blush on the arm, or with distinct points of pus deposition occurring here and there. The inflammations, we hold, are all a difference of degree, not of kind ; more common, it is true, to the superficial than deep-seated parts ; but commencing with the same chill, or cold stage, with a fever and constitutional symptoms of the same kind ; showing the like intermitting character, with the like biliary, or chylipoietic derangement, and cured by the same means."

Though not clearly expressed, the views set forth in the foregoing extract

from Dr. H.'s paper are deserving of deep attention. It may excite surprise that the author has not referred to the observations of M. Lebert, and the opinions of Rokitsansky on pyæmia; the leading views expressed by both these writers correspond in many respects with those advanced by Dr. Holmes.

Dr. H. admits the now, we think, well-established fact that puerperal fever may result from the same atmospherical cause that is productive of erysipelas, of the epidemics of which it is an almost invariable concomitant.

"Peritoneal inflammation, arising from childbirth, we hold," says Dr. H., "is not strictly a proper term; it is an inflammation of surrounding parts—of uterus, ligaments, neighbouring intestine; all these are frequently partially glued together, or bathed in the same sero-purulent exudation, engorged with blood, or partially so in patches, and thickened and improperly lubricated."

We have no desire to analyze the therapeutics of erysipelas as laid down in the paper before us. Suffice it to say, that the chief, almost the only, remedies Dr. H. relies on are *large* doses of quinia, bloodletting, and tartarized antimony. We are much better pleased with the views of the author in relation to the pathology of erysipelas, tainted though they may be by a few hypotheses of doubtful accuracy, and some expressions of equivocal significance, than we are with his plan of treating the disease, and the *rationale* adduced by him of the *methodus medendi* of the remedies he relies upon to arrest its course.

The next paper is "A Report on the Medicinal and Toxicological Properties of the Cryptogamic Plants of the United States," by F. Peyre Porcher, M. D., of Charleston, S. C.

This is an able and valuable paper, one which has evidently cost its author a vast amount of labour. To bring together, as he has done in this report, the leading facts that have been developed by various observers in relation to the properties and probable available uses of the cryptogamous plants met with in this country; to indicate their respective localities, and to identify their species and varieties, so as to be able correctly to appropriate the recorded observations in relation to their curative, poisonous, or other properties, was, we can readily conceive, a task of no trifling character, demanding considerable research, much time, and no little patience, more especially as the entire subject belongs to a department of inquiry which has had few labourers in this country. The present report required, therefore, the embodying of a comparatively large amount of material, in order that our knowledge of the subject might be brought up to the present day, and that future inquirers, being placed in possession of what has been already accomplished, may proceed directly to more experimental researches.

The real value of this contribution of Dr. Porcher, as well to the *materia medica* of our country as to its resources, will, we fear, be properly appreciated by but few. The report is not one of which any satisfactory or useful analysis can be given. To acquire a clear conception of the facts it comprises, and their practical application, it must be closely studied in all its details.

The present volume contains three reports on the epidemics of Kentucky and Tennessee; of Ohio, Indiana, and Michigan; of Louisiana, Mississippi, Arkansas, and Texas. They are all replete with interesting and valuable observations, bearing on the etiology, pathology, and therapeutics of the prevalent diseases of those sections of the Union, founded upon observations chiefly made during the year 1853.

The first two, prepared, respectively, by Drs. W. L. Sutton and George Mendenhall, present a very good digest of the topography and meteorology of the States embraced in them, illustrated by an excellent map and copious tables.

The third report on epidemics is by Dr. E. D. Fenner, of New Orleans, and is devoted mainly to a history of the yellow fever of 1853. So far as regards the origin, spread, and progress of the disease in the city of New Orleans and other portions of the State of Louisiana, its history is minutely detailed; while, in relation to its prevalence in different parts of Mississippi, Arkansas, and Texas, many interesting facts, though less complete and continuous, are presented; the whole affording valuable materials for the pathologist, in his investigation of the etiology and leading characteristics of the fever in question.

The entire series of reports on epidemics, presented at this session of the Association, are well drawn up, and offer much to interest and instruct the medical inquirer. We had intended to present a number of excerpts from them, but we are admonished, by the space we have already occupied, and the difficulty of selecting from a mass of facts, all of which equally press themselves upon our notice, of the necessity of foregoing our intention; at the same time, we recommend a careful perusal of these reports to all our readers.

A short paper "On the Identity of Bilious and Yellow Fevers," by Dr. Linton, may be taken in connection with the reports on epidemics. The title of this paper appears to us to be a misnomer. Though the writer sets out with declaring that bilious remittent and yellow fever are "*specifically identical*"—which, we may remark, in passing, is rather a strange form of expression—nothing is said of the evidence upon which this identity is predicated, but the entire paper is taken up with an exposition of the author's views as to the cause of bilious and yellow fever.

For the benefit of our readers, we here present them with all that he says upon this subject, merely remarking that the only arguments he offers in their support are more specious than solid:—

"It is well known that a richer diet is required by the inhabitants of cold countries and seasons than by those of hot countries and seasons. It is known that a portion of the food forms the *materiel* of combustion in the system, and is eliminated from it in the form of carbonic acid and water; and direct experiments have shown that, in man, twice as much, and even more, carbonic acid is eliminated at 32° Fahrenheit as at 86°. This all of us would expect; for twice as much food, rich in hydrocarbon, is taken in cold latitudes as in the warm and hot, and twice as much combustion is required in the former as in the latter for the purpose of keeping up the due amount of animal heat.

"According to these facts, is it not clear, that northern strangers exposed to a southern sun will suffer the effects of retained hydrocarbon? If a man who eliminates twelve ounces of carbon *per diem* at St. Paul's, on the Mississippi River, should pass suddenly to New Orleans, is it not clear that about one-half of this material would be retained in his blood? Is it not a matter of demonstration that he would be poisoned by the retention of those very matters which, in a cold region, are necessary to health? Now, why may not this poison be the cause of yellow fever? It is proved to exist; and certainly this bilious state of the blood, thus caused, may produce hepatic congestion, portal congestion, jaundice, black vomit, &c. Indeed, the hydrocarbon is the very 'stuff' of which bile is mainly formed. No wonder that its retention in large quantities should cause bilious fever, and even yellow skin and black vomit.

"This view of the cause and nature of yellow fever, explains the fact that strangers from the north are *mostly* affected by it. It explains the fact that those who remain mostly within doors, as old men, and children, and women, are not so liable to it as those who exercise immoderately under a burning sun. It shows how it is that those who have long resided in hot climates mostly escape, and why they do not always escape. It explains the exemption of prisoners from its attack; they live low, and are not exposed to great heat. In a word, I respectfully submit that this *theory* accords with all the facts. I do not deny the malign influences of bad air, and confined air, where it exists. All I say is, that such an external poison is not a *sine qua non* to the production

of the disease. When such a state of the air exists, it renders the disease worse than otherwise it would be. Nor do I deny that the retention of other matters may have something to do in causing the disease. Often, too, a person may escape yellow fever by a bilious diarrhoea. The skin also depurates more in hot than in cold climates and seasons."

The last of the papers of a strictly professional character contained in the present volume of *Transactions*, is the essay of Dr. David Brainard, of Illinois, on a new method of treating ununited fractures and certain deformities of the osseous system, to which a prize of one hundred dollars was awarded by the Association.

The object of Dr. Brainard's essay is, 1. To establish, by experiment, the principles upon which the treatment of ununited fractures should be conducted; and to show that these principles are applicable to the human subject. 2. To propose a new method of treatment for certain deformities which result from true ankylosis, union of fractures in an angular position, rachitic curvature, &c.

The essay is divided into three parts. 1. Experimental researches on the effects of foreign bodies when allowed to remain in contact with the osseous tissue, and on certain wounds of the bones. 2. Treatment of united fractures by subcutaneous perforation of bone, with cases; and 3. Treatment of deformities of the bones by subcutaneous perforation, with experiments.

From the facts detailed in the first part, Dr. B. considers that the following deductions are fully justified:—

"1. Foreign bodies of every kind, placed or allowed to remain in contact with any part of a bone, in a manner to keep up suppuration, produce absorption of it, and have no tendency whatever to give rise to the production of callus. The use of setons, pegs, wires, and foreign bodies of every description, as a means of promoting the formation of callus, is a practice not founded on correct principles, and is often dangerous. The seton is more properly employed for the purpose of dividing a bone, or keeping up a false articulation, than for uniting it.

"2. Sequestra and foreign bodies imbedded in bone, may be brought to the surface when, by perforation or otherwise, instruments or ligatures can be so attached to them as to draw them permanently, with but a moderate degree of force, in that direction. As soon as they press against the living bone, they cause its absorption in the direction towards which they are drawn.

"3. That power of absorption of bony tissue attributed to the periosteum and the medullary membrane, exists also in the substance of the bone itself, as is proved by the insertion of pegs into perforations of bone; absorption taking place around them not only at the surfaces, but at all the intervening points. We have employed the term absorption, in accordance with received usage, to indicate that enlargement of the perforation which takes place around the peg, although it is not certain that the bone is absorbed. It is quite as likely that it is disintegrated by ulceration."

In Chapter II. Dr. B. inquires into the effects of foreign bodies on the formation of callus. The facts he presents in elucidation of this subject, justify, he considers, the conclusion, that foreign bodies which keep up suppuration, not only prevent the formation of callus to a certain distance around them, but produce absorption of callus already deposited when placed in contact with it. The practical deduction may be drawn, he remarks, that setons, pegs of ivory, &c., are suitable means to be employed for promoting the removal of deformed callus and certain exostoses.

Solutions of iodine injected between the extremities of fractured bones, with a view of producing union, Dr. B. has found to act like foreign bodies—to produce inflammation and necrosis, and to prevent the formation of callus.

After a consideration of wounds of bones, and the circumstances in which they give rise to callus, or union without it, and the state of the parts in ununited fracture, Dr. B. proceeds to the treatment of ununited fractures. His own plan of treatment, is, by means of an instrument with a point somewhat awl-shaped, but more pointed in the middle, with the view of avoiding as much as possible the formation of chips, to produce wounds of the fractured ends of the bones, transfixing at the same time whatever tissue may be placed between them. After transfixing the fractured extremities and intervening tissue in one direction, the instrument is to be withdrawn from the bone, but not from the skin, its direction changed, and other perforations made in the same manner. Dr. B. thinks it better to commence, in most cases, with not more than two or three perforations, in order that the effect produced shall not be too severe. On withdrawing the instrument, collodion is to be placed upon the point of puncture.

"Each of the different parts of the operation is," according to Dr. B., "essential to its success. The division of the tissue situated between the fragments, would, of itself, have but little effect; combined with scratching their extremities, its effect is not great, as we have demonstrated by experiment; but, when accompanied by wounds of the bone, there is a very permanent and efficient action produced, which puts the extremities in a condition to join the soft parts in effecting union. This can be rendered greater by increasing the number of punctures, or using an instrument of large size. The operation is to be followed by the application of suitable splints, or apparatus for the purpose of securing immobility. It should be repeated from time to time, and carried to a greater or less extent, as the effects produced may indicate."

To prevent the danger of the perforator passing too deeply, slipping from the surface of a bone, or injuring any vessel, Dr. B. has devised a means for regulating its action. This can only be understood by a reference to the drawing given of it, and the accompanying description.

Chapter IX. treats of the application of perforation of the bones to the cure of certain deformities, by means of interstitial fracture, or bending. The cases in which it is proposed to apply it are:—

"1. Perfect ankylosis by bone, or fibrous adhesions too firm to be separated by moderate force, and where the member is in a position to be useless. In this case its use is recommended with a view of straightening the member, where the original disease has been entirely removed.

"2. Fractures united in such a position as to render the limb useless, or from angularity of the fragments, greatly to incommode the patient. In this case, it is also employed for the purpose of straightening the member, after consolidation is perfect.

"3. Deformity from rachitis, and mollities ossium, where the disease giving rise to the deformity has been perfectly cured.

"4. For the purpose of facilitating the fracture of the femur, and formation of a false joint, in case of ankylosis of that bone with the pelvis."

"The method of applying the treatment will depend upon the object to be obtained. When it is required to weaken the bone as much as possible, in order to facilitate its fracture, it will only be necessary to perforate it several times in one direction, and then, withdrawing the instrument, to repeat the perforation from another point.

"The inflammation thus excited will, in a few days, soften the bone sufficiently to render its fracture very easy.

"If it were desired to effect but a partial fracture, the perforations and weakening should be made mostly on one side, and the same rules followed as in the other case.

"When the object is merely to bend the bone, Dr. B. would recommend, that the perforations be made at three different points, and repeated from time to

time with instruments of smaller size, until the enlargement and softening have thoroughly been effected, and then the gradual application of force, by means of a suitable apparatus, making superficial perforations to soften the surface and prevent the danger of fracture."

In addition to the application of this method, as above described, Dr. B. has employed it for simple perforation of bone, in inflammation tending to abscess. In a case of this kind, where the disease was situated on the internal surface of the tibia, below the knee, two perforations gave relief from a violent pain, without giving rise to suppuration.

Dr. B. lays no claim to originality for the treatment of ununited fractures described by him. He acknowledges that several surgeons of the present epoch have sought to attain the same end; but no one of them, as far as he knows, has "either demonstrated the principle upon which it reposes, or laid down the rules by which it should be performed, or contrived an instrument by which it could be carried into effect with safety and facility." It is his hope that the operation he proposes will be "regarded as an improvement"—and that, "as an attempt to apply the principles of physiology to practical surgery, it will meet with the favourable consideration of the profession."

We feel well persuaded that the essay of Dr. Brainard will be received as a valuable contribution to a most important branch of chirurgery—that of which the entire resources are directed to the removal of deformity and to the restoration to limbs, rendered useless by accident, the greater portion, if not all of their normal shape and functions.

An account of the railroad accident which occurred in May, 1853, and caused the death of seven physicians, who were returning to their homes after attendance as delegates at the session of the American Medical Association, held in New York, with short biographical sketches of these gentlemen, intended as "a suitable method of commemorating that melancholy event, and "the worth and professional character" of the members of the Association who were numbered among its victims; with the report of the Committee of Publication and treasurer, and the catalogue of the officers and permanent members of the Association, complete the contents of the present volume.

By a resolution of the Association, it was provided that a copy of the constitution of the Association should be appended to each volume of the *Transactions*. We regret that the Committee of Publication has not complied, for reasons which are no doubt perfectly satisfactory, with the provisions of this resolution. But few, if any members of the Association, have in their possession copies of its constitution, embracing the several amendments that have been made to it since its original adoption, and not a little inconvenience is liable to result from the difficulty there now is, in referring at once to its provisions in reference to any given question that may arise in regard to them.

D. F. C.

BIBLIOGRAPHICAL NOTICES.

ART. XVII.—*On the Nature, Signs, and Treatment of Childbed Fevers, in a Series of Letters Addressed to the Students of his Class.* By CHARLES D. MEIGS, M. D., Professor of Midwifery and the Diseases of Women and Children, in Jefferson Medical College, Philadelphia, etc. etc. 8vo. pp. 362. Blanchard & Lea: Philadelphia, 1854.

THIS is a learned book—a book exhibiting on the part of its distinguished author considerable research—a familiarity with the leading authorities on the subject treated of, whether of the present or past ages. It is in many points of view a very able treatise on childbed fever, by one who has had much experience in its management, as he has of all the maladies incident to the parturient female. But, after all, we cannot admit that the book is one calculated to put those to whom it is addressed in the possession of “the true idea” of one of the most terrible and fatal maladies to which the child-bearing woman is liable.

It is well known that Dr. Meigs is a strenuous advocate for the doctrine which ranks what is usually denominated puerperal or childbed fever among the phlegmasiæ. That he insists that the disease is not, correctly speaking, a fever, but simply an inflammation of one or other, or several of the pelvic and abdominal viscera. That it is, in every case, nothing more nor less than an endo-metritis, a uterine phlebitis, an ovaritis or a peritonitis—or two or more, or all of these local phlegmasiæ united.

“Whenever,” he asserts, “these inflammations occur in pregnant or lying-in women, then the disease is childbed fever, and it is nothing else, nor are childbed fevers ever anything else, except when they occur as accidents in typhus, smallpox, plague, and other typhous diseases, of which they are in some instances mere complications.”

This doctrine Dr. Meigs labours with a great deal of ability to establish; sweeping away with a bold dash of his pen every fact and argument, no matter how high the authority by which it is backed, that stands in opposition to it.

The fact that in the fatal cases of puerperal fever in which a *post-mortem* examination has been made, incontestable evidence has been revealed of inflammation of the womb, or its veins, or of the peritoneum, or of several or all of these parts, Dr. M. assumes as an incontestable proof of his position, that puerperal fever is purely a phlegmasia and nothing more.

This fact, however, is admitted, we believe, by all those who see in childbed fever something more than a mere local inflammation—who believe that in one form of the disease, at least, it is a true idiopathic fever, the local lesions being the result of, and not themselves constituting the disease.

It appears to us that Dr. Meigs has confined his attention too exclusively to the indications of inflammation detected after death from puerperal fever, in the pelvic organs and the peritoneum. These are not the only lesions met with in the bodies of those who have been destroyed by the disease. Rokitansky describes as frequently present a slight reddening, with investment, of the entire track of the intestinal mucous membrane, by a secretion of a thin serous or viscid gelatinous or more or less purulent character; softening of the mucous and infiltration of the submucous tissues; a dysenteric exudation on the mucous membrane of the colon, resembling that found on the internal surface of the uterus. A similar exudation is also met with on the mucous surfaces of the respiratory, urinary, or œsophageal tracts. The pleura are almost constantly found to contain exudations similar to those met with in the peritoneum; less frequently are they met with in the pericardium. The articulations very commonly exhibit exudations of a fibrinous or purulent character. The dura mater

often presents a slight reddening, with a thin, soft exudation. Rokitsky describes a black softening of the mucous membrane at the fundus of the stomach or of the œsophagus, indicated during life by black vomit, as a frequent occurrence. According to the same authority, the blood exhibits various changes, its fibrinous coagula present a viscid, greenish-white appearance, or the coagula are scanty, gelatinous, and soft. The blood is of a dirty brown red, or chocolate colour, and glutinous, or it is much attenuated and transudes all the tissues. Vegetations on the valves may form from mere mechanical deposition.

Dr. Meigs repudiates the existence of an idiopathic puerperal fever, inasmuch as the term fever excites "a certain material idea of zymosis in the mind of the hearer of it." But is it true that the term fever does invariably excite in the mind the idea of a disease resulting from the introduction into the body of a ferment. Is not the idea of a fever, in the nosological sense of the term, that of a disease depending upon some general disturbance of the vital functions of the system, without reference to its particular cause. Zymotic is, we believe, now used merely as a hypothetical term, to indicate those diseases which result from a common endemic or epidemic cause.

Dr. Meigs himself admits all that would be necessary to include puerperal fever in the list of zymotic diseases.

"Childbed fever," he remarks, "becoming epidemical, may prevail so extensively as to implicate almost all the women who are brought to bed under its reign; or the force of the cause may be so slight as to produce illness in only here and there an unfrequent example, so that the epidemic cases may be very rife or not."

And again: "It is quite clear," he says, "that the malady may break out, and rage with violence in certain circumscribed spaces; and, on the other hand, that it may prevail, at one and the same time, over extensive districts, and even whole nations and countries, and yet be ever one and the same disorder."

"There are, then," he observes in another place, "atmospheric causes, that render pregnant and lying-in women, at particular times, and in certain places, uncommonly liable to attacks of childbed-fever inflammation. What the real principle of this epidemy is, I believe there is no man can say. Be it what it may, one of the most extraordinary conditions connected with it is this: *that it should not poison men, nor boys, or girls, or non-pregnant women, but only the pregnant or lying-in portions of society.* This appears to me to be its greatest mystery."

Now, is it strictly true, that the atmospheric cause of epidemic puerperal fever, whatever its nature may be, poisons only "the pregnant or lying-in portions of society?" If we examine the histories that have been furnished us of the several epidemics of erysipelas that have prevailed in different portions of the United States, we shall find that a certain morbid condition of the atmosphere may occur, which, while it produces in some of those subjected to its influence an erysipelatous affection of the skin, in others it gives rise to inflammation of the mouth and fauces, or of the lungs and pleura; in others, again, to inflammation of the peritoneum; and, in pregnant and parturient females, to puerperal fever.

Dr. Drake makes the following statement, based upon an analysis of the several accounts given of epidemic erysipelas as it has occurred in the Interior Valley of North America:—

"The peritoneum in men and non-parturient women was obnoxious to the inflammation, but not in as high a degree as the pleura. Pregnant, and especially lying-in females were, however, peculiarly liable, and the most fatal cases were the puerperal."

In the terrible epidemic of erysipelas which prevailed near Norristown, Pennsylvania, in the autumn of 1847, "old and young, male and female, fell before it," says Dr. Corson, "and yet there seemed to be one class that it preferred. The mother, as she lay helpless and exhausted from the labour and agony endured in giving birth to her child, was marked as a victim. The deadly poison was infused into her veins, and, in many instances, a few hours sealed her doom." "I lost more puerperal women during the epidemic than for twenty years before." "This epidemic produced in one class of patients

well-marked erysipelas, in another inflammation of the mucous membranes lining the fauces and nasal cavities, and in a third, diffused inflammation of the serous tissues; while yet others were met with, in which all these conditions followed each other, or existed simultaneously." "In females, the serous membranes were affected generally, while in males the mucous or cellular tissues were almost the only parts involved."

In the winter and spring of 1851-52 epidemic erysipelas again made its appearance in the upper portion of Montgomery County, Pa. "The disease," says Dr. Vanbuskirk, "seemed first to attack the throat, and afterwards the surface of the body. In females, it was especially liable to attack the peritoneum, and one or other of the serous tissues in the male. When the peritoneum became affected, there was much hiccough from the disease extending to the diaphragm. In some cases, symptoms of arachnitis, followed by coma, presented themselves." It is further added, that many cases of puerperal fever occurred during the prevalence of erysipelas; and, as far as information was obtained, "these cases of puerperal fever were confined chiefly to the same localities as the latter disease."

Speaking of the epidemic erysipelas as it occurred in Montgomery County in 1852, Dr. Geiger informs us, that "it spared neither age, sex, nor condition." "It marked the parturient woman for its especial victim. Not a single woman living within the range of the disease, who was delivered during its prevalence, escaped an attack." "Besides those cases of puerperal fever, which were evidently erysipelatous, males were frequently attacked with symptoms indicating inflammatory disease in one or other of the internal organs, as the brain, lungs, heart, intestines, and their serous investments."

In the latter part of March, 1852, epidemic erysipelas made its appearance in Palmyra, Lebanon County, Pa. "Few lying-in women," say Drs. Gloniger and Breitenbach, "escaped its attack, and the ratio of mortality, we have been informed, was quite large."

Dr. Bennett, in his history of the epidemic erysipelas which prevailed in Danbury, Connecticut, during the winter and spring of 1847-48, says: "The serous membranes were a frequent seat of the disease, especially the pleura and peritoneum. Three cases of puerperal peritonitis are included in the list."

Dr. Mendenhall, in his report on the epidemics of Michigan, &c. tells us that "erysipelas has prevailed as an epidemic for the last two years (1851 and 1852), usually affecting the head and face." "Puerperal peritonitis prevailed contemporaneously with erysipelas in this region. In some cases, the erysipelas attacked the labia and vagina, and was soon followed by puerperal peritonitis."

But it is unnecessary to multiply evidence to prove that the same epidemic cause which gives rise to erysipelas may also produce in the male, and in the non-pregnant and non-parturient female, peritoneal inflammation, and in the pregnant and parturient woman the disease termed puerperal fever. Every historian of the epidemics of erysipelas that have occurred of late years, with scarcely a single exception, bears testimony of the fact. The intimate connection between epidemic erysipelas and childbed fever—a connection that had been already recognized by Gordon, Beatty, Hutchinson, Nunnely, Kneeland, Holmes, and others—is now, indeed, very generally admitted. Dr. Simpson, of Edinburgh, has recently advanced the opinion that erysipelas and phlebitis are diseases in "the same category as puerperal fever."

Dr. Meigs endeavours to evade the facts above referred to by denying the pathological identity of puerperal fever and erysipelas. But it is by no means necessary to prove that the two diseases are identical in their pathological characters before admitting their production by the same morbid condition of the atmosphere. The difficulty in the mind of Dr. Meigs has originated from the supposition that the epidemic malady, one of the most frequent manifestations of which is an erysipelatous inflammation of some portion of the surface, and the disease known as puerperal fever, are essentially local phlegmasiæ. May we not, with Mr. Nunnely, admit it to be highly probable, if not certain, "that there is," in erysipelas, "some change produced in the state of the blood, which change may depend upon alterations we are unable at present to appre-

ciate, but which, it is likely, occur in many tissues, and may thus affect the mass of blood more or less quickly, and to a greater or less extent, according to the influence they have upon, and the connection they have with, the blood in a state of health."

Dr. Meigs is, himself, not prepared to say "that the epidemy might not have power over the nervous mass, so to qualify its operations as to determine, in one individual, an inflammatory attack of the corpus mucosum of the skin, and, in another, an attack of inflammation of the serous coat of the belly." But he cannot conceive of a case of pure metritis or metrophlebitis being produced by the same cause as that productive of erysipelas, or possibly of peritonitis.

"Erysipelas," he remarks, "is a disease of the skin, and although, in some instances, it does take on a phlegmonous character, by extending perpendicularly downwards into the connecting areolar texture, it does so only by accident, and not as a normal process of that special phlegmasia. Erysipelas is, therefore, a membranous, but not a visceral disorder, and one of its chief characteristics is found in its propensity to expand its areas of phlegmasia far and wide over the plane of the membrane. Puerperal peritonitis, pure and simple, is also a membranous disease, and possesses the same propensity to expand its areas over the entire plane of the peritoneal membrane. In this, erysipelas and peritonitis are alike; but erysipelas and metritis are not alike. Peritonitis is also like erysipelas in this, that it has a tendency to plunge or descend vertically in the basement textures of it, and so destroy the epiploon, or gangrene the bowel, or produce ramollescence of the exterior stratum of the uterus, or exo-metritis. In erysipelas, this vertical plunge or down-sinking of the morbid states of the corpus mucosum often carries it quite through and below the corium, and far down into the substratum of areolar tissue, where it may become either phlegmonous or cedematous erysipelas, as the case may be.

"In like manner, when you shall hereafter examine the mortal remains of individuals who have died of pure childbed peritonitis, though you shall not, in general, observe any other than the results of a purely membranous inflammation, or inflammation of the peritoneum, yet, in some specimens, you may find the epiploon softened and suppurated, the ovaria reduced to a pulp, or the outer stratum of the womb completely reduced to a state of ramollescence or softening. In so far, then, as I have drawn a parallel between the two disorders, you discern a very great similarity between them."

"Why should you vex yourself with this foolish question, when you are already so well informed in your profession as to know that childbed fever is not puerperal peritonitis, and nothing else; and that peritonitis, in numbers of the cases, does not come in question at all, the disorder being pure womb-phlebitis alone, or an oophoritis, &c. &c. If you must insist that erysipelas and childbed fever are one, then pray leave out of question all the pure metritis and phlebitis, and confine your alliance between the two to the serous and dermal identities, if they must be so considered."

We agree with Dr. R. S. Holmes, in the opinion advanced by him, in his recent very able paper on erysipelas, that pathologists have committed an unfortunate error in their efforts to find some one tissue on which the inflammation in erysipelas is expended, while we are convinced that the lesion of several tissues is common, even in the milder forms of the disease. With Dr. Holmes, we hold "that the peritoneum, the pleura, or the arachnoid, may take on the erysipelatos inflammation as certainly as the lining membrane of the fauces; if the disease be constitutional, it, like many others, shows preferences to particular parts, but is not confined to those parts; it can no more be called 'a dermal disease' than it can be called a peritoneal disease. In its signs, it is a peculiar form of inflammation, with characters as strong as an inflammation where lymph is thrown out for adhesions, or pus for a covering or protection. Its pathological exudation is like that of many others, merely a deficiency in its physiological exudation; but, in proportion to the potency of the cause, so will be the power of the exudation to assume the pus formation, or the fibrinous or the simple agglutinative lymph. The pus may show a greater tendency to form on mucous than on serous textures, but that does not exclude the serous, and in proportion to the gravity of the cause will be the result."

We can readily understand, when we consider the condition of the pelvic and most of the abdominal viscera in the female immediately after parturition, why these should be particularly predisposed to the action of the *materies morbi* by which the inflammation in epidemic erysipelas is produced.

Let this be as it may, we have the fact incontrovertibly established that, during the time and in the same place at which erysipelas is prevailing epidemically, males and non-pregnant and non-parturient females are especially liable to suffer from peritoneal inflammation, while pregnant and lying-in women are particularly exposed to an attack of the so-called puerperal fever; and, in the examination of the bodies of those who have died of the latter disease at such periods and places, it is not the peritoneum alone to which the results of inflammation are confined, but the uterus, its veins, its ligaments, the ovaries, and neighbouring intestines were as frequently found involved in disease.

Dr. M. recognizes a low typhoid form of childbed fever as prevalent in over-crowded and ill-ventilated hospitals, but maintains that this form of the disease is either purely typhous fever without implication of the generative organs, or a combination of phlegmasia of these with typhous fever. Of the former cases, nothing need be said, as it is not to be supposed that any well-instructed physician would confound typhous or typhoid fever with puerperal fever.

Puerperal fever is, confessedly, the especial endemic of the lying-in wards of hospitals, and it is there that it presents itself, usually, in its most malignant and intractable form. Now, when it occurs in these institutions simultaneously with typhous fever, erysipelas, and hospital gangrene, are we to admit the conjoint presence of three distinct morbid states of the atmosphere, the one productive of typhous fever, another of erysipelas, and a third of childbed fever, or subscribe to the opinion of Dr. Walsh, that puerperal fever is not a disorder *sui generis*, confined to lying-in women, but "merely an unusual form of a very common disease," being, "in reality, no other than the common infectious fever, complicated with more or less extensive inflammation of the peritoneum;" and, we would add, the womb and its appendices.

Dr. M. would appear to admit that the endemic cause of typhous fevers may give rise to the very lesions on which he considers the so-called puerperal fever to depend.

"I beg you," he remarks, "to understand me as asserting that, while childbed fever is a phlegmasia, and that while there is not, in our nosology, such a thing as a true idiopathic childbed fever, I yet admit the possibility of typhus, jail, hospital, and ship fevers occurring in our class of patients. Some of the cases proceeding to their solution, in recovery or death, without interesting in a particular manner the childbearing organs, or the peritoneum; while there are others that early establish areas of phlogosis, which may or may not take up the mastery in the subsequent progress of the malady."

We fully agree with Dr. M. that the term puerperal or childbed fever "is a false and deceiving one, which ought to be banished from our vocabulary." In our investigations into the true pathology of the disease it is employed to designate, it has materially impeded our progress by confining our attention too exclusively to the condition of the patient as a puerperal female, and to those circumstances, solely, which pertain to her as such. Viewing these not merely as predisposing causes of the disease under consideration, but as causes which give to it a character *sui generis*.

In his sixth lecture, Dr. M. combats manfully the opinion that childbed fever may be communicated or propagated by contagion. We recommend what he has there written, though tinged with a good deal of special pleading, to the candid consideration of our readers. It is true, Dr. M., has not stated the question of contagion in puerperal fever in exactly the form in which we should receive it; nevertheless, his sixth letter, in which it is discussed, is in many respects a very able one. We are ourselves, we confess, inclined to the doctrine of contagion in puerperal fever; we, however, by no means consider it as one indisputably established. Many striking facts have been adduced in its favour; these are deserving of a careful and candid examination; if they do not legitimately

lead to the important conclusions drawn from them, let this be shown, not by denouncing either those who have recorded them, or such as credit their legitimacy, as scribblers, *gôbe-mouches*, sophomore writers, and the like incompetent reasoners and observers, but by exposing the true character and the relationship to the point in question of the facts adduced.

Dr. Meigs would appear to admit that poisonous matters brought in contact with the inner surface of the womb of a newly delivered female, may induce inflammation of the veins or tissues of that organ; and further, that these poisonous matters may be introduced into the womb by the hands of the accoucheur.

"Do I," he observes, "advise my students to soil their hands with the putrilage of an erysipelatous ulcer, and go without precautions of cleanliness to deliver the lady next door? God forbid! As soon would I advise you to plunge a wounded hand into the cavities of a patient dead with peritonitis, and so run a most dangerous and often fatal risk from dissection wounds. If you carry your impure hands, and inoculate the abraded genital surfaces of a woman in labour with the poison of the cavities, you will be as apt to give her a dissecting wound as you would be to have one yourself, if using a cut hand in your necroscopic researches. You would poison her as by the bite of a rattlesnake, and so make her sick, and her sickness would be prone to assume the characteristics of a childbed fever."

And again, speaking of the puerperal female, Dr. Meigs says: "To these I should be very loth to communicate the inoculable virulency of a dissecting wound; not purely because erysipelas and metritis are contagious disorders, but purely because a newly delivered woman, like a bundle of flax, or like a pistol-pan of powder, is likely to be inflamed and go off for the smallest spark."

The fact of uterine phlebitis being produced by the introduction of poisonous matter into the vagina and uterus of parturient females, is incontestably established by a fact mentioned in Routh's *History of the Endemic Puerperal Fever at Vienna*. For a long period, puerperal fever had more than decimated the obstetric patients of the great hospital of that city. It was ascertained that the disease was owing to the cadaveric matter communicated by the medical men and students, who had recently been handling *post-mortem* specimens. Proper precautions being adopted, the disease has been almost banished from the institution. Thus, while in 1846 there were 459 deaths among 3,354 parturient patients, after the careful employment of chlorinated solutions by the medical attendants and students, for their own purification, had been introduced, the deaths in 3,356 lying-in women had sunk to the comparatively small number of 45.

The entire work of Dr. M. abounds in propositions in physiology, the correctness of which, to say the least of them, is very doubtful. It is not our intention, however, to enter upon an examination of them, inasmuch as they have but little bearing, one way or other, upon the important questions involved in the discussion of the pathology and treatment of childbed fever.

A very good description of one of the most common forms of puerperal fever is given in the Letter XV.; and the want of precision in the delineation of all the varying features of the disease, of which it may be accused, is amply compensated by the very interesting remarks of the author, in the ensuing letter, on the diagnosis of childbed fevers.

We cannot follow Dr. Meigs in his prælections on the treatment of these diseases. His therapeutical precepts, though admirably adapted to the control of puerperal fever in its open and sthenic form, cannot be considered as appropriate to the disease in all the aspects under which it presents itself. With Dr. M., childbed fever is, in every case, and under all circumstances, primarily and essentially a phlegmasia of some one, or several, or all of the pelvic viscera, or of the peritoneum. Hence, with him the lancet is the principal, almost the only remedy by which it can be conducted to a favourable termination.

"At the age of sixty-two years," he remarks, "and after a life in which my medical studies, commencing in 1809, and a copious clinical experience, and no little reading of authors, have continued until the present day, I am become immovably fixed in the conviction that, for the cure of childbed fever, blood-

letting is the chief, not to say *sole reliable remedy*. I go further, and say that the dreadful mortality that marks the epidemic forms is discreditable to our class; and I say this, founding it upon the irrefragable truth that the disease is always a phlegmasia, and that it has a stage during which the application of the proper remedy is possible, and that only a lack of proper vigilance, or a lack of perception and judgment, too often allows that evanescent stage to be over and passed, while we, alas! doubt and hesitate, and at length judge wrongly of the duty."

Even in the low typhoid forms of fever, the remedy, according to Dr. Meigs, is still the lancet.

"I have had hospital cases of childbed fever under my care," he tells us, "and I bled them against all protests, and even with fear and trembling sometimes; nor did my professional conduct on those occasions leave in my mind any sting of remorse. I have had many occasions to treat typhoid forms of childbed fever in this city. I bled the patients, and, though I have not cured all, I remain, in a somewhat advanced stage of life and clinical experience, profoundly impressed with the thought that it was the best remedy in my power to offer; and the whole result of my ministry in this way, gives me to-day no heartache, when I examined myself to learn what it is I have done."

The propriety of any plan of treatment, in any given form of disease, is to be based, not upon any hypothetical reasoning as to the pathological character of the latter, nor as to the presumed therapeutic powers of the former. It is only from the results of clinical experience that a definite judgment can be arrived at. How stands the results of clinical statistics in relation to the active depletory treatment of childbed fever? Dr. Meigs has furnished us with no evidence in its favour derived from this source. Even such evidence, in regard to a disease like the one under consideration, which, in its different epidemic and endemic visitations, may assume a sthenic or an asthenic character—a decidedly inflammatory or a low typhoid form—must be taken with a very great deal of caution. In one endemic or epidemic, under an active antiphlogistic treatment, the number of deaths has been comparatively small, while, in another, a similar treatment has been found altogether unsuccessful, if not decidedly prejudicial. To prove the propriety of bleeding in puerperal fever, it is not enough merely to cite the experience, in its favour, of this or that practitioner, without a reference to the particular characteristics of the cases which fell under his care, and the prevailing epidemic constitution of the season. Unfortunately, the disease has been looked upon as an affection *sui generis*, presenting, whenever it occurs, a uniformity in its essential characteristics; hence, as Dr. John Clarke very correctly observes, each author insists upon erecting his own experience into a standard, by which to judge of the descriptions and practice of others.

No description of childbed fever can be given which shall stand as the invariable type of the disease; no plan of treatment can be laid down which will be alike applicable to every case.

Leeching and cupping Dr. M. holds to be of very little account in childbed fever. He says:—

"I wish you to regard me as one very distrustful of leeches and cupping, believing them likely to mislead us, by begetting within us a false opinion that we can, by them, deplete the affected areas—which we cannot do; and also as seducing us to suppose that, because we draw blood only from the capillaries, we are not, in reality, bleeding the woman, but only leeching her. But, I repeat it, I will not presume to aver that, after a good bleeding, it may not be, sometimes, judicious to apply leeches over the ligamenta rotunda as they pass along through the abdominal canals; but I do ask you to think well and distinctly, whenever you are halting between two opinions as to bleeding and leeching the childbed fever patient."

Emetics, Dr. M. believes, can only be useful in those cases where "a bilious or mucous saburra" might lead the practitioner "to fear bad gastric complications of the childbed fever inflammations."

"The majority of puerperal women go into labour," remarks Dr. M., "with overloaded bowels. This oppressed state of the alimentary tube is the cause

of irritation that might well suffice, in epidemic times, to invite the affluxions of an incurable inflammation. Seeing, then, the strong empirical reliance to be placed in calomel as a purge, I consider it very discreet and prudent, in such a case, to exhibit calomel in the dose of ten or fifteen grains, after having premised the *indispensable venesection*; and, to insure a cathartic operation after calomel, where the meteorism is not already great, few things are preferable to Chaussier's mixture of syrup of rhubarb; two ounces of the syrup with one of the oil gives a mixture, of which half a fluidounce is a proper dose—to be repeated every hour or two hours until the bowels are freely enough moved."

To this extent Dr. M. would seem desirous of limiting the employment of mercury in childbed fever. It is true that when, after the violence of the disease has been conquered by the lancet, some fever still remains—some local turgescence; some rebellious engorgements—and the sullenly retiring inflammation, that has been conquered but not wholly destroyed, may yet demand further proper remedies, he admits that, "in consultation practice," he "would not object" to the administration, every two or three hours, of a grain of calomel, two of sulph. antimon. aurant., two and a half grains of nitre, and one and a half of camphor, in combination. By the slight nausea produced by this combination, he would hope to moderate the force, volume, and frequency of the pulse; to augment the functions of the skin; and, at the same time, by its action on the mucous, biliary, pancreatic, and urinary secretions, ameliorate the circulation in the meseraic, hepatic, and emulgent system of vessels.

We recommend an attentive perusal of the twenty-first to the twenty-sixth letters inclusive, in which the author discusses the use of emetics, of mercurials, of purgatives, of opium, turpentine, sudorifics, blisters, and external rubefacients, stupes, etc., in childbed fever; they are replete with sound practical precepts, from which all may receive instruction. In our estimation, the one great error which pervades the teachings of Dr. M., on the therapeutics of puerperal fever, consists in his insisting that, whenever inflammation of the pelvic or lower abdominal organs occurs in the puerperal female, no matter by what causes, or under what circumstances, this inflammation has been called into being, we are to look to bloodletting as the chief, not to say sole reliable remedy for its subduction.

D. F. C.

ART. XVIII.—*A Short Exposition of the Circulation and Nervous System, with reference to Disease and Treatment.* By G. HAMILTON BELL, F.R.C.S.E.; formerly of the Madras Medical Establishment. 8vo. pp. 67. London, 1854.

THE author of this short exposition informs us, that he was early led to doubt the existence of a muscular power, properly so called, in the arteries, and, consequently, to place no reliance on pathological theories founded on the assumption of an independent power of action in those vessels.

"The fibrous coat of the arteries is," he remarks, "that to which muscularity is ascribed. It is evident, however, that such a quality would confer on the vessels distributing the arterial blood independent powers of action, and expose the circulation to the risk of irregularities wholly inconsistent with its requirements. The term *tonicity*, which the study of the structure and function of the artery has led modern physiologists to adopt, appears to me to express, without the risk of misapprehension, the resistant and elastic qualities of these vessels. It is entirely dependent on the power of the heart, supplying to that organ all the assistance muscular action in the vascular system could give, in maintaining and regulating the flow of blood; the slight annular yielding affording a modified resistance to the heart's systolic force, while the longitudinal stretching threads the blood, as it were, on the retraction of the vessel during the diastolic cessation of the *vis à tergo*, rendering the flow of the blood continuous."

It would appear, Mr. Bell remarks, that the *elastic* property of tonicity does not extend to the minute arterial branches.

The means by which the capillary vessels influence the blood flowing through them, according to Mr. Bell, unlike the elasticity of the arteries, is not dependent on the action of the heart, but is one of those properties which, in lieu of a better term, are called vital.

"The capillaries do not pulsate, and, without appreciable alteration in their caliber, are enabled to accelerate, retard, or even to suspend the current of the fluid they circulate. In their normal state, therefore, they must be capable of resisting the interference of the cardiac force; and when they no longer possess this power, they are injected with red blood, and inflammation, attended with pulsation, heat, &c. is established."

Mr. Bell rejects all the ordinary explanations adopted by physiologists of the cause of the motion of the blood through the veins, as false or unsatisfactory.

"It is, however," he admits, "more easy to prove that others have failed to account for the continuous flow of blood in the veins, than to determine what is really its source, and I am quite aware," he adds, "that I make no great step in elucidating the point by ascribing this important function mainly to the agency of a principle of which we know nothing but by its effects; such, however, is the result of all attempts to account for living phenomena; we must at last reach the *fons ignotus*, and I am quite aware that I have arrived at this point when I attribute the movement of blood in the veins to the principle of *vitality*."—"In contending that the venous circulation is in a great degree under the influence of vitality, is only extending to that portion of the vascular system the influence of a power which is admitted to pertain to those vessels in which the veins originate."

The circulation of the portal system of veins is adverted to, as an instance in which the vena portæ distributes blood to a great glandular apparatus, little inferior in bulk to the lungs, without muscular aid, and thus affording evidence of the power ascribed to vitality.

So much for our author's views in regard to the circulation, which, while they possess but little novelty, advance in no degree our knowledge of this interesting physiological problem. We come, now, to the doctrines inculcated by him in relation to the nervous system with reference to pathology and therapeutics:—

"Although," he remarks, "the nervous system in man, as a whole, is under the influence of the brain, which, no doubt, by nerves appropriated to the purpose, is enabled to harmonize the whole complicated economy of the frame, still, the characteristics of the progressive development, from the inferior to the more perfect structures, seem to be retained, in so far that there are distinct classes of nerves, acting on different departments of the system, each liable to be influenced by diseases, accidents, or medicinal agents, which do not act directly on the several classes of nerves.

"Following out these views, I find four divisions of nerves, viz: the Sensorial, the Respiratory, the Motor-Organic, and the Sympathetic, or Secerning-Organic.

"1. The Sensorial class of nerves includes the senses, the nerves of sensation, and those controlling voluntary motions.

"2. The Respiratory nerves are those by which breathing is maintained as an *involuntary* function.

"3. The Organic motor nerves are those which act on the organic muscles.

"4. The Sympathetic, or Secerning-Organic system of nerves, acting on non-muscular vessels, is the principal agent in effecting digestion, assimilation, secretion, nutrition, and excretion."

The correctness of the above classification of the nerves, the independent sphere of action of each class, and its susceptibility to morbid impressions and the action of remedial agents, which do not influence directly either of the other classes of nerves, Mr. Bell has endeavoured to establish by a reference to certain pathological phenomena, and to various physiological experiments recorded by different writers.

The reference of each of the several functions belonging to the living or-

ganism to the agency of a particular class of nerves, is certainly a very beautiful and attractive theory, and simplifies very much the study of physiology. When, however, it is subjected to a close analysis, and tested by the phenomena developed in disease, or by a series of direct experiments sufficiently varied to obviate the erroneous conclusions so liable to be arrived at from the study of nervous influence in a single and limited point of view, the theory is found to be encompassed by too many difficulties to admit of its being received except to a certain very limited extent.

The existence of nervous influence as the exciting and controlling agent of many of the vital actions can scarcely be doubted; there are, however, on the other hand, many of the acts constantly performed during life which cannot be shown to be the immediate results of the influence derived from any particular class of nerves.

We present to our readers the following quotations from the section of the work before us, which treats of the sympathetic or secerning nervous system, or that on which depends assimilation, nutrition, secretion, and elimination, "in so far as nervous influence is engaged in those processes." They will show the manner in which the author presents and enforces his views in reference to what may be considered the most hypothetical portion of his doctrine of nerve action.

"Whether the semilunar ganglia and solar plexus shall be regarded as endowed with an independent *vis nervosa*, or be viewed as possessing merely delegated powers from the brain, it is clear that in a normal state of the frame the organic secerning nervous system is not interfered with by that organ.

"The ramifications of this system are not less extensive than those of the cerebral nerves; its branches being distributed wherever there is a vascular structure—with the exception, perhaps, of the brain itself, in which the blood-vessels are not proved to be accompanied by branches of the sympathetic system.

"The importance of this great division of nerves to physiology and pathology is sufficiently evident; and I believe it to possess, in addition to its admitted influence over secretion, nutrition, &c., the power of regulating the flow of fluids in non-muscular vessels, and of influencing, by a reflex action, the organic muscular fibre. Its conditions, therefore, are worthy of constant consideration in the treatment of disease."

"The alimentary canal has a succession of functions to perform on which the strength and health of the body depend, and these call for secretions, defecation, absorption, and muscular action, in turn, as the exigencies of the system may demand.

"Thus, assimilation requires material prepared for its purposes by mastication, a normal state of the mucus of the alimentary canal, the secretions of the liver and pancreas in due quantities, an energetic state of the lacteals, and a controlled muscular action in the intestines. All these requisites, however, are not continuously called forth, nor in regular order, but the whole is occasional—and even then one process may be more in request than any of the others; defecation may be slow, or the lacteals inactive, so that it may be necessary to retard the progress of the aliment in the canal; or, from the nature of the ingesta, a greater supply of blood than usual may be required by the capillaries. In short, the *succession* of processes involved in assimilation, as in every secretion, could only be effectually promoted by a *controlling power*, and this unquestionably must be nervous.

"In the sensorial system there are sentient and motor nerves which, although in one sheath, do not act on each other. Thus, if the hand have been unconsciously brought into contact with hot iron, the skin may be destroyed before the nervous circle is completed; the sentient filament having to communicate with the brain before the motor nerve is called into action. In the organic nervous departments, a similar arrangement appears to me to exist; but the brain is not in the circle; and there is not, in my mind, any doubt as to one use of the ganglions on the spinal nerves being to effect this required communication between the two classes of the organic nerves—the sensitive sympathetic, in the same sheath with the motor nerve of the spinal ganglionic system,

is thus enabled, as in the case of the cerebral system, to excite or regulate the intensity of organic muscular action, without going beyond the ganglion appropriated to the locality. And it is probable that this sensitive nerve may be enabled to extend its action beyond the ganglion, so as to come into communication with the sentient portion of the cerebral system, and so bring the state of the viscera in disease under the cognizance of the brain.

"It will be seen, by referring to Sir B. Brodie's experiments with the *upas antiar*, that although the heart had ceased to act, its cavities were distended with florid and dark-coloured blood. In another of his experiments with that poison he says: 'On opening the thorax, I found the heart to have ceased contracting. It was much distended with blood, and the blood in the cavities of the left side was of a scarlet colour. There were two full inspirations after the incision of the thorax was begun.' Proving that the power which effects the circulation in the veins, had retained its influence after the heart had ceased to act on the blood: The *upas antiar*, therefore, and other poisons which act similarly on the motor organic class of nerves, do not affect the sympathetic system, which continues to excite the current of blood in the veins until the gorged and silenced heart interrupts its further progress.

"A fit of ague is ushered in by lassitude and debility; the nails, on examination, are blue; the lips and cheeks soon become livid; the whole periphery shrinks; the pulse is thready, and the action of the heart feeble; the breathing hurried and oppressed; a general state of rigor ensues, accompanied often by cramps; and, for a time, all attempts to restore the natural temperature of the body prove unavailing. The balance of the circulation is upset, the arterial blood is deficient, and the veins are loaded. This *disease* (for the *ague* is truly the disease) generally after a time yields, the circulation of the veins is restored, oxygenated blood once more reaches the heart, colour, heat, &c. return; but the heart is supplied in excess, reaction is the result, and what is called the 'hot stage' of the intermittent is established.

"Reaction may not, however, take place; the 'cold stage' continuing, torpor and coma follow, and death, from venous congestion, is the result."

"Cholera asphyxia affords an example of a disease arising from venous remora, which still more strikingly illustrates the point I wish to demonstrate. In cholera, as in ague, the patient may discover the accession of the disease by the blueness of his nails; or even when still engaged in his usual occupations, his friends are struck by his sunken features and livid colour; he may scarcely admit that he is ill; his pulse, however, will be found thready—it may even be imperceptible; his skin will be deadly cold; he will own that he has had frequent motions, and cramps in his feet or hands;—in short, he is far advanced in the fatal disease. Cholera and ague differ; in the absence of shivering in the former, and of the characteristic gastro-enteric evacuations in the latter.

"In cholera, from the outset, the arterial circulation fails; there is suspension of the arterial secretions—tears, saliva, urine, &c., and the *post-mortem* examination exhibits empty arteries and gorged veins.

"Now, in both of these diseases, the patient retains the energy of the sensorial and respiratory powers. In cholera, long after the pulse has ceased to beat, and when the flutter of the heart can scarcely be perceived, when the sufferer is like a corpse, he will give you his hand, and answer you quite collectedly. As, however, the extent to which the system is affected, and the extent of the functions that have failed, point evidently to the loss of a *nervous power*, we must turn to the organic nervous divisions in search of the immediate cause of diseases, which we must conclude belong to the systems of organic life.

"The pathognomonic symptom of cholera is in the characteristic abdominal evacuations, which also presents a pathological difficulty. For, after all secretion has ceased, excessive quantities of watery fluid are discharged from the alimentary canal, and after death the same fluid is found loading the intestines. Even if there were not otherwise sufficient evidence to prove that secretion, properly so called, had ceased from the accession of the disease, the fact of empty arteries and the absence of arterial blood, are sufficient to justify the conclusion, that the cholera ejection is not the result of secretion."

"The active state of the gastro-enteric muscular fibres sufficiently proves that in so far as their action may depend on the organic motor nerves, that system retains its energy. The heart, from not being duly supplied with blood, is, of course, in a state of inactivity. There is, however, an experiment of the greatest interest, which proves that the heart in cholera is only idle from the deficient supply of blood."

The allusion here is to the restoration of the action of the heart, in cases of cholera, by the injection into the veins of a weak solution, in water, of the muriate and carbonate of soda. A case, in illustration, is detailed; in relation to which the author remarks:—

"The change, almost instantaneously effected, from a livid whispering corpse, to a florid, strong-voiced man, is truly marvellous, and the release from terrible suffering to ease very gratifying. It may be likened to Sir B. Brodie's restoration of life by artificial respiration after poisoning with woorara, with this difference—that the poison suspends the sensorial and respiratory powers, but leaves the organic division of nerves uninjured; whereas, cholera suspends the energy of the sympathetic system, without directly affecting the sensorial and respiratory classes of nerves; and, as is proved by the above *experiment*, and by the active state of the muscular coat of the alimentary canal, throughout the disease, the actions normally under the influence of the organic motor nerves, are not destroyed by cholera.

"Sir B. Brodie, after three hours of artificial respiration, overcame the otherwise mortal injury of woorara on the animal nervous system. In the above quoted case of cholera, after persevering for eleven hours, in the course of which, twenty-six pounds of salined water were injected into the veins, the morbid condition of the circulation was subdued, and the patient saved from *dying of cholera*."

In the section which follows that from which the foregoing quotations are derived, Mr. B. presents his views of the nature and object of the portal venous system, including, under this term, all the veins of the abdominal viscera.

He adverts to the fact that the ejections in cholera consist mainly of the serum of the blood in an unaltered state, which, in a bad case, is so completely drained from the system, that the remaining blood is of the consistence of tar.

"This excretion," he remarks, "continues to be discharged with great force after the prostration of strength is so great, that the sufferer, with his livid colour, death-like coldness, and sunken features, is more like a subject experimented upon by galvanism, than a living body! In those circumstances, the circulation having been long at an end, the question is, whence is the enormous quantity of serum derived? It is probably poured into the intestines by the capillary vessels. But how does the blood reach these, to enable them thus to drain off the serum from the crassamentum?"

"This is a point which I regard as one of very great importance; not with reference only to the extraordinary disease of which it is so prominent a feature, and to the mortality of which it so much contributes; but as affording evidence of a venous determination of blood to the alimentary canal, which may not only occur in disease, and under the influence of medicinal agents, but which may exist in a normal condition of the system. In health, affording the means of ridding the venous blood of the more peccant colluvies—and fulfilling the office of removing the results of incipient decomposition in an ever-changing frame—which the veins convey for the purpose of being discharged from the body.

"When too large a dose of croton oil is taken, there very quickly ensues violent action in the gastro-enteric canal—a great quantity of watery fluid is discharged from the stomach and intestines, unaccompanied by arterial excitement—the reverse, indeed."

In Dr. Christie's case, the muco-serous discharges from the bowels were accompanied by a scarcely perceptible pulse, cold extremities, and contracted features; in addition to which Mr. B. has noticed cramps like those of cholera.

"The determination," says Mr. B., "of the requisite material to secreting organs, is a subject of the greatest interest. The rapid secretion of the kidneys under the influence of mental anxiety, is illustrative of the extent to which their function may be excited in health. While, in a diseased state of the discerning

process, as in diabetes, a quantity of urine may be voided in a given time, more than double the whole ingesta." "In these circumstances, the secreting organs must not only be sufficiently supplied with blood, but we must conclude that there exists an *elective* principle, capable of determining the pabula of the morbid secretion to the kidney, analogous to what takes place in galvanic decomposition, in which a component portion of a substance is attracted to the positive or negative pole.

"It is probable that the excessive quantity of fluid which continues to be discharged from the intestinal canal in cholera, after the circulation of the blood has ceased, is derived from the veins generally, by a process of *determination*, and not merely drained from the gorged mesenteric vessels.

"It is not, I think, overstrained analogy to refer to galvanism in attempting to explain vital processes; and I trust I may be excused entering more fully into this point.

"Elimination in all its phases evidently depends on the agency of the sympathetic nerves, which are so freely distributed to every bloodvessel in the body. Secretion may not require more than that the *power* controlling the capillary circulation should be enabled to regulate the supply of blood according to emergencies. Excretion is a totally different process. The object of this function is to expel from the system the effects and injurious products of vital processes, contained in the blood, and we may conclude that it would not suffice to purify merely the portion of the blood circulating in the emunctories, but that it is necessary that the colluvies to be discharged should be determined from every quarter of the circulation to the emulgent locality.

"Galvanic action, in decomposing an alkaline sulphate in solution, determines its acid base to the positive, and its alkaline to the negative pole; and the acid may even be made to pass through an alkaline medium without the usual chemical combination resulting.

"If, therefore, the speculation I have ventured to propound be admitted, viz: that the excretory function of the enteric mucous membrane is effected by the mesenteric veins—and if the above illustration be apposite, a retrograde current in the portal veins would not be indispensable."

Mr. B. treats next of the curative action of mercury. He supposes that, in addition to its admitted influence on the secretion of the liver, it acts as a *tonic* on the capillaries of the gastro-enteric mucous membrane, and, also, as a stimulant to the excretory function of the bowels.

"Mercury," he remarks, "reduces the vascularity of the enteric mucous membrane, acting of course on the arterial capillaries. And while it increases the hepatic secretion, it stimulates the emunctory function of the intestines—proving its influence over the entire portal system. We have thus a combination of actions, only to be accounted for by admitting the interposition of nervous agency. And my hypothesis is, that the therapeutic effects of this medicine are to be attributed to its acting as a *stimulant* on the sympathetic system of nerves.

"A case of dysentery treated with mercury, appears to me to illustrate this conclusion. The violent attacks of this disease, so common in India, are attended with indescribable agony—tormina, tenesmus, continual urgent alvine calls, scarcely permitting the patient to leave the *commode*—small quantities of blood and mucus being passed with terrible straining, and without relief; fixed pain in a part, or in the whole course of the colon; there is high fever, and alarming debility. In such circumstances, a scruple of calomel will act as if it were an anodyne; and the second or third repetition—every five or six hours—will completely master the disease.

"The *ratio symptomatum* in dysentery, and the *modus medendi* of mercury may, I think, be satisfactorily explained on the views I have been endeavouring to illustrate. Thus, there are inflammation and depraved secretion, discharge of blood from the distended capillary vessels of a portion of the enteric mucous membrane; and, whether as cause, or effect, the discerning nervous system is involved in the morbid state of the discerning surface. The motor-organic system of nerves is communicated with, and the muscular coat of the intestines is thrown into diseased action, producing, from the inflamed state of the intestines,

tormina and tenesmus; the brain is brought into cognizance of these morbid changes, through the sensitive branches of the sympathetic, and the sentient nerves in the spinal column, the normal functions of the portal venous system being suspended by the influence of the disease on the sympathetic class of nerves. Mercury, in reducing the vascularity of the enteric mucous membrane, overcomes the inflammation, stops the discharge of blood and depraved secretion, and, in restoring tone to the portal system, re-establishes the excretory function of the bowels—thus responding to all the indications of treatment in dysentery.”

We cannot follow Mr. B. in all the practical deductions he has drawn from his doctrines in reference to the circulation and nervous system. The following remarks, which we quote from his views in regard to the treatment of fever, will give our readers a very fair idea of this portion of the work.

“We may ascribe a *vis medicatrix nature* to the fact, that, while one department of the nervous economy is morbidly affected, life is preserved by the other classes of nerves being free from disease. Thus the sensorial and respiratory systems perform the duty of a preserving power in cholera, while the energy of the sympathetic system is suspended. In fever (continued), the sensorium being at fault, assistance must be sought for in the organic classes of nerves. In the treatment, therefore, while the condition of the head requires constant attention, the object should be to counteract the abnormal influence of the brain on the secerning and emunctory functions. The observations and illustrations of the therapeutic powers of mercury, as a tonic to the sympathetic system, on which I have enlarged in a previous portion of this essay, are sufficient, I think, to justify the efficacy which I ascribe to this medicine whenever the purpose is to restore the functions of the alimentary canal.

“It has been proved that mercury, while it determines to the portal system, stimulates the emulgent function of the intestines. This medium, therefore, responds to the most important indications in the treatment of fever—it relieves the vessels of the brain, and discharges from the system the accumulated impurities, resulting from suspended emunctory processes.”—

The mode of prescribing mercury in continued fever, which in his own practice Mr. B. has generally found successful, he thus describes:—

“Having in a case of continued fever resorted, when advisable, to an emetic, and prescribed a dose of calomel, followed by a saline purgative, I immediately commence the persevering use of calomel in very small doses, generally combining it with antimony. Regulating the doses of the calomel, and of James’s powder, from a sixth to the eighteenth part of a grain of each (according to the age of the patients), and administering them every two hours, night and day—being careful that they are thoroughly triturated with refined sugar, before division. Small as these quantities are, their persevering use thus prescribed, soon produces frequent very dark-coloured, and offensive alvine motions. As the case advances, the character of the evacuations improves—the tongue is less dry, and begins to clean at the edges; the pulse becomes more steady, the skin softer, the sleep is less disturbed; and even, when as yet there may be no greater evidence of improvement in the cerebral condition, it is clear from the improved state of the emunctory functions, that the organic nervous system is now less under the influence of the distempered brain, and that it may be depended upon as a *vis medicatrix* in the further progress of the treatment.

“It is not, however, only in synochus and typhus that minute doses of calomel and antimony prove invaluable; the above mode of exhibiting these medicines, acts almost as a specific in the exanthemata and infantile fever. I cannot give better evidence of this than by stating the fact that, in twenty-five years of extensive family practice in this city, and usually in charge of three or four private seminaries for education, while I have had no casualty from measles, I have only lost three cases of scarlet fever—in two of which the fatal event occurred within a few hours of the accession of the disease, which was only ascertained to be from scarlatina, by similar symptoms, viz: sudden sinking, in other members of the family, terminating in virulent attacks of the malady.

“I trust it will be understood that, in advocating the useful qualities of a remedy, which I believe to be of the greatest value in the treatment of disease,

I do not overlook the importance of attention to the state of the head in fever, of the fauces and surrounding glands in scarlatina, of the chest and head in measles, and above all, the indispensable necessity of watchfulness in every serious malady, especially during infancy."

We may, perhaps, be accused of giving undue importance to the purely hypothetical views of Mr. B., by the copious extracts we have made from the work before us. We admit that the author has failed to establish in a satisfactory manner his leading physiological and pathological views; and that many of his peculiar doctrines in relation to the nervous system and the circulation, and the application of these to the explanation of the pathological conditions of the organism, and of the curative action of remedial agents, will not, probably, stand the test of close investigation and careful observation; still, the essay is one replete with interest; in the course of it many suggestive propositions are thrown out, the full development of which would, we are persuaded, lead to highly interesting results in the domain of both pathology and therapeutics.

D. F. C.

ART. XIX.—*Human Physiology: Designed for Colleges and the Higher Classes in Schools and for General Reading.* By WORTHINGTON HOOKER, M. D., Professor of the Theory and Practice of Medicine in Yale College; Author of "Physician and Patient." Illustrated by nearly 200 Engravings. New York, 1854. 12mo. pp. 389.

THE physiology of the human frame—the vital processes and endowments of that curiously constructed organism by which we live, and move, and have our material being—is a study that recommends itself to every one; not merely from the deep personal interest we must all necessarily feel in whatever relates to the mechanism of our own bodies—the wonderful series of living processes necessary to their full development and continued existence, and the mysterious indwelling motive and controlling power to which all their parts are constantly subjected—but also from the fact that it is only from a knowledge of the vital laws of the human frame that we can acquire a just appreciation of the means that are adapted to preserve both mind and body in a state of healthful activity to the latest possible period.

Whether, therefore, physiology be viewed merely as an attractive and important branch of natural history, or in its directly practical application to the elucidation and enforcement of a correct system of hygiene, it is proper that it should be made a regular branch of instruction in all our schools, public as well as private.

Heretofore, the want of a treatise upon the subject, well adapted for the purpose of school instruction, has been an obstacle in the way of introducing the study of physiology as a constituent part of our educational course. This obstacle, we are happy to say, has been removed by Dr. Hooker in the preparation of the work before us. In it will be found detailed, with great clearness and conciseness—but at the same time with sufficient fulness—the leading facts in relation to the physiology of man—all of which are admirably illustrated by a series of engravings adapted to impart to the learner correct ideas of the several subjects treated of. Dr. Hooker has, as far as possible, avoided the introduction of technical terms, and whenever he has been obliged to make use of them, he has explained them in such a manner as to obviate the necessity of a glossary. The more obscure and difficult parts of the science he has endeavoured to render plain and intelligible by full illustration—by divesting them of all speculations and hypotheses, and by so arranging them that the mind of the student may be prepared, by the preceding details, for their proper comprehension.

The work is divided into three parts. The first, which has been made as short as possible, is *preliminary* to the consideration of the particular subject of the treatise. In the second part, the human structure is presented, simply

as a *structure*, with the manner in which it is constructed and kept in repair. In the third part, all those subjects which relate to the *uses* for which the structure is designed, are treated of. "This natural division of the whole subject, not only presents it to the mind of the student in an interesting point of view, but secures that natural gradation in its development necessary to a clear understanding of its deeper and more intricate portions."

We recommend the work of Dr. Hooker to the unprofessional student and general reader as one well adapted to impart to them correct views of the construction and vital laws of the human organism. We should be pleased to see it introduced as a text-book, in the more advanced classes of all our public schools, as well as in the High School of this section. D. F. C.

ART. XX.—*Notes of M. Bernard's Lectures on the Blood, with an Appendix.* By WALTER F. ATLEE, M. D. Philadelphia: Lippincott, Grambo, & Co., 1854. 12mo. pp. 224.

THESE notes—placing, of course, full confidence in the accuracy with which they have been taken—are particularly interesting. M. Bernard is unquestionably one of the most industrious and acute physiological inquirers of the present day. On many important points connected with the condition, changes, and properties of the blood, and the forces to which it is normally subjected, his investigations have already thrown much light.

His lectures on the blood—of the leading features of which the notes of Dr. Atlee may be considered as a very brief outline—in connection with those of M. Robin, a few brief notes of which are given in the appendix, present not merely an instructive *resumé* of all that other labourers in the same field of inquiry have established as truths in relation to the anatomy, physiology, and pathology of the blood, but the especial views on these important subjects deduced from his own investigations. The notes of Dr. Atlee are, it is true, much too loose and concise to satisfy such as desire to make themselves fully acquainted with the particular views of MM. Bernard and Robin, or who would study the physiology of the blood in all its details and bearings. They will, nevertheless, amply repay the reader for the time spent in their attentive study, presenting, as they do, a general view of some of the leading opinions promulgated by two of the most eminent living authorities in biology, on subjects which, at the present day, are admitted by all to be of prominent interest and importance in medical science. D. F. C.

ART. XXI.—*A Manual of Pathological Anatomy.* By C. HANDFIELD JONES, M. B., F. R. S., Fellow of the Royal College of Physicians, Assistant Physician to, and Lecturer on Physiology at St. Mary's Hospital; and EDWARD H. SIEVEKING, M. D., Fellow of the Royal College of Physicians, Assistant Physician to, and Lecturer on Materia Medica at St. Mary's Hospital. First American edition, revised. With three hundred and ninety-seven illustrations. Philadelphia: Blanchard & Lea, 1854. 8vo. pp. 733.

THIS work supplies a want that has long been experienced—a comprehensive manual of the actual condition of pathological anatomy—a summary of ascertained facts within that department of medical inquiry which has for its object the various forms and conditions of morbid structure, with the views of the leading authorities as to the manner in which these facts are to be interpreted in the study of diseased action and its results.

Of the entire subject, extensive though its scope, and vast the accumulation

of materials in all its various departments that have been furnished by the industry of its recent cultivators, the authors of the work before us have presented a very accurate and instructive outline. It may be considered, in truth, one of the most complete manuals of morbid anatomy, if not the only one in the English language.

The arrangement of the several points of inquiry embraced in the general subject of pathological anatomy, adopted by Drs. Jones and Sieveking, is, first, the consideration of the several diseased conditions of the blood; then, those of the various tissues; after this, the superadded formations or growths—the so-called tumours—are described; and, finally, the parasitic beings of the human body, whether animal or vegetable. These make up what may be termed the general department of pathological anatomy—they occupy the first five chapters of the *Manual*.

The remaining thirty-eight chapters are devoted to an examination of the several abnormal states developed by disease, in the nervous system, in the circulatory, respiratory, digestive, urinary, and generative organs, in the joints, and in the osseous system.

In their investigation of the morbid states of the blood, the authors discuss the subject of fluxes and dropsies; that of hyperæmia and inflammation, and those of pyæmia and the different crases of the blood. The only one of the latter, however, that is treated of at any length, is the tuberculous.

Under the head of new formations, the subject of cancerous or malignant tumours is discussed.

Mr. Jones, by whom this portion of the work was prepared, remarks that his own examinations "have most thoroughly convinced him of the non-existence of any special structural character, absolutely and in all cases distinctive of cancer.

"This point," he adds, "which is in accordance with the teaching of the best authorities, seems far from being correctly understood in the present day, and we cannot but think that there is still much tendency to overestimate the microscope as a means for the diagnosis of cancer. It is our opinion that the cases are very rare indeed where the microscope will avail to detect cancer, with any certainty, where the naked eye features are insufficient. On the other hand, we have more than once seen unquestionable cancers made up of substance which we should have been led, from microscopic examination alone, to consider as of a simple nature."

The subject of cancer structure and its microscopic diagnosis being one, confessedly, of deep interest, the editor of the present edition of Drs. Jones and Sieveking's *Manual* has added the illustrations of cancerous structure from an interesting paper on the subject by Dr. Francis Donaldson, of Baltimore, Md. This presents a very fair view of the opinions of those who maintain, in opposition to Mr. Jones, the existence of a special structural character in all true cancerous growths.

Were we to enter upon a formal criticism of the different sections of the work before us, we should be inclined to differ from the authors in regard to the correctness of some of the conclusions at which they have arrived in reference to the character and mode of production of certain of the pathological changes that have been detected in the fluids and tissues of the animal organism. Taken as a whole, however, the *Manual* of Drs. Jones and Sieveking presents so accurate and instructive an outline of what is known in the domain of pathological anatomy, that we feel no hesitancy in recommending it to the favourable notice of our medical brethren. For the use of the student, it will be found peculiarly well adapted; while, as a compendium of the leading facts known in relation to the structural and other changes resulting from the different forms of diseased action, its pages may be consulted with profit by the physician also.

On the several topics embraced within the general scope of the branch of medical inquiry of which it treats, it will always instruct, and seldom mislead those who resort to its pages for information.

The present edition of the *Manual* is illustrated by two hundred and thirty wood engravings, in addition to those contained in the London edition. This we consider a very decided improvement. In a work intended mainly as a

text-book for the student, well-executed pictorial illustrations of the subjects of which it treats, whenever they can be introduced, are great assistants in enabling him to form a correct appreciation of the latter, even though they may be wanting, as simple wood-cuts, inserted on the same page with the text, must necessarily be, in the proper elements of size and colour, so important in illustrations of pathological anatomy. The additional illustrations are all well selected, and, in common with those in the original edition, are admirably executed. They portray the subjects they are intended to represent as accurately, perhaps, as this can be done by simple drawings in black and white.

D. F. C.

ART. XXII.—*The Principal Forms of the Skeleton and of the Teeth.* By Professor R. OWEN, F. R. S., &c., Author of "Odontography;" "Lectures on Comparative Anatomy;" "Archetype of the Skeleton;" "On the Nature of Limbs;" "History of British Fossil Mammalia," etc. etc. Philadelphia: Blanchard & Lea, 1854. 12mo. pp. 329.

THIS little work, whose author is "the most distinguished osteologist of the age," is the first attempt which has yet been made to popularize that difficult but highly interesting and important department of anatomical science, the determination of the like parts, or homologies—as Prof. Owen has expressively called them—in the skeleton of all vertebrated animals.

In determining the homologies of the skeleton, Prof. Owen found the necessity of constructing a new nomenclature, applicable to the same parts in all their modifications of form and position throughout the series of vertebrate life. This has been done in so admirable a manner, that the terms not only define the same parts in all skeletons, but they also, in most cases, express the physiological function of these parts. This philosophical nomenclature is adopted in the above work, and very much contributes to the simplicity and interest of the study of which it forms an essential part, and which has always been the most unsatisfactory and confusing in previously written works on comparative osteology. To the commencing student of the latter subject, this book will be found an invaluable aid as an elementary treatise, and to the student of human anatomy and physiology it will give an interest and an insight into the value of many points of human osteology which have hitherto almost entirely escaped notice.

J. L.

ART. XXIII.—*Anatomical and Physiological Observations.* By JOHN STRUTHERS, M. D., F. R. C. S. E., Lecturer on Anatomy. Part I. Edinburgh, 1854. 8vo. pp. 239.

THIS volume consists of a collection of distinct papers—sixteen in number—by the author, containing interesting observations in human anatomy and physiology, of which most of them are on anomalies of structure. Among those which strike us on a hasty perusal, are: I. Observations on the existence of a supra-condyloid process of the humerus. III. On double stomach. XI. On rudimentary ribs. XIII. On diverticula from the small intestine. The author also gives a list of the papers which form the matter of the forthcoming Part II. of the work.

Books of this character are of much service to the working physiologist, as they present him accumulative facts, which may serve as a basis of important laws.

J. L.

ART. XXIV.—*A Dictionary of Medical Terminology, Dental Surgery, and the Collateral Sciences.* By CHAPIN A. HARRIS, M. D., D. D. S., Professor of the Principles of Dental Surgery in Baltimore College, &c. &c. Philadelphia, 1855. 8vo. pp. 800.

THE first edition of this work, published in 1849, with the title of "*A Dictionary of Dental Science, Biography, Bibliography, and Medical Terminology*," was designed to supply an important desideratum to the dental profession—satisfactory definitions of the technicalities of the dental art, and a compendium of necessary and curious collateral information. "The several medical and surgical dictionaries," observes the author, in his preface to it, "however well adapted to the wants of the student and practitioner of general medical science, contain no information on the subject of Practical Dentistry. Operations of every kind, except those on the teeth, are there carefully described, and all manner of pathological affections examined and illustrated, except those peculiar to the organs of mastication." Accordingly, Dr. Harris, in his Dictionary, in the department of medical terminology, "confined himself, for the most part, to mere definitions, but upon all subjects connected with dental surgery proper," he treated in the fullest manner.

In the preface to the present edition he states: "In the preparation of the first edition, he omitted many of the terms" belonging to medicine and the collateral sciences, "fearing they might be regarded as out of place in a lexicon designed principally for the student and practitioner of dentistry." Subsequent reflection has convinced him that a more extended view of the subject was necessary, since the scope of professional education for the dentist has become so widened that general medicine and collateral science are now, to a considerable extent, embraced in the curriculum of dental study. He has, therefore, introduced into the present edition, not only the words and phrases purposely omitted in the first, but also those that have subsequently been added to the literature of the above-mentioned departments of science. * * * Further, he adds: "In these departments of the work he has confined himself, for the most part, to mere definitions, but on all subjects connected with dental surgery proper, as well as with the anatomical structures, diseases, treatment, and operations on the mouth and adjacent parts, this Dictionary will be found very full." To avoid any considerable increase in the size of the volume, the more elaborate articles have been rewritten and compressed, and the Bibliographical and Biographical departments entirely omitted.

Such are the alterations and improvements introduced into the present edition, and which make it more complete without changing its original character.

A friend, who stands among the first in this specialty, assures us that "upon all the topics strictly belonging to dentistry—its anatomy, diseases, operations, implements, and materials—the work is as elaborate and complete as its nature admits." We can consequently recommend it with confidence to the student and practitioner of dentistry, and would do this the more emphatically, because of the change in the title of the present edition, which appears to us to be injudicious, being calculated to mislead as to the characteristic feature of the work. The dentist is likely to be deterred from making its acquisition under an impression that it was principally a dictionary of medical terminology, which the precedence given to this term in the title page would fully justify; while the medical student would be disappointed on finding that the large size of the volume was in no small degree due to the elaborate manner in which subjects of little or no particular interest to him, are treated.

We must, therefore, again recommend this work to those for whom it was originally designed, as having strong claims on their attention, being well adapted to their particular wants.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *On Absorption. Influence of Cerebro-spinal Nervous System; of the Sympathetic System; Connection between Secretion and Absorption; Influence of Abstraction of Blood, of Atmospheric Pressure, of Electricity and Galvanism, on Absorption.* By CLAUDE BERNARD.—The nervous system does not act as the conductor of poisonous substances. Strychnia, when placed in the cellular tissue, is not absorbed by the nerves, and when placed on an exposed nerve, it has no poisonous effect. If strychnia is placed on the nervous centres, or in the centre of the cerebral substance, it is quite inert unless when it is absorbed by the vessels. Yet this substance, in producing convulsions, acts *upon* the nervous system. The veins alone absorb poisons; the nerves and arteries are not employed in their transmission. All this has been proved by Magendie.

The nervous system does not appear to have any direct influence on absorption. Paralyzed members absorb, otherwise they would die. But although the cerebro-spinal nervous system has no effect on absorption, the great sympathetic exerts an indirect influence on this function. We know that, on dividing the nervous filaments of the sympathetic, the temperature is raised, and the circulation increased in the parts to which they are distributed. The phenomena of absorption are increased at the same time, for the conditions produced are those which are favourable to their development. We find that these phenomena occur in deep-seated organs, especially in those which are secretory in their functions. Thus, on dividing the sympathetic branches which supply the liver, the kidneys, or the salivary glands, their circulation becomes accelerated, and their secretions are increased. The phenomena of circulation and secretion are intimately connected with each other; and the latter are increased in exact proportion to the augmentation of the former.

There exists a relation between *absorption* and *secretion*, just as between *endosmose* and *exosmose*; the *sympathetic* and the *intermediate*. In the liver, there is an external as well as an internal secretion; the former is that of bile, which is carried away by the bowels; the latter is that of sugar, which becomes diffused in the blood. Probably there exists an internal secretion within the spleen, with which we are as yet unacquainted. The internal secretion of the liver is intimately *en rapport* with active absorption; and a *want of harmony between these processes produces diabetes*. When we irritate the cerebral peduncles, we augment the circulation within the liver, and by so doing excite the increased production of sugar. The sugar thus secreted is not destroyed, because absorption is not proportionably excited.

It is of importance to know the influence which *abstraction of blood* has upon

absorption. M. Magendie's experiments show that by abstracting blood, and thereby diminishing its lateral pressure, the power of absorption is increased; but that this process is impeded by distension of the vessels with blood. Hence, a less quantity of poison is required to destroy an animal having little blood, than another which is full-blooded.

Absorption is influenced by certain physical and chemical causes. It is influenced by *atmospheric pressure*. A dense, compressed atmosphere drives the fluids of the body inwards. Thus, undue pressure on the peripheral capillaries may occasion internal congestions. A rarefied atmosphere, on the other hand, by diminishing the pressure, causes the blood to be sent to the surface, and produces epistaxis. Absorption, therefore, which takes place from without inwards, is diminished by a rarefied, but increased by a heavy compressed atmosphere. A practical application of this fact has been made in medicine. It has been proposed, in the case of wounds made by poisoned instruments or venomous animals, to place a cupping-glass over the place, to prevent absorption of the virus, and to favour the withdrawal of what had been already absorbed. Barry made many interesting experiments which proved that the application of cupping-glasses to poisoned wounds retarded, for a long while, the process of absorption.

M. Bernard has repeated the experiments of M. Fodéra, and has found that *electricity and galvanism* greatly augment the process of absorption.—*Monthly Journal*, Nov. 1854, from *L'Union Médicale*.

2. *New Formation of Bloodvessels*.—Dr. JOS. MAYER has investigated the formation of bloodvessels in plastic exudations in the serous membranes, and in wounds of the skin. The small red points and striæ in fibrinous exudations, considered by Hunter as the first stage of the formation of new bloodvessels, he believes, in the majority of cases, to be nothing more than blood-corpuscles which have escaped with the exudation, and which are destined to undergo the usual metamorphosis of extravasated blood. He combats Vogel's views that the new formation of capillaries in exudations originates in branched cells which contain blood, and subsequently unite to form a network. It would appear, however, that the points and striæ in other cases, especially in older exudations, actually present small vascular networks. From the results of observations and experiments, he concludes that the new vessels which appear in an exudation are to be regarded as direct prolongations of those of the tissues on one or both sides of the exudation. The vascular networks in false membranes and wounds are, in fact, prolongations of the capillary vessels already existing before the inflammation; the prolongations are formed, at first, as fine solid threads, which gradually become thicker, and hollowed so as to admit the passage of bloodvessels; here and there they present spindle-shaped or triangular enlargements, in which a secondary nucleus, generally single, is produced. In the neighbourhood of the new vessels, as well in false membranes as in the granulations of cutaneous wounds, spindle-shaped and stellate cells occasionally appear, which are generally transformed into elastic fibres. The vessels first formed are but simple capillary tubes; but by subsequent addition of nuclei and fibre-cells, become converted into vessels of a higher order—arteries and veins. The time within which vessels are formed is variable; in his experiments, Mayer has observed them on the fifth day, but usually on the ninth.—*Brit. and For. Med.-Chirurg. Rev.* Oct. 1854, from *Schmidt's Jahrbucher*, Nos. 3 and 4, 1854.

3. *On the Malpighian Bodies of the Spleen of the Alligator and Crocodile, and on the weight of the spleen as compared with the bodies of these animals*.—Mr. CRISP exhibited to the Physiological Society (Nov. 13, 1854) coloured drawings of the viscera of a large alligator, and read the following communication:—

"Nearly all the writers on the spleen that I am acquainted with deny the existence of Malpighian bodies in fish and reptiles—an error that a more extended examination of the vertebrate animals would readily have corrected. These bodies are very distinct in some of the cartilaginous fishes, and in many

reptiles; but in none that I have examined are they so large as in the alligators and crocodiles. The preparations on the table, from the spleen of an alligator and of a crocodile, are sufficient evidence of this, although when recently taken from the body they were more apparent. The specimen, however, in which they have been subjected to the action of dilute nitric acid displays them better. They are about the size of these bodies in the sheep and other of the ruminants; they measured, when fresh, about the twenty-fourth of an inch in diameter. Another error of more importance respecting the spleen is the assertion of every writer that I know of, 'that the spleen decreases in size as we descend in the scale of vertebrate animals'—a mistake that can only have originated in that common source of error, the lazy system of taking for granted that which previous writers have stated as correct, and of not investigating for ourselves. It is from this cause that the English works on physiology, and the articles in our Cyclopedia (copied mostly from continental writers), are crammed with mistakes, the question too often being: 'Where does the information come from?'—not '*Is it true?*' From a recent work '*On the Spleen*,' with which one of the reviewers was so charmed that he broke forth into a soliloquy on 'Truth, I extract the following passage: '*Reptilia*.—As we descend in the scale of the vertebrate series, the function of the spleen appears to be considerably reduced in importance, as shown by the extreme diminution of its size—a diminution more marked than in any other of the vertebrata.' But let us test the correctness of this descending scale by the class of animals in question. I have examined ten of these—four crocodiles and six alligators, varying in weight from a few ounces to two hundred weight. All were inspected soon after death, and in seven, the bodies and viscera were carefully weighed by myself, and the alimentary canal measured. In the three larger specimens, the weight of the body only was guessed at. In all, the stomach contained stones; and in one, large pine-knots, partly digested. The relative weight of the body and of the spleen in the ten specimens was as follows: Crocodiles, $\frac{52}{8}$, $\frac{50}{2}$, $\frac{14}{66}$; a large specimen, the body not weighed, about $\frac{59}{7}$; alligators, $\frac{10}{6}$, $\frac{90}{6}$, $\frac{11}{66}$, $\frac{13}{84}$; two large ones, the bodies not weighed, about $\frac{8}{11}$, and $\frac{10}{45}$. If the above proportions are compared with those of some orders of birds, the alligator's spleen will be found to be relatively much heavier; and when the weight of the tegumentary covering (possessing but little vascularity) in these Saurians is considered, the proportion is greatly increased. It may be remarked, too, that some of the animals above mentioned had not fed for several months. If we investigate this descensive scale still further, and examine a lower grade of the vertebrata—viz. fishes, we find a much greater proof of its erroneousness than in the reptiles."—*Lancet*, Nov. 25, 1854.

ORGANIC CHEMISTRY.

4. *On the Immediate Principles of the Excrements of Man and Animals in the Healthy Condition*.—WILLIAM MARCET, M. D., in a paper read before the Royal Society, describes a new method of extracting the immediate chemical constituents of the excrements of man and animals, and gives an account of the substances obtained by its employment. Healthy human feces are boiled to exhaustion in alcohol. The residue is insoluble in ether, and yields to boiling water nothing but ammoniaco-magnesian phosphate. The strained alcoholic solution deposits, on standing, a sediment, from which it is decanted, and then mixed with milk of lime. The subsiding lime is of a yellow-brown colour; it is dried on filtering-paper, and treated with ether, cold or hot; and the solution thus obtained yields, on spontaneous evaporation, beautiful silky crystals, which are purified by solution in a mixture of alcohol and ether, repeated filtration through animal charcoal, and recrystallization; they then appear in circular groups, have the form of acicular four-sided prisms, and polarize light very

readily. This crystalline body the author proposes to call excretine. It is very soluble in ether, cold or hot, but sparingly soluble in cold alcohol; its solution has a decided though weak alkaline reaction. It is insoluble in hot or cold water, and is not decomposed by dilute mineral acids. It fuses between 95° and 96° C., and at a higher temperature burns away without inorganic residue. When boiled with solution of potash, it does not dissolve. As to its qualitative constitution, it is found to contain nitrogen and sulphur, though in small proportions; the products of its decomposition have not yet been investigated. The author has in several cases observed the excretine to crystallize directly in the alcoholic solution of feces before the addition of lime, and has scarcely any doubt that it exists for the most part in a free state in the excrements, and constitutes one of their immediate principles. As to its source, he observes that it appeared in excess when a considerable quantity of beef had been taken, and in less than the usual quantity in a case of diarrhoea attended with loss of appetite; but none could be directly obtained from beef on subjecting it to the same process of extraction as feces. Neither could it be found in ox-bile, the urine, or the substance of the spleen. From the difficulty of obtaining the contents of the human small intestine in a healthy state, its presence or absence in that part of the alimentary canal has not yet been satisfactorily determined. The lime precipitate, after having been thus thoroughly deprived of the excretine by ether, is next treated with hydrochloric acid, and water or alcohol, by which means margaric acid is extracted from it. The author is uncertain whether the margaric acid of the feces is free or combined with excretine; but he is disposed to conclude that the neutral fats are decomposed in the intestinal canal, and their acid set free. Not having been able to detect stearic acid in human evacuations, he supposes that what is contained in the fat of mutton or beef taken as food must be converted into margaric acid in its passage through the alimentary canal. The lime precipitate, freed from excretine, and dissolved in alcohol, by means of hydrochloric acid, forms a dark, port-wine coloured solution, from which the margaric acid is deposited. On then adding water to the solution, and concentrating it on the water-bath, a flaky colouring matter separates, which, being purified by solution in ether, and washing with water, is obtained as a dark-brown or black amorphous substance, similar to the colouring matter of blood, and to that which Dr. Harley has lately extracted from urine. The matters brought down with the lime having been thus extracted, the sediment which spontaneously subsides from the alcoholic solution of feces before its treatment with the milk of lime is next examined. This deposit appears to be complex in its nature; it has a strongly acid reaction, and presents under the microscope small oily globules, mixed sometimes with crystals of excretine, and accompanied by a yellow amorphous matter. By boiling with alcohol, and filtration, a residue remains, which the author has not yet examined, and two substances are obtained from the filtrate. The first is deposited on cooling; when collected and dried on filtering-paper, it has a granular character, and is quite colourless; it is very sparingly soluble in ether, fuses by heat, and burns with a bright fuliginous flame, leaving a white residue, consisting of phosphate of potash. The author has not yet been able satisfactorily to decide whether there is a pure immediate principle or not; he is inclined to consider it as a combination of phosphate of potash and a pure organic substance. The filtered fluid, after separation of this matter, still contains a substance, which he has called excretic acid. It is obtained by evaporating to dryness, extracting the residue with ether, adding to the ethereal solution alcohol and lime-water, and heating. The acid is precipitated in combination with lime, from which it is separated by means of sulphuric or hydrochloric acid and solution in ether. The ethereal solution, after being well washed with water to remove mineral acid, yields the pure excretic acid on evaporation. This body is of an olive colour; it fuses between 25° and 26° C., and at a higher temperature burns without residue. It is insoluble in water and in a boiling solution of potash; very soluble in ether; sparingly soluble in cold alcohol, readily so in hot; its solutions having a marked acid reaction. The author is disposed to believe that in excrement it is combined in form of a salt, with excretine, or a basic substance closely allied to it, which is obtained in the filtrate from which the

excretoic acid is precipitated in combination with lime, in the process of its purification. The author failed to obtain evidence of the presence either of butyric or of lactic acid in the clear alcoholic solution of feces filtered from the precipitate formed by the milk of lime. From the above investigation, therefore, it appears that healthy human excrements contain:—

1. A new organic substance, possessing an alkaline reaction, which the author names exeretine.

2. A fatty acid, having the properties of margaric acid, but not constantly present.

3. A colouring matter, similar to that of blood and urine.

4. A light granular substance, whose properties have not yet been sufficiently examined to admit of its being considered a pure substance.

5. An acid olive-coloured substance, of a fatty nature, named excretoic acid.

6. No butyric acid, and no lactic acid.

The feces of various animals were submitted to the same process of analysis, with the following results:—

1. The excrements of carnivorous mammalia, viz: the tiger, leopard, and dog (fed on meat), contain a substance allied in its nature to exeretine, but not identical with it. They contain no exeretine; they yield butyric acid, which is not present in human excrements.

2. The excrements of the crocodile contain cholesterine and no uric acid; while those of the boa yield uric acid and no cholesterine.

3. The feces of herbivorous animals, viz: the horse, sheep, dog (fed on bread), wild boar, elephant, deer, and monkey, contain no exeretine, no butyric acid, and no cholesterine.—*Med. Times and Gaz.* Sept. 30, 1854.

5. *Sugar found in the Fluid of Ascites.*—In a case of fatty-liver, accompanied with ascites, FRERICHS found in the ascitic fluid removed by the trocar, abundance of sugar, which has never hitherto been discovered in this effusion, when occurring separate from a diabetes. In other cases of ascites, resulting from cardiac disease, &c. this author tried to detect sugar in the effused fluid, but without success. In the case related, he attributes the presence of the sugar to the influence exercised by the liver over the formation of sugar in the animal economy; which process, he has observed in many cases, is not interrupted by the occurrence of ascites. In cases of this disease, he is of opinion that sugar should always be looked for in the abdominal effusion, inasmuch as a knowledge of its presence therein is useful in forming our diagnosis.—*Monthly Journ. Med. Sci.* Oct. 1854, from *Wien. Med. Wochensch.* 1854.

6. *Composition of Exudations.*—GROHE has succeeded in demonstrating the presence of urea in all abundant exudations occurring in the pleuræ or the pericardium, where there existed no renal disease or complication of uraemia. He formerly showed, by negative proofs, that there existed, in the flesh of frogs, the materials for the formation of crystals of nitrate of urea. Like Scherer, he was unable to find any urea in the exudations within the abdomen, or in the liquor amnii.

He found sugar in the pleural and pericardial exudations in a case of epilepsy, and also in a case of chronic pneumonia, combined with suppuration and infiltration of the lymphatic glands. As to the cause of the sugar in the first of these cases, he offers no opinion; but in the latter, he thinks its presence was occasioned by the interruption of the respiratory functions by the disease.—*Monthly Journ. Med. Sci.* Oct. 1854, from *Oerhaudl, d. physic-med. Gesell, zu Würzburg.* iv. 2.

MATERIA MEDICA AND PHARMACY.

7. *Medicinal and Poisonous Effects of Citric Acid and Action of Lemon-Juice.* By H. BENCE JONES, M.D.—[Lemon-juice has been so extensively used, latterly, in the treatment of gout and rheumatism, that the following remarks on the action of this acid, from "Lectures on Materia Medica," given at the Royal College of Physicians, by Dr. Jones, will be read with especial interest:—]

"Mitscherlich gives the following experiments: Half a drachm of citric acid killed a rabbit in twenty minutes. One drachm did not kill a larger animal. Two drachms killed in from twenty-seven minutes to some hours. The animals showed strong action of the poison on the spinal cord. There was spasm of the muscles of the back, and of the respiratory muscles and masseters. Often opisthotonos. The sensation was much diminished, and the heart's impulse enfeebled. On examination, no inflammatory appearances were observed in the stomach and bowels. The mucous membrane was uninjected, but softened in spots. The large external veins were filled with fluid blood, and the blood flowed easily from a vein when opened. The blood itself was very thin, and coagulated slightly, if at all. Death was caused by the action of the poison on the stomach, blood, and nerves.

"In man, the weak acid—1. Assists digestion; but, when taken in large doses, it acts on the mucous membrane of the stomach, and produces a state which disorders digestion. 2. It makes the blood thinner and less coagulable; this has been established by giving rabbits citric acid for many days, and examining the blood before and after the acid was taken. It makes the action of the heart and arteries to be gentler, slower, and feeblér. It reduces the temperature of the body. 3. In long-continued doses it is injurious to nutrition. 4. It promotes the action of the bowels, of the liver, of the skin, and of the kidneys, when fever has lessened the activity of these organs. It acts only conditionally, not absolutely, in promoting secretion.

"The actions of citric acid may be stated thus: It is antiphlogistic or antifebrile, evacuating, fluidizing. It is useful, 1. In inflammations and fevers. 2. In hyperæmic congestions. 3. In cases of poisoning by alkalies and narcotics. 4. In scurvy, liver disease, and dropsy. It produces action of the skin in rheumatic, arthritic, and catarrhal attacks. Whytt prescribed it with great success in hysteric attacks. It has been recommended in diarrhoea, diabetes, and syphilis.

"Having brought before you the action of citric acid, you will now be the better able to enter into the question of the action of lemon-juice.

"The first point which immediately presents itself is the identity or difference between citric acid and lemon-juice. What other substances does the lemon-juice contain in addition to citric acid? Do any of the many substances which exist in lemon-juice so modify the action of the citric acid which it contains, that the medicinal effect of these two remedies cannot be considered as identical? The first question was a chemical one. Can lemon-juice be considered as a solution of citric acid in water, or is it an acid citrate of potassa, or does it contain some other active substances in solution?

"At my request, Dr. Hofmann undertook an analysis of the ash of lemon-juice, and this was made by Mr. Whytt, of the College of Chemistry. Some experiments which I have made on the degrees of acidity of fresh lemon-juice I shall also mention here.

"The lemon-juice was obtained from fresh lemons. It contained no trace of sugar; it had the specific gravity of 1036.0, 1037.9, and 1038.4. The acidity varied in different specimens. Thus:—

"1000 grains of lemon-juice required, in one experiment, a solution of caustic soda containing 30.4 grains of soda. In a second experiment, 1000 grains of lemon-juice required 29.4 grains of soda.

"These quantities of caustic soda are equivalent to 62.2 and 60.8 grains of pure anhydrous citric acid. Hence, each ounce of the lemon-juice examined contained about 28.1 to 27.5 grains of pure anhydrous citric acid.

"1000 grains of the lemon-juice, when evaporated to dryness, and burnt, left

3.60 grains of ash. Hence an ounce of lemon-juice contains only 1.64 grains of ash.

“Mr. Whytt’s analysis of the ash gave, in 1000 parts:—

	1st.	2d.	Mean.
Potassa	444.467	433.501	438.984
Soda	14.888	25.945	21.416
Lime	76.631	74.059	75.345
Magnesia	32.124	33.850	32.987
Sulphuric acid	123.540	—	123.540
Chlorine	72.090	12.079	12.084
Carbonic acid	194.620	—	194.620
Phosphoric acid, combined with lime and magnesia	71.971	77.691	74.829
Phosphate of iron	9.385	11.980	10.682
Silicic acid	5.986	5.420	5.700
Carbon	1.035	—	1.035
Alumina	Absent	—	—

991.232

	Composition in 1000 parts, after deducting Carbon.	In one ounce of Lemon-Juice.
Potash	443.4	.767
Soda	21.6	.038
Lime	76.1	.131
Magnesia	33.4	.058
Sulphuric acid	124.7	.215
Chlorine	12.3	.022
Carbonic acid	196.6	.339
Phosphoric acid	75.6	.130
Phosphate of iron.	10.6	.018
Silica	5.7	.010

1000.0

1.728 grs.

“As it appears from these experiments that an ounce of lemon-juice contains 27 to 28 grs. of citric acid, and only three-quarters of a grain of potash, which, at the very utmost, can only combine with 1 grain of citric acid, it follows that an ounce of lemon-juice does not contain two grains of citrate of potash, and for all practical purposes we may consider it as a solution of free citric acid, every ounce containing about 26 or 27 grains of free citric acid. With regard to the difference of the medicinal action of lemon-juice and citric acid, I instituted some experiments on the action of lemon-juice in increasing the acidity of the urine. The question I proposed to myself was to determine whether lemon-juice acted like a free acid and increased the acidity of the urine, and to what extent it did so: whether it caused the formation of free urea and crystals in the urine on standing.

“The urine of a person in very good health was examined each time it was passed for twenty-four hours by taking a fixed quantity, and exactly neutralizing it by a solution of carbonate of soda, containing for each degree the twelfth of a grain of carbonate of soda:—

Urine passed at	Quantity passed. Ozs.	Specific gravity.	Acidity. Degrees.	Appearance after standing two days.
8.50 A. M.	12	1024.5	34	Urates only were deposited.
10.30 A. M.	2½	1027.8	26	“ “
1 P. M.	4	1026.4	11	“ “
5.30 P. M.	6	1028.3	41	“ “
11 P. M.	7½	1032.7	20	Urates and oxalates.
7.10 A. M.	13	1028.5	33	{ 2d day, urates; 3d day, oxalates.

"The following day lemon-juice was taken; six ounces of pure juice, undiluted, at 12.10, and six ounces more at 3 P. M. :—

Urine passed at	Quantity passed. Ozs.	Specific gravity.	Acidity. Degrees.	Appearance after standing two days.
8.50 A. M.	2	1025.8	36	{ 1st day deposited urates only ; 2d day, urates.
12.10 A. M.	7	1029.4	{ Alkaline from fixed alkali.	{ Phosphate of lime and oxalates.
3 P. M.	3 $\frac{1}{4}$	1033.1	40	{ 1st day, urates only; 2d day oxalates.
6.10 P. M.	5 $\frac{1}{2}$	1028.5	50	{ 1st day, uric acid and urates ; 2d day, ditto.
11.30 P. M.	8 $\frac{1}{2}$	1032.9	20	{ 1st day, oxalates ; 2d day, oxalates.
7.20 A. M.	12	1026.6	32	2d day, urates.
38 $\frac{1}{4}$				

"The following day, the same quantity of lemon-juice was taken at 12.20 and 3 P. M. :—

Urine passed at	Quantity passed. Ozs.	Specific gravity.	Acidity. Degrees.	Appearance after standing two days.
8.30 A. M.	1 $\frac{1}{2}$	1024.5	40	2d day urates.
10.50 A. M.	5	1024.9	12	Oxalates.
12.20 P. M.	2 $\frac{3}{4}$	1029.5	{ Alkaline from fixed alkali.	{ Phosphates.
3 P. M.	5	1030.5	38	No uric acid; urates.
6.20 P. M.	7 $\frac{1}{2}$	1025.7	45	Uric acid; urates.
11.20 P. M.	6	1031.8	30	Urates.
7.30 A. M.	19	1019.8	20	Clear.
46 $\frac{3}{4}$				

"The following day no lemon-juice was taken:—

Urine passed at	Quantity passed. Ozs.	Specific gravity.	Acidity. Degrees.	Appearance after standing two days.
8.30 A. M.	2 $\frac{1}{4}$	1020.0	26	Clear.
11.25 A. M.	6	1022.0	{ Alkaline from fixed alkali.	{ Clear.
12.40 P. M.	2 $\frac{1}{4}$	1027.5	{ Alkaline from fixed alkali.	{ Phosphates.
2.50 P. M.	5 $\frac{1}{2}$	1025.2	30	Clear.
6.35 P. M.	6	1024.6	32	Clear.
11.45 P. M.	9	1030.3	{ Alkaline from fixed alkali.	{ Clear.
7.30 A. M.	21			Clear.
52				

"From these experiments, it is perfectly evident that lemon-juice in twelve-ounce doses daily does temporarily cause an excessive acidity of the urine, and gives a deposit of free uric acid in the urine. Lemon-juice acts, then, as free acid on the urine, and does not at all resemble in its effects those produced by citrate of potassa and other vegetable acid salts.

"Regarding the diuretic properties of lemon-juice, the quantity of urine passed each day during the four days shows that when taken pure, without water, by a healthy person, no strong or immediate diuretic action is observable.

"The pulse also presented nothing remarkable in force or frequency when the lemon-juice was taken.

"The results, then, of these experiments, and the knowledge which is obtained by chemical analysis, shows that lemon-juice may be considered as a

solution of citric acid. You will remember that citric acid is a poison having an action similar to that of oxalic acid. In half-drachm doses it killed a rabbit in twenty minutes, causing loss of sensation and muscular action, and stopping the action of the heart. On examination, after death, no inflammatory action was observed. The action of lemon-juice is similar, but less in degree, to that of oxalic acid, and other vegetable acids.

"From the testimony of observers like Dr. Rees and Dr. Babington, there can be no doubt that its anti-febrile sedative action may be obtained in disease. In acute rheumatism and gout it is by extended observation certainly no specific. It does not even reduce fever so surely and quickly as many other remedies; but it is worthy of attention, that in opposition to the theory of excessive acidity as the cause of these diseases, it certainly does not aggravate the symptoms as might have been expected.

"The account published in the *Revue Méd. Chir. de Paris*, August, 1853, of cases of acute rheumatism, subjected only to the expectant treatment, shows how easily recovery may be attributed in acute rheumatism to the last remedy employed. Still, though lemon-juice is no specific for rheumatic inflammation, yet its anti-febrile action may occasionally be thought preferable to that of nitre, or many other saline medicines.

"It ought never to be forgotten that it acts like a free acid on the urine, setting free uric acid crystals. In St. George's Hospital, I have found free uric acid in urine twelve hours after it has been passed, in every case in which large doses of the lemon-juice were given.

"Hence, in chronic rheumatism and gout, the remedy should be used with care, lest red sand or uric acid calculus should be produced. A pint (20 $\frac{3}{4}$) and more of lemon-juice in twenty-four hours cannot be given for three, or four, or more weeks continuously, without risk of the formation of gravel."—*Med. Times and Gaz.* Oct. 21, 1854.

8. *Action of Theine and Caffeine.*—From experiments made upon frogs and rabbits with the citrates of caffeine and theine, Prof. ALBERS has ascertained the following to be the physiological actions of these substances:—

1. Both salts are capable of producing in frogs a tetanic rigidity of the body as complete as that induced by strychnia. 1 gr. introduced below the skin has this effect.

2. They affect not only the external parts, but also the action of the heart, and in this respect theine is the more active narcotic.

After the administration of both alkaloids, the heart was found to be much contracted, a circumstance which distinguishes their action from that of other narcotic alkaloids. Their action occurred more violently in the cold-blooded than in the warm-blooded animals; strychnia, on the other hand, affects both in the same degree. Caffeine produces the same effects when administered internally as it does when introduced into a wound. It is worthy of note that although caffeine affects thus violently the nervous system of frogs, it acts, when given to men, or especially to women, as a most powerful sedative and anodyne, especially in cases of neuralgic headache. The dose in such cases should not be large; from 3 to 5 grains is rather too much; 2 grs. is quite sufficient to administer.—*Monthly Journ. Med. Sci.* Oct. 1854, from *Deutsche Klinik.* 51.

9. *Simple and Ozonized Oil of Turpentine.*—Some very interesting experiments have been made by Dr. SEITZ (*Archiv. fur Wissenschaftl. Heilk.* Band i. Heft. 4; and *Schmidt's Jahrb.* No. 9), on the comparative action of the simple and the ozonized oil of turpentine. The latter substance is thus prepared: The common oil of turpentine is placed in white bottles, of which it only fills the half or quarter, and is then freely exposed to sunlight. From time to time the bottle is opened, so that the atmospheric air may have free ingress. The oil thus prepared has the smell and taste of peppermint oil, smelling disagreeably, tasting hot and bitter, and giving to the tongue a peculiar pain and sensation of cold. From experiments on animals, it is found that, on the mucous membrane of the mouth and the digestive organs, the ozonized oil acts like the simple

oil; it irritates, and causes an increased flow of saliva and mucus. It is then rapidly absorbed; the pulse increases in fulness and frequency, the respirations are quicker, and, when large doses are given, they become even painfully so. If the doses are frequently repeated, inflammations of the endocardium and pericardium, and congestion, and hæmoptoic infarctus of the lungs, are caused. Small doses act as excitants to the nervous system; but large doses cause stupor, convulsions and paralysis. Like the simple, the ozonized oil passes off through the lungs and the kidneys, giving to the breath the smell of the oil, and to the urine the usual violet odour. The quantity of urine is increased. In one case, in a horse, albumen, sugar, and benzoic acid appeared in it. In men, the ozonized oil, in doses of five to fifteen drops, caused a sensation of coldness on the tongue, and a slight pricking sensation; the saliva was increased; there was a feeling of warmth in the stomach; the skin became hotter, and the pulse more frequent; the urine had the violet odour, but presented no other change. Applied to the skin, it produced the same effect as the non-oxygenized oil, such as redness and feeling of warmth. The ozonized oil has been given in various diseases with some good effect, viz: in chronic cystitis and in incontinence of urine. In menorrhagia, and in a case of hæmatemesis, it was also useful. But its most decided action was evinced in cases of gouty and rheumatic pains, and especially in sciatica. The dose is ten to twenty drops on sugar, or in sugar and water, or mixed with honey, or the yolk of egg.—*Brit. and For. Med.-Chirurg. Rev.* Oct. 1854.

10. *Decoction of Oats as a Diuretic.*—Many years ago, Dr. Thémont called the attention of medical men to the remarkable diuretic properties of decoction of oats. Although the paper announcing his observation contained the narrative of a case of cardiac dropsy cured by the sole use of this remedy, yet he did not succeed in exciting much interest on the part of the profession in his discovery. We have seen the oat-tea tried pretty frequently of late in cases of dropsy, in most of them in combination with other treatment, but unassisted in a sufficient number to fairly test its virtues. That it does really possess diuretic properties there can be no doubt. Its powers are probably not at all superior to those of the decoction of broom; and as a good alternating remedy with the latter, its proper place in therapeutics would perhaps be assigned. Its simplicity and freedom from injurious qualities are great recommendations, since it may, without risk, be intrusted as a domestic remedy to patients not under regular care. In several cases of slight œdema of the extremities, consequent on heart disease, the patients succeeded, by its use alone, in getting rid of that symptom. The mode of preparation is to take two handfuls of common oats (not in any way prepared), and boil them in three quarts of water for about a quarter of an hour. Of the strained decoction, a teacupful should be given frequently, as an ordinary drink.—*Med. Times and Gaz.* Sept. 9, 1854.

11. *Ethereal Solution of Nitrate of Silver.*—Every one who has tried it is aware that the application of a watery solution of the nitrate of silver over a large extent of skin is a troublesome and patience-requiring process. The fluid does not dry quickly, and it runs about, being prevented, by the greasiness of the skin, from being rapidly absorbed. A plan which we see adopted by Mr. Ward, in the London Hospital, obviates very completely these difficulties. It consists in making the solution with the common nitric ether, instead of water. The ether acts as a solvent of any sebaceous matter which may be on the skin, and, from its volatility, very quickly dries in, producing, at the same time, a sensation of coolness very agreeable to the patient. If wished, several coatings may be applied successively to the same part, with loss of but little time. The strength which Mr. Ward generally employs is eight grains to the ounce; but it may, of course, be modified according to the wishes of the surgeon. The use of nitrate of silver externally, in erysipelas and other low forms of inflammation of the skin, is a very favourite practice in most of the London hospitals—either an aqueous solution, or the solid stick moistened, being usually employed. Mr. Ward informed us that the use of the ethereal menstruum was not original, but had been suggested to him by a gentleman by whom he had been consulted.—*Ibid.*

12. *Counter-Irritant Plaster*.—The following is the composition of an irritant plaster which we observe that Dr. Hughes very frequently makes use of in cases of chronic chest affections treated in Guy's Hospital: R. Emplastri picis comp. 5iss; antim. potassio tartratis gr. x. Solve et misce, ft. emplastr. Dr. Hughes states that he finds it a very efficient means of producing a mild, but not very transitory, degree of counter-irritation. The plaster is usually worn for a few days, and then cut away piecemeal as the pustules show themselves. The cases in which we have noticed it employed have been those of chronic pneumonia, either idiopathic or complicated with phthisis and severe old standing bronchitis.—*Med. Times and Gaz.* Oct. 14, 1854.

13. *New Hæmostatic*.—M. HANNON has been led by experiments on the blood of animals to believe that the following preparation is the most powerful hæmostatic known, whether applied to the wound, the seat of the hemorrhage, or administered internally: R. Benzoic acid, 1 part; sulphate of alumine and potash, 3 parts; ergotine, 3 parts; water, 25 parts. The whole is to be boiled for thirty minutes in a porcelain capsule, constantly stirring the mass, and replacing the water evaporated. If this be then evaporated to the consistence of an extract, constantly stirring the mass to prevent the benzoic acid from separating, it presents a brown colour, and has a strong astringent taste.

It is applied externally by spreading a layer of greater or less thickness of the extract over the seat of the hemorrhage. For its internal administration, from which Hannon has derived much benefit in hæmoptisis, it is not necessary to boil the ingredients together; it is sufficient to form them into pills according to the following formula: R. Benzoic acid, 1 gramme; powdered alum, ergotine, of each 3 grammes. M. Make into sixteen pills, one to be taken every two hours.—*Presse Médicale Belge*.

14. *Elastic and Medicated Collodion*.—Simple collodion is apt to become too stiff, and to desquamate; hence various expedients have been resorted to in order to obviate this, and to render it pliable and elastic for medical purposes. M. Robert Latour has proposed the following formula for the manufacture of elastic collodion:—

Collodion		30 grammes;
Venice turpentine		1.5 décigrammes;
Castor-oil		5 grammes. <i>Misce.</i>

This fluid is to be applied by means of a hair-pencil, taking care to make the layer sufficiently thick to be resistant. A simpler elastic collodion, less solid than the foregoing, may be made thus:—

Collodion		40 grammes;
Castor-oil		5 do. <i>Misce.</i>

Glycerine may be substituted for castor-oil.—*Arch. Gén. de Méd.* July, 1854.

It has been proposed to combine various medicines with collodion, to render it a suitable application to various cutaneous inflammations. M. Aran has proposed mixing collodion with an equal quantity of the tincture of the perchloride of iron. Dr. Hannon, of Brussels, recommends very strongly a combination of collodion and acetate of lead, made by adding thirty grammes of collodion, drop by drop, to a warm concentrated solution of the said salt. The collodion thus formed is said to be very flexible and elastic.—*Presse Médicale*, 1850.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

15. *Anæmia of Infancy*.—Professor L. W. RITTER VON MAUTHNER, of Vienna, remarks that, for many years, the practice of venesection has been on the decline, and he quotes the words of Professor Richter, of Dresden: "Poverty of blood is, next to tuberculosis and cancer, the increasing evil of our time, which

will bring down a gradual deterioration of the race, and therefore merits our most earnest consideration." According to Valentin, most neuralgic affections are caused by anæmia; and, according to Trousseau, chlorosis now prevails in the general pathology of the female. An anæmic mother will produce anæmic children; anæmia may be congenital, or acquired from too rapid development and quick growth. It is difficult to believe in the disease as congenital, the quantity of blood in the infant's tissues being normal. Valentin has shown that the amount of blood in the newly born is proportionately greater than in later life; for a child of five to six pounds has nearly two pounds of blood; while, at the age of thirty, it barely attains one-fifth of the weight of the body. But, in infancy, as in old age, the watery constituent is more considerable than in middle life.

The cause of this so often congenital anæmia lies in the general corporeal weakness of the mother, whence also it comes that there are so many abortions and early deaths. Want of proper food during pregnancy exerts a potent influence, too commonly at work among the lower orders, oppressed with want and care; and the younger children are more subject to the disease, because the exhausted mother loses in time the power of nourishing her children by her own milk, and the father has not the means of procuring a wet-nurse. A child born of a mother who has suckled another infant during her pregnancy, generally suffers from poverty of blood. Losses of blood, or profuse mucous discharges, by injuring the mother's health, are to be regarded as prejudicial to healthy foetal development. An aged or diseased father commonly begets an anæmic child. Congenital syphilis must also be regarded as a cause. The morbid change in the blood being unknown, we must be content with the term, "anæmia syphilitica." It is not always recognized, but is of most momentous importance as regards the rising generation. Should an infant affected with this disease be vaccinated, a peculiar glandular disease is apt to ensue from this second poisoning of the blood. The author proceeds to enumerate the symptoms of congenital syphilis. The child suffers from excoriations about the mouth and anus, etc.; from roseola, pemphigus, eczema, psoriasis, or from a peculiar tenuity, smoothness, and transparency of the epidermis. They do not possess power to resist external influences; they are long in teething; tuberculosis is apt to ensue in the course of time.

The anæmia of development comes on when growth, at any period, is very quick. Hence we have the anæmia of dentition, the anæmia of puberty, or chlorosis.

From experiments upon animals, Nasse has shown that animal diet renders the blood more coagulable than vegetable diet, and increases the number of the blood-corpuscles. From sugar and starch-meal there is formed a glutinous lymph plasma, but no corpuscles. A purely vegetable diet, therefore, is not suited for infancy; the more so, from the anatomical fact, that the cæcum, that part of the intestinal canal where vegetable digestion goes on, is but imperfectly developed at that period. Anæmic children are very apt to suffer from inflammations. Nature endeavours to excite a reaction from this depressing influence; and stasis of the blood commonly ensues in organs unfitted for active circulation; but the exudations tend only yet more to impoverish the blood, and venesections materially increase the evil. One of the evils of infantile anæmia is hemorrhage from the congested and delicate vessels of the large intestine. This occurrence is frequently overlooked, or confounded with other disorders, such as convulsions. The author has verified the fact by dissection.

When the plastic power of the organism, says Canstatt, is exhausted by rapid natural and morbid evolutions, then tuberculosis suddenly forms, and the latent material begins to be deposited upon any excitement in the different viscera. Thus, it is to be feared, during the blood-impovertyment of dentition; and it attacks, especially, children subject to perspirations, and to excited pulsations of the heart.

The practitioner should never forget, in considering the diseases of childhood, that sudden attacks, even death itself, may occur just as easily in children from anæmia as from hyperæmia. Thus, cases of convulsions, and other attacks, once regarded as inflammatory, require, in the present day, more careful exami-

nation and more accurate diagnosis.—*Med. Times and Gaz.* Nov. 4. 1854, from *Journal für Kinderkrankheiten*, July and Aug. 1854.

16. *Tubercle*.—M. MANDL has published (*Archiv. Gén. de Méd.* March, April, 1854) two interesting papers on the microscopic examination of tubercle, in which he enters pretty fully into the literature of the subject. He denies altogether that tubercle presents any specific morphologic elements. He states : 1. The tuberculous substance is an amorphous matter strewed with fatty molecules ; it is finely granular at first, then diffuent. It infiltrates the elements of tissues, and solidifies in the interstices. The fragments of this amorphous substance, presenting neither determinate form nor size, are analogous to those of all other amorphous exudations. There are no special tubercle-globules or corpuscles ; there are no characteristic elements. 2. The tubercular substance, being an amorphous matter, cannot increase and develop. Tubercles grow only by juxtaposition ; *i. e.* by fresh exudations. This is a proof the more that the progress of the disease is dependent on an incessantly active cause, which cause must be got rid of, if we would root out the tuberculization. 3. Softening of tubercle is due to a fatty degeneration, which can declare itself before products of inflammation, such as pus and “ inflammatory globules,” show themselves. 4. This degeneration is a certain proof that tubercle cannot organize itself, as fatty degeneration occurs only in tissues, the nutrition of which is suspended. 5. By means of the fatty degeneration and the products of inflammation, which are joined to it at a later period, tubercle is completely eliminated. 6. If one is permitted to draw a therapeutical inference from these facts, it is that attention should be directed, first, to the cause, and secondly to the modifications which the tubercle undergoes ; *i. e.* the natural course of the disease.—*Brit. and For. Med.-Chirurg. Rev.* Oct. 1854.

17. *Occurrence of Pigment in the Blood*.—The occurrence of a large amount of pigment granules in the blood has been long known. Meckel, Virchow, and Heschl have described it as frequently happening after intermittent fevers. In many of these cases the pigment appears to cause no injury to the system ; but in other instances, and Dr. J. PLANER describes many of these, very sudden death ensued, with symptoms that led to the diagnosis of a disease of the brain or of typhus. In other cases, again, the collection of pigment was associated with dropsy and albuminuria, but whether it had any intimate connection with these symptoms was doubtful.

Dr. Planer relates all the cases of pigment in the blood which have been discovered in the dead-house at Vienna during the last two years. He refers first to the—

(1.) *Cases in which there were Cerebral Symptoms*.—The pigment in the blood was found in the state of black, or more uncommonly of brown-yellow, brown, or (very rarely) red granules, many of which were united together by a clear hyaline substance, which was soluble in acids and alkalis. Meckel observed pigment-cells very rarely ; Virchow more frequently ; Planer never. Planer also never saw in the pigment masses anything like a nucleus. Besides these masses, the aggregation of the pigment grains sometimes forms black or brown flakes of the most variable form ; these flakes are sometimes evidently constituted by a hyaline substance, in which black pigment is embedded, and, in two cases, in the blood from the right ventricle. Planer found two hæmatoidin crystals adhering to this clear substance.

The relative number of the pigment masses, as compared to the blood globules, was not determined. In some cases the capillaries seemed almost choked up with them. Planer did not find that the colourless corpuscles of the blood were more numerous. The pigment was always found in the blood of the heart ; it was not more common in the blood of the portal or splenic vein than elsewhere. The pigment masses were little affected by reagents. Meckel found them decolorized by chlorine ; but Planer does not confirm this.

In addition to being in the blood, pigment was found, in these cases, in the organs, especially in the spleen, and it was found in this organ even in cases in which other organs and the blood contained very little of it. The special seat

of the pigment in the spleen was not determined; its form and general characters were exactly the same as in the blood. Virchow and Meckel have found the pigment in cells in the spleen; but Planer found only the agglomeration of the pigment granules by the hyaline substance.

In the liver, the pigment granules were often very numerous. They were found to be sometimes in the vessels; but it could not be determined whether they were wholly thus placed, or whether some part of them lay outside the vessels between the cells. The livers thus affected were of normal size; the cells were normal, or contained more fat than usual, and also brown granules of bile-pigment, which was, however, easily distinguished by its appearance, as well as by its chemical reactions, from the pigment in the blood.

The cerebral substance was often affected with this pigment change, and in this case it was certain that the pigment was in the vessels; in some cases the flakes, already referred to as seen in the blood in the heart and large vessels, were in the cerebral capillaries of such size that it seemed impossible they could pass. In fact, Planer conjectures that the extreme abundance of pigment granules in the cerebral vessels must have been caused by the fact that they could not pass through the cerebral capillaries, which (especially in the gray substance) are the finest in the body (Kölliker).

Meckel describes a case which occurred in Vienna, in which there were in the gray substance numerous punctiform hemorrhages produced by blocking of vessels through pigment; and since this time, several cases of the same kind have been seen by Planer.

Besides the spleen, liver, and brain, the kidneys were found to contain pigment, both in the vessels of the cortical substance and of the Malpighian capsules. Some pigment was also found external to the vessels, and perhaps was the residuum of punctiform hemorrhages, but no fresh punctiform hemorrhages, such as occurred in the brain, were ever seen in the kidneys.

The pigment was found, in the pulmonary vessels, in very variable quantity. In the vessels of the other organs of the body its quantity was unimportant, except in the lymphatic glands, where it was sometimes abundant.

After making these general remarks, Dr. Planer relates 19 cases on which they are based, and refers also to 10 other cases, whose histories he does not give. The cases are too shortly reported; but it is evident that they belong to a class not seen, or but very rarely seen, in this country. In 12 of the 19 cases, the patients were comatose when brought to the hospital, and in most of them the coma had set in suddenly sometimes with, and sometimes without, previous symptoms of malaise or fever. In the 7 other cases there were symptoms of intermittent fever, and then gradual fatal stupor came on. In several other cases there was paralysis as well as coma.

In the whole 29 cases referred to in this series, there were 14 in which the pigment was in such quantity in the cerebral vessels as to give the gray substance a remarkably dark slate-gray aspect; in 8 of these cases there were numerous little hemorrhages. In the other cases the pigment was in less quantity in the cerebral vessels, but still was always found there.

(2.) *Cases of Albuminuria and Dropsy with Pigment in the Blood.*—Nineteen such cases were discovered in the dead-house; in 12 there had been previous intermittent fever, or this existed at the time; in the others, it could not be learned whether there had been previous ague.

The appearances of the pigment in the blood and organs were the same as in the former series, except that the cerebral vessels contained little of it, and the kidneys a great deal. In spite of the albuminuria and dropsy, the kidneys were normal, except as far as the pigment was concerned, and in no case could it be safely said that there was Bright's disease.

(3.) *Cases in which there was Pigment in the Blood and Organs without appearance of injury to the health.*—26 cases are related in which death occurred from the following causes: Phthisis, 4; typhus, 2; pneumonia, 6; arthritis and pericarditis, 1; peritonitis, 1; pyæmia after operation, 1; puerperal affections, 3; gangrene of the leg, 2; granular liver, 1; marasmus senilis, 4; calculous nephritis, 1. In all these cases the liver and spleen were as pigmentary as in the other series of cases; in the blood the pigment was in less amount, and it was

scarcely found in other organs; in only 1 of these 26 cases was the gray substance of the brain darkly coloured through it.

(4.) *Origin of this Disease.*—Dr. Planer believes that the development of the pigment is closely connected with the occurrence of intermittent fever, although he admits that in several of the cases there was no authentic evidence of previous ague. He observes that he has in many cases of ague-cachexia got a little blood during life by pricking the finger, and has been astonished at the number of pigment granules and flakes exactly resembling those found in the dead bodies already referred to.

Planer remarks, finally, that, in spite of the number of cases he has seen, this pathological condition is very little understood, because observations on living subjects are yet wanting. It is certain, however, that he has opened up a new and most important inquiry, which promises to give us some new insight into malarious diseases.—*Brit. and For. Med.-Chirurg. Rev.* Oct. 1854, from *Wien Zeitschrift*, Feb. 1854.

18. *Weight and Specific Gravity of the Brain in the Insane.*—Dr. DAVID SKAE has published a very interesting paper on this subject in the *Monthly Journ. Med. Sci.* for Oct. 1854.

Although numerous and extensive investigations have been instituted for the purpose of discovering the pathological conditions by which the symptoms of mental derangement are produced, we are still in ignorance of the pathology of the disease.

Dr. Skae commenced, some years ago, a series of experiments by collecting the weights of the brain, cerebrum, and cerebellum, by measuring the gray matter, by endeavouring to determine the degree of hardness or softness of the cerebral substance, by ascertaining by what length of a column of water it could be broken up, and more lately by taking the specific gravity of the gray and white substance of different parts of the brain.

In the present paper Dr. S. confines himself to the weight and specific gravity of the brain, and to such general deductions from these observations as the number of facts seem to warrant. He has compared his observations on the weights of the brain with those collected in the Royal Infirmary of Edinburgh, by the late Dr. John Reid and Dr. Peacock, and the experiments on its specific gravity, with those published by Dr. Sankey in the *British and Foreign Medico-Chirurgical Review* for January 1853 (vol. xi. p. 240), and made in the Royal London Fever Hospital. The weights used were avoirdupois.

The observations of the weights of the brain comprise 172 cases, of which 83 were males, and 89 females. The results, with those obtained by Drs. Reid and Peacock, are given in tabular form. We have not space for the tables, and must content ourselves with giving the results.

A comparison of the columns showing the heaviest brains in the sane and insane, shows that in only one instance did the encephalon exceed 60 ounces in the insane, while in several of the sane it amounted to 62 ounces and upwards, the heaviest brain in the one series being 60 oz. 8 dr., and the heaviest in the other being 62 oz. 12 dr. It will also be seen that, with two exceptions, the superiority in weight was among the brains of the sane in each of the quinquennial or decennial periods into which they are classified. "Whether," says Dr. S., "this justifies the inference that persons having large brains are less frequently the subject of mental derangement than others, I shall not venture to say; but the fact is a striking one when contrasted with the results derived from a comparison of the average weight of the entire number of cases, where it appears that the average weight is *increased* in persons dying insane. The average weight in the insane from 15 to 90 years of age being 30 oz. 4 dr., and in the sane 49 oz. 14 dr.

"The same results are derived from a comparison of the weights of the brains of the females; here, in one exceptional case, the brain weighed 61½ ounces in a female who died insane, but in a majority of cases the greatest weight was on the side of the sane. On taking the average of all the cases, the weight of the brain in the insane was 44 oz. 7 dr., and in the sane 44 oz. 5 dr.

"This comparison establishes, I think, a strong presumption that the absolute weight of the brain is increased in the insane, a presumption which is greatly strengthened by the reflection, that in many cases of insanity the absolute *size* of the brain must be materially diminished by the large quantity of serous effusion found in the ventricles, arachnoid sac, and sub-arachnoid tissue.

"This increase in weight, however, appears to depend chiefly upon an increase in the weight of the *cerebellum*, for, on comparing the weights of the *cerebra* in the two series of cases, it will be found that the difference is inconsiderable, and indeed in the case of the females that it is in favour of the sane. On comparing, however, the columns showing the weight of the *cerebellum* with the pons and medulla in the two classes, it will be found that there is almost a uniform preponderance in the weights of those of the insane. The average weight in all the cases of the insane males exceeds that of the sane by 4 drachms, and in the females by 3 drachms.

"In the two last columns of these tables the ratio between the *cerebellum* (with the pons and medulla) and the *cerebrum* is given at the different ages distinguished. Here the same fact is very clearly brought out; in almost every instance the *cerebellum* in the insane being heavier in relation to the *cerebrum* than it is in the sane. From the average of the entire number, the *cerebellum* was found in the males to be as 1 to 6.8 in the insane, and as 1 to 7.06 in the sane; and in the females as 1 to 6.68 in the insane, and only 1 to 7 in the sane. The *cerebellum* therefore is considerably heavier in relation to the *cerebrum* in the insane.

"This mode of estimating the results appears to me to be free from the source of fallacy arising from the small number of cases compared, when we compare the absolute weights together, as here, on examining all the cases separately we find, in almost every instance, the *cerebellum* of each brain heavier in relation to its own *cerebrum* in the insane than in the sane."

On arranging the weights of the brain according to the form of disease under which the patients laboured, and taking the average, the following results are obtained:—

"In mania, the average weight of the *encephalon* in males was 54 oz. 11½ dr.; in monomania, 51 oz. 11¾ dr.; in dementia, 50 oz. 5¼ dr.; and in general paralysis, 49 oz. 12¾ dr.; the weight being greatest in mania, and least in general paralysis. In comparing the average weights of the *cerebella*, &c. however, in the same series, while they are found to follow the same decrease from mania, the highest, through monomania to dementia, the weights being respectively 6 oz. 11½ dr., 6 oz. 10¾ dr., and 6 oz. 9¼ dr.; the cases of general paralysis present the highest average, the average weight being 6 oz. 12¼ dr. From the fact that in general paralysis the morbid appearances are most constant and most distinctly marked, this result might, perhaps, be anticipated, if we assume what I have endeavoured to show, that the relative weight of the *cerebellum* to the *cerebrum* is increased by insanity, and that the *cerebellum* is the organ principally affected. This increase, too, would appear from a review of the history of the cases to bear a constant relation to the form of the disease, and to be greatest in the more protracted and gravest cases. On comparing the average weight of the *cerebrum* to that of the *cerebellum* in the cases classified as above, it was found in males to be as 1 to .1396—.1478—.1511 and .1582, in mania, monomania, dementia, and general paralysis respectively. The same gradual increase in the relative weight of the *cerebellum* was found to obtain in the females, the *cerebrum* being to the *cerebellum* in them as 1 to .1526—.1552—.1541, and .1601, in the same class of cases respectively.

"It appears, therefore, that in cases of acute mania, a disease of comparatively short duration, there is the smallest amount of increase in the relative weight of the *cerebellum*, and that in general paralysis the greatest increase takes place.

"In reflecting upon these results, the impaired control over the voluntary movements, which forms so marked a feature of general paralysis, and the very constant increase of the relative weight of the *cerebellum* in this disease, must

at once be viewed as very interesting and important facts in connection with the functions ascribed to the cerebellum by Flourens, and other physiologists.

"May it not be asked—supposing it to be established by an extended series of observations, that there is uniformly an increase in the relative weight (and specific gravity, as we shall presently see) of the cerebellum as compared with the cerebrum in all forms of insanity—whether it may not be inferred that the cerebellum is the organ through which we exercise self-control—control over the volitions, and successions of our thoughts, as well as over the voluntary muscular movements; a perturbed volition, or a loss of self-control, being, of all other symptoms, the most essential characteristic and pathognomonic feature of insanity?"

The *specific gravity* of the gray and white substance of the brain was taken in 62 cases, of which 29 were males, and 33 females, and the results, with those of Dr. Sankey, are arranged in tables. These tables show that the specific gravity, both of the gray and white matter, in the cases of insanity was almost uniformly higher.

"In Dr. Sankey's cases, the lowest specific gravity of the gray matter was 1028; in the asylum ones the lowest was 1030; the highest in Dr. Sankey's cases was 1046, in mine 1049; while the average specific gravity in all the cases of both sexes was, in the former, 1034, and in the latter, 1039, showing an increase in the specific gravity in the cases of insanity.

"The lowest specific gravity of the medullary substance in the healthy brains was 1032, in the diseased 1034; the highest in the healthy was 1048, in the diseased 1053; the mean of all the cases was 10,411 in the sane, and 10,424 in the insane, showing an increase in the specific gravity of the white matter of the brain in cases of insanity.

"These results are corroborative of those obtained by Dr. Sankey, in his observations at the London Fever Hospital, where, in all the cases complicated with cerebral symptoms of a grave character preceding death, such as convulsions, strabismus, paralysis, and utter unconsciousness, the specific gravity was high, averaging both in the gray and white matter 1041. He makes two exceptions in regard to the white matter, in both of which the specific gravity was below the mean—these were both cases of children of eight and ten years of age, and both of tuberculous meningitis. Deducting these cases, the average specific gravity of the white matter in his series of cases complicated with cerebral symptoms was 1043."

Dr. Skae, on examining his cases in detail, found that most of those cases where the specific gravity of the gray matter was considerably below the mean, the patients had died of phthisis, and in other instances of exhaustion occurring at an advanced age. Where exceptions to this general inference were met with, it was found that either the symptoms immediately preceding death were of a grave character, or that the morbid appearances found in the membranes indicated chronic inflammatory action.

Dr. S. has also taken the specific gravity of the gray and white matter of the cerebellum in 27 cases. The number of observations is too few to warrant more than a presumption that the specific gravity of the cerebellum is higher than that of the cerebrum. The difference is much more apparent when, instead of comparing averages, the specific gravity of the cerebellum is compared with that of the cerebrum to which it belonged.

Dr. Skae has not been able, as yet, to obtain a sufficient number of data to enable him to compare the specific gravity of the cerebellum of the sane with those he has made. In a series of five cases, of males dying without cerebral symptoms, the average specific gravity of the gray substance of the cerebellum was 1042, that of the cerebrum being 1041. In the cases of the insane examined by Dr. Skae, the average was 1040 for the cerebellum, and 1035 only for the cerebrum, showing a much greater increase in the ratio in the insane than in the sane, although the absolute specific gravity was less in these particular cases.

"The *white* substance of the cerebellum in the five cases referred to, had an average specific gravity of 1.0433, while that of the cerebrum was 1.0430. In the cases examined in the asylum, the average specific gravity of the white

substance of the cerebellum was 1.044, while that of the white substance of the cerebrum in the sane cases was 1.039 only—thus showing a small increase in the absolute specific gravity of the white substance of the cerebellum on the insane in the cases compared, and a very considerable increase in the relative specific gravity to that of the cerebrum in the case of the insane.

“From these data, although limited, I infer that the specific gravity of the cerebellum is increased in insanity, and attains a greater increase in relation to that of the cerebrum than it does in persons dying sane.

“In comparing the specific gravity in the different forms of mental disease, and taking the average of all the cases of each kind, I find the lowest specific gravity of the gray matter to occur in cases of dementia, where, however, it is still 0.003 above the average in the sane. The next highest specific gravity occurs in cases of melancholia and monomania, the next in general paralysis, the next in mania, and the highest in epilepsy.

“Of the *white* substance, the lowest average of specific gravity occurred in cases of mania (1040), the next in dementia (1041), the next highest in general paralysis, the next in monomania (1044), and the highest also in epilepsy, being 1.0458.

“The results of the preceding observations are curious and interesting. They are inferences certainly from a comparatively limited number of data, but they are, I think, sufficient to prove that more extended observations of a similar kind may probably lead to some satisfactory and important deductions regarding the pathology of insanity, and the functions of the brain.”

19. *Etiology of Cholera*.—The following extract of a letter from Prof. LIEBIG to Prof. Hoffmann, gives an account of some interesting results which have been obtained by Prof. THIERSCH and PETTENKOFER during the late severe epidemic of cholera at Munich.

“Dr. Pettenkofer, who was commissioned by the Bavarian Government to make the inquiry, arrived at the following conclusions:—

1. A basin-like conformation of the ground (*muldenartiges Terrain*) generally favours the occurrence and fatal issue of cholera. Those houses especially which occupy the lowest site in towns, and in which the privies and cesspools are so constructed that the liquids they contain do not pass from the house, but enter it, are the most liable to the disease.

2. A loose soil under the houses, capable of absorbing the liquids, augments the tendency of a district to the miasm, of which a substratum of rocky stones renders it much less susceptible.

3. A certain degree of moisture of the soil also increases the susceptibility.

4. The products of decomposition of human and animal excrements contained in the soil, appear to be the elements that determine the soil's capability for absorbing the miasm.

5. In towns, the inhabited declivities or hollows, the soil of which presents this absorptive character, are always more liable to cholera than the more elevated sites in their neighbourhood, independent of their absolute elevation above the level of the water of the sea.

6. The same conditions prevail in privies and sinks, especially when provided with wooden pipes and pans, as in soil impregnated with putrefying excrements.

7. In towns, those districts which have no privies, but only night-stools, are more obnoxious to cholera; and here, again, those houses especially which have common cesspools in the yard, provided only with a slight fall, or with a fall inclining towards the houses.

8. The excrements of cholera patients, when in a state of decomposition, become fertile sources for the propagation of the disease in families.

The observations of Dr. Thiersch have remarkably confirmed the facts elicited by Dr. Pettenkofer. Dr. Thiersch found that cholera patients, the corpses of persons recently dead of cholera, and the matters evacuated by vomiting and diarrhoea, do not propagate the disease; they are not contagious. If the matters evacuated by stool are left to themselves, at a temperature of from 5° to 10° Centigrade (41° to 50° Fahr.), from three to seven days a change takes

place; they undergo a process of fermentation; they are then capable of exciting cholera in healthy individuals. The period within which cholera evacuations can produce this effect, does not last longer than eight days. If recent cholera evacuations are mixed with the food of mice, in small quantities, no effect is produced in the animal. After being left to themselves for from three to seven days, the evacuations pass into a state of fermentation, which may be recognized by their converting amygdalin into the well-known products of its decomposition, a property not possessed by the fresh evacuations. If the matter in this condition be added to the food, the animals which have eaten of it sicken in from four to seven days, and die within thirty-six or forty-eight hours of their being seized. The disease commences with diarrhoea, and the dead body shows all the phenomena observed in human corpses that have died of cholera.

The facts ascertained by Dr. Pettenkofer and the investigations of Dr. Thiersch, mutually explain, and are supplementary to, each other. The poison of cholera is developed in the excremental liquids after they have left the body. It is thus that privies, sewers, and all places in which they become mixed with animal matters, become real sources of cholera poison.

The well-known preventives of fermentation and putrefaction remove the cause of the propagation of cholera. Chloride of lime, sulphuric acid, tar-water, &c., at once present themselves to the physician as disinfectants. It is the duty of the medical man, in promoting the interests of science and humanity, to employ these remedies, and to determine the extent of their power.

In the prison of Kaisheim (in Bavaria), the cholera was introduced by an individual who died there; and the early employment of disinfectants was the cause of the disease being limited to this case, and of but one of the other 500 or 600 prisoners, although in a dangerous locality, having been attacked. On the other hand, in Ebrach (in Bavaria), when, from ignorance, this precaution was not at once adopted, 15 per cent. of the 350 prisoners died.

The discoveries of Dr. Thiersch throw much light upon the nature of contagious poisons. The blood-producing substances, in the condition of their normal metamorphoses, are essential to nutrition and health. The same substances and their products, in a state of abnormal decomposition, give rise in the healthy organism to disease and death."—*Med. Times and Gaz.* Nov. 18, 1854.

20. *Presence of Fat in the Feces, and certain Alterations of the Blood Corpuscles in Cholera.*—Dr. W. L. LINDSAY has published (*Monthly Journ. Med. Sci.* Aug. 1854) four cases of cholera, in all of which fatty or oily matters existed to a notable extent in some of the fluid feces of the fever stage and convalescence; and in one there existed a morbid structural alteration of the blood corpuscles. These cases occurred in the Surgeon's Square Cholera Hospital of Edinburgh, while Dr. L. was resident physician thereof.

In these four cases, fatty matters were found in the feces on seven different occasions. In five of these they occurred in a solid form, resembling pieces of tallow or suet; in two as a fluid oil. It is noteworthy that three of the patients were members of the same family. "I have frequently," says Dr. L., "found a greasy scum to collect on the surface of the collapse, as well as the fever and reaction, stools, on allowing these to stand for various periods. Under the microscope, this usually consists of oil globules, entangling a few prismatic phosphates. It often happens, moreover, where no appreciable scum or sediment exists, that the microscope points out the presence of abundance of minute oil globules. In the vomit of cholera, fatty bodies of various forms and oil globules are very common, evidently, however, having their origin from the food; and in the urine occasionally, and the bile very frequently, I have met with considerable quantities of oil globules. In several cases the hepatic epithelium has been loaded with oleo-albuminous granules; in a less proportion of cases the renal, and in a still smaller number the epithelium of various parts of the respiratory and digestive systems, has been rendered dark and granular from infiltration by the same material. Of the cause or source of the presence of these oily matters in the feces, I shall not here speak; but it is right to mention that, in

most, if not all, of these cases, castor oil had been administered, one or more days previously, to obviate a tendency to constipation. That this substance, however, is not its true source is probable, because—

"1. The fatty or oily matter did not appear in the *first* stools produced by the action of the drug.

"2. The phenomenon occurred sometimes two or three days after the drug was administered, several stools devoid of any fatty intermixture having been passed in the interval.

"3. It did not occur in many other patients, who laboured under precisely similar symptoms, and to whom castor-oil was given.

"4. It was exhibited in three members of the same family.

"5. It is improbable that castor-oil could be the origin of fatty matters having such opposite characters, viz: hard concrete fat, soft yellow fat, and liquid oil.

'In Case I. it will be observed that the blood disks are noted to have been remarkably altered in appearance. I have met with a similar appearance, and with other changes of the blood in other cases of cholera. In one instance, the white corpuscles were present in marked excess, the red being normal in appearance; in some cases the red corpuscles seemed to have a strong tendency to adhere in rouleaux, in others the very reverse obtained; frequently they became hazy or granular.

"Without at present making any comments, I would merely call attention, *en passant*, to the following points, which the foregoing cases also serve to illustrate:—

"1. Albuminosity is not a distinctive feature of the *collapse* evacuations, as it occurs frequently also in the reaction and fever stools. I have found it occasionally in healthy feces, and in the stools of typhus, phthisis, dysentery, &c.

"2. Non-correspondence, in date or degree, of the comatose tendency of the 'consecutive' fever with the suppression or re-establishment of the urinary secretion.

"3. The distinctive histological and chemical features of the collapse, reaction and fever stools, vomit, and urine.

"4. The following pathological phenomena in Case I.

a. Distension of the gall-bladder, and comparatively *healthy* condition of the bile. Presence of a *gall-stone*.

b. Thickening, softening, and exuviation of the mucous membrane of the large intestines.

c. The marked difference in the histological character of the *post-mortem* contents of the intestines and the stools passed during life.

"5. The presence of the so-called 'annular' bodies, vibrios, &c. is merely accidental and immaterial, so far as regards the causation of the disease.

"6. The occurrence of the so-called 'cholera-exanthem' in Cases II. and III.

"7. Insufficient police and parochial sanitary regulations a fertile predisposing cause of cholera, &c."

21. *Absorption of Medicines in Cholera.* By M. AUGUSTE-PHILIBERT DUCHAUSOY.—There is scarcely a practitioner in medicine, familiarized with the practice of cholera, who is not convinced that the functions of absorption are disturbed during the period of cyanosis and of algidity. Every one must have had experimental proof of the fact in seeing the patients upon whom he had expended certain active substances, alcohol, for example, remain for some time insensible, and begin to complain of the effects for the first time upon the establishment of reaction. Theory may, without great risk, be brought forward to explain these facts. The great condition of absorption is the existence of a circulating current. Beneath this condition must be placed: 1st, a certain degree of fluidity of the blood, which keeps free the calibers of the smaller vessels; 2d, the integrity of the nerves, capable of influencing the capillary circulation. In algid cholera, the circulation is singularly embarrassed, the blood is viscid, the nerves respond imperfectly to electrical excitement, and the stagnation of the blood in the capillaries is evident. The effect of such conditions upon the function of absorption is already well known, but we do not hesitate to say that no person has submitted the important question ever before

to regular experimentation. The merit is due to M. Duchaussoy and M. Ver-
nois, his *chef de service*.

The plan of M. Duchaussoy is as follows. He selects a certain number of active substances, the physiological effects of which are well known—sulphate of quinia, iodide of potassium, mercurials, belladonna, sulphate of strychnine. When any doubt exists respecting the substances, he submits them to a special experiment. Then he applies these same substances in contact with absorbing surfaces in patients arrived at different periods of cholera; and he establishes the fact, that the physiological effects are null during the algid period, and that they become apparent for the first time during the commencement of reaction. The following is the *resumé* of his experiments.

Sulphate of Quinine.—The thesis contains two observations. In the first, relative to algid cholera, three lavements, containing each a scruple of sulphate of quinine, were retained several hours. The same was the case with a scruple of this substance taken by the mouth. Nevertheless the patient did not experience headache, ringing of the ears, deafness, disturbances of vision, nor disorders of the intelligence. In the second, nearly similar doses remained in the digestive tract with the same result. But, from the moment of reaction, the physiological effect began to be pronounced.

Iodide of Potassium.—Habitually, iodide of potassium in large doses determines heat of skin, headache, a sort of intoxication, lancinating pains in the eyes and temples, pricking of the throat and nostrils, and divers cutaneous eruptions. In consequence of the salivary glands possessing the power of eliminating this substance, their secretion rapidly acquires a saline taste. This character, studied by M. Bernard, is very valuable. M. Vernois prescribed iodide of potassium to five patients suffering from cholera. It is stated in the thesis, that, in three cases, no signs of absorption were manifested. In one case, the patient took, by mouth and by anus, 280 grains of the ioduret in six days; the particulars of the other are not given. In the remaining two of the five cases, some phenomena, which might be attributed to the medicine, were remarked. One patient, a pregnant woman, took 160 grains in four days, a part subsequent to the reappearance of the pulse. On the day when she took her last dose she was delivered of a dead infant, and the day following was attacked by urticaria. The other patient took 100 grains of the iodide in one day. There was no appreciable physiological effect, but it was found after death that the salivary glands and the pancreas contained a little iodine, as was ascertained by chemical tests.

Mercurials.—The author gives but one observation, in which, six grains of calomel having been given to a patient with cholera, and retained, the evacuations which followed during four and twenty hours were noticed at first to be white, then black, and not green, as is usually the case after the administration of this medicine.

Belladonna.—Belladonna administered to several subjects by the digestive tract, and by the skin, introduced twice into the subcutaneous areolar tissue by a recent wound, ejected once into the bladder, and once into the cephalic vein. The observations may be given in the following *resumé*:—

1. Cholera cyanica with asphyxia. Six grains of extract of belladonna by the mouth during the period of asphyxia, eleven hours before death; no result.

2. Cholera cyanica with asphyxia. Eight grains of extract of belladonna taken into the stomach in sixteen hours. No signs of absorption; death.

3. Cholera cyanica with asphyxia. Eight grains of extract of belladonna introduced into the stomach, and retained for seven hours. No dilatation of the pupils. At the autopsy, there was found in the stomach the mucus, covered by little dark marks made by the belladonna.

4. Cholera algida cyanica with asphyxia. Twenty-two grains of extract of belladonna in the stomach and rectum in about thirty hours. No dilatation of the pupils. Frictions in the axilla, with the pommade of belladonna five hours before death; no effect.

5. Very intense cholera in a pregnant woman. Frictions in the axilla, with the pommade of belladonna. Ten grains of calomel taken by the mouth during the algid stage; no signs of absorption. The next day (there had been cya-

nosis, but no algidity; the pulse was perceptible) the application of belladonna on the eyelids produced slow and incomplete dilatation. Many days afterwards the same application caused rapid dilatation. The same results were noticed upon the introduction of belladonna into a recent wound (Case 10). The injection of two grains of the extract into the cephalic vein produced complete dilatation of the pupils in three minutes.

Sulphate of Strychnine.—This substance appears of all others most refractory to absorption; and has failed, contrary to the assertions of some authors, to influence the beatings of the heart. The conclusions drawn from these experiments are absolute. Patients suffering from algid cholera have lost the faculty of absorbing. If in any case absorption is not positively null, it is at least so feeble that no dependence could be placed on it for a therapeutic result. Then, medicines accumulated in the digestive canal may become positively injurious when reaction is established, and absorption again goes on.

The immediate effects produced by the injection of belladonna into a vein may direct the attention of practitioners to this method of administering medicine.—*Med. Times and Gazette*, October 14, from *Gazette Hebdomadaire*, Sept. 8, 1854.

22. *Nature and Causes of Green Vomiting.* By Dr. FRAZER, of Dublin.—The ejection of green-coloured fluids from the stomach is noticed in the course of many abdominal diseases; thus, we meet it during attacks of peritonitis, puerperal fevers, and continued fevers with gastric complications, in strangulation, in intussusception of the bowels, in that rare affection, acute gastritis, and in various enteric inflammations. Its appearance has been attributed by many to the presence of bile in the secretions evacuated during protracted vomiting, and we often find it described as consisting of *green bilious matter*. For various reasons, this explanation did not appear at all satisfactory to me.

I consider this green matter is composed of the blood in an altered and modified condition, the discharge being altogether of a hemorrhagic character. Repeated microscopic observations, in those instances in which I have noticed its occurrence for some time past, has confirmed my ideas regarding its nature, and examination shows that the little fragments of which it consists are various sized clots, containing abundance of blood-globules; this alone might decide the inquiry; however, I am desirous of placing it on other grounds, in addition to such as are afforded by the microscope. I may, therefore, bring forward the following arguments, which further support the view I have taken. 1st. The form of vomiting alluded to occurs in diseases where there is much congestion, and often inflammatory engorgement of the vessels of the stomach or intestines, as in gastritis, and fevers with gastric complications; and after death we find patches of softening in the mucous membrane, or decided marks of excessive vascularity; at least such has been the case in those fatal instances where I have had the opportunity of inspecting the stomach. 2dly. The coincidence of vomiting of ordinary blood, in addition to the green matter, which sometimes occurs (as was noticed, for example, in the case of the woman with enlarged spleen, already alluded to); besides, I have remarked that, in many cases of fever which were attended with this peculiar green discharge, hemorrhagic purpuric eruptions were frequently seen. 3dly. We have the physical properties of this substance; it forms small green clots in the fluid, in addition to tinging it with its peculiar hue, as we might expect blood to do. 4thly. We must consider that a similar green colour occurs in various parts of the body, and under very dissimilar circumstances. This is, perhaps, the most interesting branch of our subject, and one on which medical records afford little information; as instances of this change, we have, among other examples, the peculiar green hue of contusions that are of some standing, and the green colour of the ecchymoses that occur on the extremities in land scurvy, many cases of which were noticed in the hospitals, in the late years of famine. The colour of the great intestine in various cases of fatal dysentery, and of the mucous tissue of the stomach in inflammatory gastric affections. The grass-green appearance of the interior of the bladder, and of the kidneys, after death, in many acute or sub-acute diseases. The well-known greenish tinge often seen in moist gan-

greenous affections. The colour of the body, as it begins to decay after death. The green hue of coagulable lymph, and of purulent exudations in bad constitutions, and many other similar facts in pathology that might be enumerated, which are as yet unexplained.

If we reject all extraneous considerations from the foregoing class of morbid changes, we have, I conceive, one uniform fact in them all, namely, the presence of blood undergoing some of its mysterious changes; what these are will require additional investigation; but I think we may conclude that the death of the blood is essential—that having lost that vital attraction for oxygen which distinguishes it, and acted on by extraneous sources (probably deoxidizing ones during decay), it becomes changed in its hue.

With regard to its practical bearings, there is much importance in the view now brought forward; it cannot be regarded as a mere theoretic matter, and, therefore, of secondary value, for those green discharges being recognized as hemorrhagic, will go far to account for the collapse, cold extremities, failing circulation, and other alarming symptoms that attend its presence when ejected in any quantity; and with reference to *treatment*, which is the ultimate aim of medical knowledge, we have a rational explanation of the benefit that creasote, ice, and other well-known styptics exert in checking it.—*Dublin Hospital Gaz.* Oct. 15, 1854.

23. *Treatment of Certain Forms of Constipation.*—Mr. Baly read before the North London Medical Society, Oct. 25th, 1854, a communication on this subject. A young lady came under his care in the autumn of 1853, anæmic, and of feeble constitution; had been troubled with habitual constipation from childhood; from the age of fifteen had constantly had recourse to large doses of aloetic aperients; and for many years had not had an action of the bowels without their aid. Latterly, she had frequently used injections; but these now fail frequently, and at the present time she generally obtains relief, every three or four days, by a dose of blue-pill and colocynth at night, followed by an enema in the morning; the latter frequently requires to be repeated. She complains of great debility; inability to make exertion, mental or bodily; temper irritable; spirits depressed; pulse feeble; catamenia regular, but pale; frequently suffers from cold extremities; appetite small. When the bowels have not acted for three or four days, she suffers from a sense of fulness and distension of the abdomen; she frequently, after an evacuation, suffers for some hours from severe prostration. In this case, Mr. Baly revived the treatment proposed by Dr. Strong in 1842, it being evident that the continued employment of any form of aperient would be hurtful. Having, as a preliminary measure, cleared out the bowels with a dose of blue-pill and colocynth, followed by an enema, he ordered sulphate of zinc, five grains; bread-crumbs, sufficient for a pill, to be taken three times a day, immediately after each meal. This was continued for ten days, without causing sickness, and with decided relief to the abdominal distension; the appetite improved; the listlessness decreased. No evacuation of the bowels occurred from the second day till the tenth, when the following was administered: Calomel, four grains; compound colocynth pill, six grains; make a pill, to be taken at bed-time, followed by a black draught in the morning. On the day following this, the bowels acted spontaneously; and from that time until now, nearly twelve months, have been relieved daily without aperients or enemata. The use of the sulphate of zinc was continued for three weeks, and then gradually exchanged for sulphate of quinia. A curious fact deserved notice in this case. For some time after the discontinuance of enemata, the patient was not sensible of the action of the rectum during the passage of feces. Other cases were cited in support of this treatment, the use of which Mr. Baly limited to cases dependent upon want of tone.—*Lancet*, Nov. 4, 1854.

24. *Treatment of Diarrhœa in Phthisis.*—Among the out-patients attending at the City Hospital for chest diseases, cases of diarrhœa have been unusually frequent during the last two months, in consequence, no doubt, of the epidemic influence now prevailing. Those occurring to the subjects of phthisical disease

have, in many instances, been more difficult to control than usual. The cases not being of the acute or choleraic type, the sulphuric acid treatment has been tried in but few instances. The remedy chiefly relied upon has been opium, exhibited in one or other of the following forms: In mild cases of relaxed bowels, attended with much irritability of the stomach, the diarrhœa being liable to be set up again by every slight dietetic cause—R. Bism. trisnitr. gr. x, mucilaginis ʒij, vini opii ℥x, acid. hydrocy. dil. ℥ij, aq. pur. ʒj. Ft. hauster die sumend. This mixture, continued for a week or two, has often sufficed to relieve the condition alluded to, and to enable the stomach to tolerate, not only ordinary plain food, but even the cod-liver oil. In more severe, suddenly occurring cases—R. Pulv. kino co. gr. x, o. n. vel. o. n. et m. sum. Patients liable to attacks of sudden and severe diarrhœa have been directed to keep these powders on hand, and take them at once on a reappearance of the symptom. In many cases of the latter class, Dr. Peacock prefers to the kino a pill containing the acetate of lead and opium (gr. ii and gr. ss), given either every night or twice in the day, as circumstances indicate. The employment of the emulsion of mutton suet and milk in cases of irritable bowels is a very favourite practice at this hospital, and, in the milder class, frequently suffices, without resort to other remedies. The patient is directed to procure fresh mutton suet, cut it very fine, tie it loosely in a coarse muslin bag, and let it gently simmer in milk for a few minutes, after which a quantity of the best cinnamon is to be grated in. The quantity taken may depend upon the patient's palate, and it is generally well relished. The cinnamon probably exerts, as an astringent, an important influence, and should never be omitted.—*Med. Times and Gaz.* Sept. 9, 1854.

25. *Treatment of Albuminuria after Scarlatina.*—Formerly it was customary to treat this and all other dropsical affections with diuretics. Prof. MAUTHNER, however, in the dropsy which occurs after scarlatina, and is accompanied by bloody and albuminous urine, sedulously avoids whatever might overstimulate the kidneys, as likely to cause an acute renal inflammation, or an attack of Bright's disease. He has seen many cases where the swelling increased daily in spite of the administration of squills, digitalis, bitart. potassæ, &c., in which a cure was effected by a simple regulation of the diet. In such cases he advises the exclusive administration of milk, and rice and milk, or at most of decoct. althææ, or of decoct. lini sem. By the exclusive use of milk diet, he has seen the action of the kidneys so regulated, that a copious secretion of urine, free from albumen, was established, by means of which the dropsical condition was removed. When the administration of milk fails to effect a cure, he endeavours, by means of alkaline agents, to alter the condition of the urine; and, for this purpose, he prefers using the alkali of the urine itself. He accordingly administers urea in small doses (either in the form of pure urea, or of the nitrate of urea), giving generally the third of a grain for a dose, in combination with a little powdered sugar. After six or eight grs. of the urea had thus been taken, he has frequently seen profuse diuresis occurring, followed by the speedy disappearance of the dropsical affection.—*Monthly Journ. Med. Sci.* Oct. 1854, from *Journ. für Kinderkrankheiten*, 1854, Heft i. and ii.

26. *Treatment of Itch by Frictions with common Vinegar.*—Dr. LE CŒUR, Prof. in the Preparatory School of Medicine at Caen, states (*Union Médicale*) that he has successfully treated two cases of itch by this means. He uses a rather coarse sponge dipped in strong vinegar, with which the affected parts are three times a day rubbed with sufficient force to penetrate the skin and rupture the vesicles. The mean period of treatment has been five days.

27. *Belladonna in Profuse Salivation.*—A woman who had been treated profusely with mercury for the cure of enteritis, had violent salivation from the use of the drug. Extract of belladonna was ordered by ESPENBECK, in doses of 2½ grains in an emulsion; and the following day the salivation was found to be completely arrested, and the mouth dry. When the administration of the belladonna was suspended, the ptyalism returned; and again it disappeared when

the use of the drug was resumed. Espenbeck has employed it successfully as a prophylactic against salivation.—*Hannover Corresp. Blatt.* June, 1853.

28. *Cubebæ in Infantile Eneuresis.*—Dr. DEITERS has found cubebæ more effectual than any other remedy in curing the incontinence of urine so common among children. This complaint may depend upon atony of the bladder, or on the presence of intestinal worms. In the former case, the cubebæ act as a tonic, in the latter, as a valuable anthelmintic. The medicine requires to be given in considerable doses; two pinches (*i.e.* a few grains or *Zwei Messerspitzevoll*) for infants, and half a teaspoonful twice or thrice daily for children of a somewhat more advanced age. Its effect is speedy and permanent; and although occasionally it happens that during its administration the incontinence returns at periodical or irregular intervals, these recurrences gradually become less frequent, and eventually disappear altogether. To effect a radical cure, the author has often found it necessary to continue its use for a period of from three to eight weeks, and he has never observed any injurious effects from its administration.

Deiters observes that he has found the same remedy most efficacious in checking nocturnal emissions in cases of spermatorrhœa.—*Monthly Journ. Med. Sci.* Oct. 1854, from *Preuss Verein Zeitung*, No. 16, 1853.

29. *Birch-Oil in Eczema.*—BLAZIUS has been in the habit, for upwards of fifteen years, of using *Oleum Rusci*, the empyreumatic oil of the common birch (*Betula Alba*), with great success for the treatment of eczema. The affected parts are well smeared with the oil every day, and then enveloped in linen cloths; after this has been done for a few days, they are well cleansed with soap and water, and then the application of the oil is recommenced. This treatment is continued until not only the formation of vesicles and secretion of fluid are arrested, but until the skin resumes its normal whiteness, smoothness, and softness. Blasius does not recommend the application of the oil so long as the eczema continues to be *acute*, and attended with inflammation; and when, in *chronic* cases, it occasions pain, burning, and itch, attended by swelling and redness of the parts, he discontinues its use for a few days, until these troublesome symptoms have quite subsided. This oil is called “Dagged” in Russia and Poland.—*Monthly Journ. Med. Sci.* Oct. 1854, from *Deutsche Klinik*, No. 29, 1853.

30. *Extractum Sanguini Bovini.*—This remedy is beginning to be much used by German physicians. It is prepared by Mauthner's process by straining the blood of the ox through a fine sieve, and drying and powdering the residue. Of this, Dr. Höring gives from ten to thirty grains in the twenty-four hours, according to the age of the patient. He relates the particulars of three cases in which he administered it; the first, a case of curvature of the lower part of the spinal column, with loss of power of limbs; the second and third, cases of tuberculosis, occurring in a child aged 3, and a man aged 40. The treatment was successful in every case, the curvature being arrested in the first, and the cough and emaciation greatly improved in the second and third.—*Monthly Journ. Med. Sci.* Oct. 1854, from *Wirtemb. Corresp.* Bl. 32, 1853.

31. *Hydrophobia.*—Dr. ZIMMERMAN has given (*Casper's Vierteljahrschrift*, Bd. iv. Heft 1) an account of an epidemic hydrophobia which prevailed at Hamburg in 1851 and 1852. Dr. Z. knew of thirty people bit by mad dogs, not one of whom became hydrophobic—not from treatment preserving them, for only a few were treated, and they only with the usual external appliances—and he and his friends heard of many more who had likewise been bitten without any bad consequences, and without employing any, even the simplest, preventive measures. In fact, during the whole course of the epidemic, the only man who became *hydrophobic*, and died from it, was bitten by a dog *not mad*. The dog was killed, and examined by Veterinary Surgeon Schröder, who had made 120 dissections of dogs during the epidemic, and who distinctly asserted there was little or nothing wrong with any organ in the body, and certainly not

a trace of the appearances proper to *rabies canina*. Zimmerman says, that in all known cases the disease found its origin only in the bite of a dog already mad, and that other animals, as horses, so bitten, likewise became mad. The last opinion seems to require confirmation; the former is not borne out by his facts of fifteen dogs bitten by a known mad one, two only became mad; two ran away, eleven remained healthy.—*Monthly Journ. Med. Sci.* Oct. 1854.

32. *Cutaneous Anæsthesia in Lunatics*.—A lunatic received a severe injury of the great toe by the fall of a heavy piece of wood, so that the nail was torn away. Dr. SNELL, on examining the patient, remarked that he seemed scarcely to feel this injury, ordinarily so very painful. On examining the case more minutely, he found that this lunatic had completely lost cutaneous sensibility. This circumstance led him to examine all the lunatics in his establishment, and he found that out of 180 insane persons (100 males and 80 females), 18 (17 males and 1 female) had complete anæsthesia of the skin; and that in 6 others, who were males, the sensibility to pain was very much decreased.—*Monthly Journ. Med. Sci.* Oct. 1854, from *Zeitschrift für Psychiatrie*, x. 2.

33. *Lumbrici in the Left Pleura*.—Lumbrici have been found in many parts of the abdomen, and even free in the peritoneal sac; but Professor LUSCHKA communicates an extraordinary case, in which, through the intermediate process of a retro-peritoneal abscess, four lumbrici were found encysted in the left pleura.

A man, æt. 23, who two years before had had slight peritonitis, suffered, in 1852, from return of this complaint, with especial pain in the left lumbar region; death ensued, with typhoid symptoms. In the left pleura, between the lower lobe of the lung, the thoracic wall, and the diaphragm, there was a sac formed of pseudo membrane, in which six lumbrici and a large quantity of brown fluid were contained. An opening in the diaphragm led into a cavity formed by adhesions between the upper end of the descending colon, the left kidney, and the diaphragm, and in which some lumbrici were also contained; this cavity, or abscess, communicated with the descending colon by three contiguous openings, situated on a level with the under part of the spleen. In the colon there were also lumbrici.—*Brit. and For. Med.-Chirurg. Rev.* from *Archiv. für Pathol. Anat.* bd. vi. heft 3.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

34. *Curability of Cancer, and its Diagnosis by Means of the Microscope*.—At a meeting of the Academy of Medicine, held on the 26th September, M. JOBERT presented a report on a case of sarcocele in a young infant, in which M. Pamard, of Avignon, operated, and cured the little patient. The alleged cure in this case gave rise to an interesting discussion, which is not yet concluded, on the curability of cancer, and its diagnosis by means of the microscope.

M. ROBERT doubted if encephaloid cancers were ever cured. The little patient had, doubtless, recovered from the disease, but it was not exempt from its recurrence.

M. VELPEAU moved that the question of cancer should be brought before the Academy at an early meeting. All surgeons were not of M. Robert's opinion as to the incurability of cancer. He expressed doubts as to the value of the microscope in the diagnosis of cancer, and undertook to prove its fallibility in the diagnosis and prognosis of cancerous tumours.

October 3d.—On the President announcing that this was the day appointed for the discussion of cancer, much amusement was created by M. Velpeau declining to speak before M. Robert, and M. Robert refusing to speak before M. Velpeau.

M. LEBLANC opened the debate on the curability or incurability of cancer, by

briefly going over the researches which he had made on the subject with M. Trousseau, and those he had lately made microscopically, conjointly with MM. Lebert and Follin. He considered the microscope of great service for the diagnosis of the cancers so common among animals. He did not believe in the incurability of cancer; and considered that an *early* operation diminished the chance of recurrence. His speech contained much valuable matter, but no new facts.

M. BARTH followed, making, on this occasion, his *début* in the Academy. His address was a very excellent one. As to *diagnosis*, he considered that, in a very great number of cases, this may be made with the naked eye, but that sometimes the characters were better seen by means of the microscope. In these first cases, the microscope afforded additional certainty of diagnosis. In a certain number of cases the physical characters are doubtful, and in these the microscope furnished an essential element of diagnosis; but it did not always remove doubts, and might sometimes even occasion mistakes. As to the *curability* of cancer, he thought that absolute incurability was disproved by facts; and unsupported by analogy. We should take into account all the characters of the tumour—those inherent in it and accessory to it—in order to determine the possibility of chances of recurrence.

M. GERDY made some very interesting and novel observations on the anatomico-pathological characters of the sanguineous cancerous tumours. It was singular that, although these tumours were known in science, there was nothing said about them in books. How were they to be distinguished? Chiefly by their anatomical characters. They were rich in bloodvessels, and in *splenoid* tissue analogous to the placenta. Surgeons know that they recur after extirpation. Some of these tumours were situated in or near the bones, and were vascular and pulsatile, analogous to the spleen or placenta. Breschet, Dupuytren, and Lallemand, called them simple tumours, although they have found colloid matter in them; but they are not such. He considered the microscope useful in many cases, but in others as likely to confuse.

October 10th.—M. ROBERT: I have said that the cure of cancer, especially of the encephaloid variety, is an exceedingly rare fact, if it can be well proved. It seems that M. Velpeau is not of this opinion. On consulting his work on diseases of the breast, I find that he has operated in 250 cases, in which the patients have survived the operations. He lost sight of 100 of these patients after one year, 150 after two years, and 200 after five years. It is matter of regret that, in these cases, the final result was not ascertained. Of the 50¹ remaining, 20 have been free from the disease for five, ten, fifteen, twenty, and even twenty-five years; and these cases M. Velpeau regards as radical cures. This enormous proportion of cures seems astonishing, when we call to mind the results recorded by other eminent surgeons. Munro says that 4 out of 60 cases in which he operated for cancer were free from the disease for two years; but that three of them eventually had occult cancer of the mamma, and the fourth an ulcerated cancer of the lip. *Ergo*, 60 operations and 60 relapses. Mayo says that carcinoma of the mamma, even when operated on under the most favourable circumstances, and removed early, recurs in 95 cases out of every 100. Macfarlane operated in 32 cases of cancer, and collected the particulars of 86 other cases of his colleagues; and out of these 118 cases *there was not one in which the malady did not recur*. Boyer says that out of more than 100 cases in which he has known individuals operated upon for cancerous tumours of the breast, or of other parts, he has only known *four or five cases* which were thereby radically cured. M. Broca, in his prize work on cancer, says, that of 19 cases in which tumours were removed by Blandin, which were demonstrated microscopically to be cancerous, *not one* remained exempt from a recurrence of the disease for two years. Lebert says that, of 34 cases operated on for cancer, 6 died after the operation, 21 had a recurrence of the disease within a period varying from three months to two years; and the remaining 7 were lost sight of. I have said enough to show how unanimous are the opinions of the greatest authors as to the curability of cancer.

I cannot help thinking that the marvellous discrepancy in M. Velpeau's

¹ [There is evidently some error in these figures.—ED. AM. J.L.]

statistics has been due to errors of diagnosis, and to his having mistaken benign for malignant tumours. The surprising cases which he says remained cured for fifteen, twenty, or twenty-five years, must have been seen at a time when an error in diagnosis would have been quite excusable even in M. Velpeau. But M. Velpeau says he has never been deceived; and that in more than a hundred cases his diagnosis has been confirmed by the microscope. This, I would remark, *en passant*, is not very flattering testimony in favour of the microscope. Let me recall to your recollection the discussion that occurred in 1844, in the Academy, on fibrous tumours of the mamma. On that occasion, M. Cruveilhier described very excellently the anatomical and clinical characters of those non-malignant tumours of the breast, which were often confounded with cancer, and showed how he had often dispensed with operations by recognizing their presence. His opinions, and above all, the name of "fibrous bodies," which he gave these tumours, were combated by MM. Bérard, Blandin, Roux, Amussat, etc., who admitted the existence of such benign tumours, but held that it was impossible to distinguish them from cancer. M. Velpeau, on that occasion, defended the opinion of M. Cruveilhier. That he has studied these non-malignant mammary tumours, is evident from a paragraph in his article MAMELLES, in the *Dict. de Méd.*, in thirty vols. He affirms that diagnosis was possible.

But he had not *then* the experience which enabled him *now* to rectify error in the matter. Hear, therefore, how he then expressed himself before the Academy: "M. Cruveilhier has gone too far in affirming that fibrous tumours of the breast may always be easily distinguished from those which are cancerous. In some cases this distinction is very difficult, and even impossible." And he further added: "The defiance which MM. Bérard, Blandin, and Roux have given M. Cruveilhier to recognize on the living woman a benign tumour, I would not be afraid to accept myself; and I do not hesitate to say, that if *I only committed four errors out of ten cases, I would feel quite satisfied.*" Gentlemen, may we not with justice conclude, after this avowal, that among M. Velpeau's cases of cancer, cured before the year 1844, there must have been several patients who were operated on for merely non-malignant tumours? And observe that, by a singular coincidence, the proportion of four errors of diagnosis out of ten is precisely that of the successful cases in his statistics, viz. 20 in 50.

Since the year 1844, the study of benign tumours of the breast has greatly advanced. Velpeau erred at first in calling them fibrinous; and it was only after the researches of Lebert that he gave them the name they still retain, *adenoid* tumours. These researches have greatly elucidated the pathology of tumours, and thus advanced clinical medicine; while the microscope, in revealing their minute structure, has made us acquainted with physical characters never before observed. I cannot admit the infallibility which M. Velpeau claims for himself, for I believe that cases are still met with which may deceive the most able practitioner. It must, therefore, be believed that M. Velpeau's results are not rigorously exact, or capable of proving that the cure of cancer is less rare than is supposed.

Hitherto, we have only been discussing the two kinds of mammary tumours which are liable to be confounded with each other; but if we turn our attention to the tumours of other organs, we will find the causes of the errors of diagnosis to be more numerous, and hence we can understand thereby the diversity of opinion which exists among surgeons as to the curability of cancer. In these organs, besides cancer and hypertrophic tumours analogous to those of the breast, we find, also, epithelial and fibro-plastic growths. The latter, however, present some affinity to true cancer, and their external characters do not permit of their always being clinically recognized; consequently, they may grow again after the operation. Now, persevering microscopical researches, made by eminently qualified and conscientious men, have demonstrated, on the one hand, that the minute structure of such tumours differs essentially from that of cancer, and that often the naked eye may distinguish them from it by incontestable characters. Clinical observation, on the other hand, has shown that, if these growths reappear after extirpation, they do so less frequently than cancer; that the relapses are generally local, and admit of fresh opera-

tions being performed with a better chance of definite success; and, finally, that their becoming generalized, or causing death, only occurs in rare cases, and is exceptional.

If we would put an end to the confusion which exists at present on all that relates to cancer, and above all to its curability, we would sedulously exclude from the question all those epithelial and fibro-plastic growths which have only special analogies to it, but which differ essentially from it in their intimate structure and evolution.

M. VELPEAU: M. Robert, in his speech, has only discussed one side of the question, viz. the curability of cancer; and he has said nothing of the value of its microscopic characters, although he appears to be a partisan of the microscope. He complains that I have not given the definite results in a large number of my cases of operations. Among the 200 patients whom I unavoidably lost sight of, relapses occurred in a certain number of cases, but in the rest I never could ascertain the results. Out of the 50, whom I carefully watched, there are 20, all of whom I could find to-day, perfectly cured. M. Robert explains this considerable proportion of cures by premising errors of diagnosis. I do not pretend to be more infallible than him; I have sometimes been deceived; but there *are* cases in which one *cannot* be deceived. I have taken great care to call *cancer* only those cases which presented all the characters with which it was impossible to be deceived at the bedside. In these cases, the microscope has never shown my diagnosis to be fallacious; and the same has occurred in every case in which I diagnosed an adenoid tumour. Far from undervaluing microscopical researches, I have always encouraged them, for I wish much to see science possessed of more certain marks than at present. But in granting a high place to the microscope for diagnosis, clinical observation must not be excluded. When M. Robert meets an ulcerated, fungous, softened tumour, he cannot be deceived; nor can he, if he meets one which is hard, knobby, and uneven. For my own part, I confess that in such cases I would not be deceived; and that the microscope has always confirmed my opinion, I would appeal to M. Lebert, who has for a long while attended my visits at *La Charité*.

M. Robert, in recalling the former Academy discussion of 1844, wishes to make me contradict myself, in making me say that I had then confessed having been deceived. I still frankly admit that there are cases in which I may be deceived. When I say that I have always diagnosed the tumour, I mean in the cases in which they presented their habitual characters.

I repeat, I have been far from discouraging microscopical researches. For thirty years I have studied the subject of cancer with lively interest, and a sincere desire to arrive at the truth. In 1836, I stimulated M. Donné to make researches in this direction, and later still, I advised M. Lebert to follow out the inquiry. I said to them: "Search; for cancer ought to have a special character." I was especially anxious that something might be found in the blood of the cancerous; for if the characters of cancer could be found therein, nothing would be easier than, by examining a few drops of blood, to diagnose the disease. Microscopists found nothing peculiar in the blood; but at last they discovered a something, and, overjoyed at this, I was one of the first to applaud the achievement. But is that something sufficient? I believe not; and it is because I consider the microscopical characteristics insufficient that I have studied with the greatest care the clinical indications.

I tried, at first, to isolate benign from malignant tumours, and I commenced to separate from true cancer those tumours usually confounded with it. I have now come to the conclusion that, in true encephaloid cancer, the variety which is most prone to return, a cure *may* sometimes take place. In support of this proposition I shall cite some facts, and the Academy will judge whether they are good, or whether they deserve the severe judgment pronounced upon them by M. Robert.

In February, 1836, a woman consulted me, who had on her breast a large tumour, the size of both fists, which was ulcerated and fungous, discharging an insupportably fetid ichor, and which crumbled under the finger. There could be no more characteristic specimen of encephaloid cancer, and although all the microscopes in the world had been testifying against me, I should still

have held this opinion. Well, I operated on this woman. This was nearly twenty years ago; and I saw the patient recently; she was perfectly cured, and has had no relapse.

Another lady had a tumour very similar to that which I have described, which was removed; the wound cicatrized perfectly; but, eighteen months afterwards, a tumour very like the original one manifested itself in the axilla. This second tumour was removed in 1841, and since then she has enjoyed perfect health, and has had no relapse. I may state that M. Jobert was the surgeon who operated the second time. (Here M. Jobert made an affirmative sign.) Are there not other tumours, besides cancerous, which reappear thus, not at the original point, but at a distance? We cannot tell.

I know that to these facts an objection may be raised. It may be said: "How do you know that the recurrence, which has not happened at the end of two, three, five, or ten years, may not take place in twenty, twenty-five, or thirty years?" and in this manner it may be said, if no return of the disease has occurred, that the patient has died before there was time for its manifestation.

It may be objected that, in the cases I have just reported, the microscope was not used; but since 1847, when the use of the instrument became general, every tumour that I have extirpated has been examined by means of it. To those who could say, with great justice, that I am not sufficiently qualified for microscopical researches, I would observe that the cancerous growths were examined for me by the learned young microscopists who attended my visits, by M. Follin, M. Robin, M. Broca, and M. Lebert himself. Now, since this time (1847), all the cases in which I extirpated malignant growths, and in which no recurrence took place, were previously demonstrated by the microscope to be true cancerous tumours. It may be remarked, however, that sufficient time has not yet elapsed to enable us to say with certainty that these patients are exempt from any chance of a relapse. But we know that when recurrence of the disease happens, it is generally within two years after the operation, and that after this period the chance of this becomes much lessened. Now, these patients have been free from disease for *seven* years. I think, therefore, that it is proven that cures have happened in cases where the existence of cancer had been microscopically examined. I maintain that errors of diagnosis were impossible in the cases I have cited.

M. Robert thinks that I confound different things together; as, for example, epithelial and fibro-plastic tumours. This objection brings me back to the microscope, on the use of which I wish to make a few remarks.

Laënnec divided tumours into two classes, which are merely those called, now-a-days, homeomorphous and heteromorphous growths. Among the *heteromorphous* tumours we class encephaloma, schirrus, phagedenic ulcers, etc. Among the *homeomorphous* we rank fibrous and fibroid tumours, cutaneous excrescences, condylomata, corns, fungosities, etc. Can it be said that there is the least analogy between a malignant fatal tumour and a corn on the foot? The microscope reveals the same minute structure in a venereal vegetation and in an excrescence (*verruë*). Are they, then, the same thing? M. Lebert having remarked that the tumours in question contain only epithelial matter, concludes that they are not cancer. He must have seen many cases, however, since the publication of his *Traité de Physiologie Pathologique*, to convince him that it is very difficult to reconcile clinical facts with the results of microscopical observations.

He says that the composition of these tumours explains their non-recurrence; but this is erroneous, for epithelial tumours return like others. Nay, more, I declare that they return *oftener* than other tumours. However, M. Lebert said to me: "You do not distinguish, but confound together tumours which are different; those observations only are valuable which are confirmed by the microscope, and all preceding ones are worthless." I cannot entertain this objection. However, I commenced investigating anew; I submitted cancers of the lips to M. Lebert; he found them epithelial, and consequently pronounced them to be non-malignant ulcers. Yet, in the most of these cases, the disease returned after the operation. In the majority of cases, ulcers of the lips are

epithelial tumours, and they return like cancers. M. Lebert has been obliged to acknowledge this; but he says that, unlike cancer, they grow on the same place, and produce no general infection. I would answer, that cancer also sometimes returns on the same place; and I would ask if it is true that epithelial growths only return on the same site? I am inclined to dispute it; and Lebert and his disciples have attempted to get out of the difficulty by saying that, when epithelial cancer does not return at the same place, it recurs in the neighbouring lymphatics (*ganglions*); that is, in tissues directly continuous with those in which the malady primarily appeared. But does not this also happen in cancer?

Homœomorphous tumours, therefore, are exactly like cancer, for they recur both at the same place, and at a distance from it. The micrographers say that they never occur except in the neighbourhood, or in the lymphatics. I need not, in answer, adduce old cases of recurrence in the viscera, for in these the growths were not microscopically examined; but I may point to recent cases in which homœomorphous tumours recurred in different tissues from those primarily affected—as in the bones.

The treatment of the two classes of tumours is the same; we do not apply caustic, the cautery, or the knife, any less to epithelial than to cancerous growths. Wherein, then, do they differ clinically? It may be said that epithelial tumours do not cause death so speedily as cancerous, the latter often recurring in the viscera. But if both are fatal, what matters it where the recurrence happens? To recapitulate: epithelial tumours recur after extirpation, not only on the original site, but, like cancer, at a distance from it; they return alike in homologous and heterologous tissues, in the bones, and in the viscera. Call them epithelial cancers, if you will, to distinguish them from cancer, properly so called, and from scirrhus or encephaloma, but they will always be regarded as cancerous by me.

The so-called fibro-plastic tumours are said to be another variety of homœomorphous growths; but I cannot accept this distinction as a decided fact. It is said that they are simple hypertrophies of a normal tissue, or a production of a tissue analogous to normal tissues. They are evidently another thing. How can we compare, for instance, the enormous fibrous tumours sometimes found on the shoulder; and yet the microscope reveals the same texture in both. Keloid is also fibro-plastic tissue; but it does not resemble the growths of which I have spoken, and yet they are not distinguishable by the microscope. I have seen enchondroma return; I remember removing a tumour of this kind from the foot, which reappeared, after some years, on the thigh. You say, perhaps it was cancer; we need more of such cases. There have been more similar to this, and confirmatory of it. I will quote one: A man presented himself at *La Charité*, having a tumour which had grown on the site of a similar one removed by M. Malgaigne; and, some years before that, a tumour of the same kind had been excised by M. Blandin, so that this was the third relapse. The new tumour had burrowed under the cicatrix of the last operation. In spite of the small chance afforded by another operation, I yielded to the earnest solicitations of the patient, and removed the growth, cutting round it very deeply, so as to make sure of leaving none of it in the wound. All went well, the patient seemed convalescent, when I observed, before the cicatrix had formed completely, a nodule commencing below it. We allowed the wound to cicatrize, and I sent him away without saying anything, firmly resolved to make no fresh attempt. Some time after, the tumour was again developed, and the patient, knowing my determination, went to the *Hôtel-Dieu*, to be under the care of M. Roux. Amputation of the thigh was performed, and he left the hospital. He entered the medical wards of the *Hôtel-Dieu* some time afterwards, complaining of something in the chest. All at once a fresh growth had occurred on the leg, and simultaneously tumours of the same nature had developed themselves in the thoracic viscera. The patient died, and his body was examined by MM. Follin, Giraudet, and Verneuil, who found in those tumours nothing but purely fibro-plastic tissue.

I could cite many similar cases; but one or two must suffice. A most beautiful young woman had an enchondromatous tumour on her arm, which had

increased to such an extent that her only chance of cure lay in disarticulation of the arm. The patient declined the operation, but remained in the hospital. The tumour grew as large as the head, and she died, and at the autopsy similar tumours were found in the lungs. M. Richard, a great partisan of the microscope, examined these growths, and found them to be composed of only fibro-plastic tissues. I remember another case of enchondroma of the foot, which returned after removal; I amputated the leg, and it recurred on the thigh; amputation of the thigh was performed, but the tumours became developed in the lungs.

M. Lebert is aware of these facts, and he admits the recurrence of homœomorphous tumours on the original place, and even at a distance in the neighbouring lymphatics. But that is not enough; he could not deny the existence of cancerous cachexia in an individual having canceroid who had become emaciated, and had a sallow icteric tinge. *There are several species of cancer—cancers with and without cells.*

As regards the cancer-cell, what have we to prove that it is the pathognomonic character of cancer; that it is not, for example, a transitory condition as yet but imperfectly understood? At present, there are two classes of microscopists, viz. the clinical and the scientific; the latter variety, who are principally in Germany and in England, naturally attach an absolute value to microscopical characters. The clinical micrographers are less absolute, and I have more confidence in them. Their assertions, however, must be taken with some reserve. For example, Lebert says: "There is nothing pathognomonic of cancer, except the cell." Now the cell is not pathognomonic, for we now know that it is the nucleus which is most important. But I am not more predisposed in favour of the nucleus than the cell. Very likely the nucleolus may, next year, be regarded as the all-important element. I sincerely trust that microscopists may arrive at something definite. If I have been somewhat severe upon them, it is because I think they have been too rash, and because I dreaded their going astray. They have affirmed that the cell is pathognomonic of cancer, and when experience demonstrated that their proposition was not always true, they actually tried to explain these pretended anomalies by an alteration of the cell, capable of making it momentarily disappear. If the cell is not *constantly* found in cancer, it cannot be said to be a pathognomonic mark of it.

The truth is, whatever be the explanation of it, *the cell does not always exist in true, well-developed cancer* (I speak of encephaloid); *and it is sometimes found in tumours which are evidently not cancerous.* I could quote many illustrative cases, and especially that of a woman with a non-malignant tumour in which the cells were found, who never had cancer, and who was not the least likely to have it. Many years ago, I removed a sanguineous cyst from the lip of a young girl, in the matter contained in which, M. Lebert found cancer-cells. The girl recovered perfectly; she has since been married and had several children, and is now in the best health. I removed a non-malignant tumour of the calvarium, in which the microscopists found cancer-cells. The following seems to me very conclusive: A woman, about forty, had, on the right breast, an encephaloid lardaceous tumour, of that worst kind which returns so soon, and always fatally. As the woman was young and fresh-looking, I removed the growth. The tumour was lardaceous scirrhus, through which were scattered, here and there, portions of cerebriform tissue, not yet softened. I cut this tumour into five pieces, one of which I sent to each of five microscopists, asking their opinion of it. *None of them could find a cancer-cell in it.* The poor woman appeared at first to convalesce, and to confirm the diagnosis of the micrographers. But, after a few months, a small tumour appeared near the cicatrix, which was soon followed by general infection, and she died ten months after the operation. A few years ago, I performed amputation of the thigh, on a lady, for a tumour containing cancer-cells, and she is now in perfect health. Fifteen years ago I performed amputation for a disease of the external condyle of the tibia, containing cancer-cells, and the patient is still alive and well.

I may remark that cancers containing *no* cells have occasioned the recurrence of cancers containing cells. We must, therefore, admit one of two things—either that the recurrence was not of the same nature as the primitive malady,

or that the first tumour was the same as the second, but containing no cells. I recollect a case where a tumour of the jaw recurred. There were cells in the second tumour, but none in the first. Must we, therefore, conclude that the first tumour was epithelial and the second cancerous? Assuredly not. The nature of a tumour cannot be judged of merely by the presence or absence of cancer-cells.

All these facts constrain me to declare that the microscope has not as yet aided much our diagnosis of cancer, and the question of its curability or incurability. I believe that there is in cancer some unknown and peculiar element which has hitherto escaped our observation. Else why should not the microscope be able to distinguish those fibrous tissues which recur from those which do not? or why should it not explain these recurrences? But, on the other hand, I am convinced that, in doubtful cases, the microscope may do us great service by confirming our diagnosis. The microscope is an additional eye which we possess; but we should never forget that this eye cannot enable us to see that which does not exist. It is difficult to avail ourselves of the instrument, and this is demonstrated by the frequency with which we see the most expert microscopists at variance among themselves.

This is only a part of what I would say on the subject. The continuation of the discussion may probably furnish me with an opportunity for still further giving expression to my sentiments.—*Monthly Journ. Med. Sci.* Nov. 1854.

35. *Difficulty of Determining the Structure and Nature of Cancer.*—Some time since, the Physiological Society of Edinburgh appointed a committee to report on this subject. At a meeting of the Society, on the 22d of last April, Prof. BENNETT stated that the committee had been dissolved without arriving at any report. In proposing the formation of that committee, Dr. Bennett said he had conceived it possible for four or five members of the Society, all of whom were acquainted with the present state of our knowledge concerning cancerous and other growths, to arrive at definite conclusions as to the structure, nature, and definition of such formations. He regretted to say that the committee could not come to a unanimous opinion on any one point concerning them, from which might be inferred the excessive difficulty of framing satisfactory conclusions on the subject in the present state of science. He hoped, however, that on some future occasion, when our knowledge was more advanced, some other committee might prove more successful.—*Monthly Journ. Med. Sci.* Nov. 1854.

36. *Researches upon the Structure of Erectile Tumours, and upon a New Form of Tumour confounded with them.*—Prof. ROKITANSKY has already described, in his *Treatise on Pathological Anatomy* (1846, vol. i. p. 276), under the name of cavernous tumours, erectile tumours as products of new formation. More recently, in an essay published in the *Transactions of the Academy of Sciences* at Vienna (1852), he brought forward additional arguments in support of his former opinions. The present work completes his observations upon this form of tumours, which is particularly interesting to the surgeon. The stroma of erectile tumours is formed by a network of fibres analogous to those of cellular tissue, the thickness of the septa being very variable. From the thicker septa there radiate others more delicate, which incompletely circumscribe irregular spaces communicating one with another. In these spaces there is found blood, liquid or coagulated, and concretions resembling phlebolithes. The seat of these tumours is very variable; most commonly they are met with in the liver. Then, in order of frequency, should be cited the subcutaneous cellular tissue, the dermis, the face, the trunk, the limbs, the bones of the skull, the dura mater, and the pia mater. They are always attached to veins, which penetrate in general into their texture. The erectile tumours of the liver correspond with the ramifications of the vena portæ. Surrounded by a fibro-cellular capsule, these tumours exert, by their pressure, an injurious influence on surrounding structures. The primitive element of development in these tumours consists, according to the Viennese Professor, in transparent, smooth, but sometimes slightly striated fibrils; moreover, there are found oblong nuclei and caudate, fusiform cellules. It is uncommon to meet with an epithelial in-

vestment. Organic muscular fibre has not been seen by the author. The principal demonstration of the independence of these erectile tumours from surrounding vessels, at the commencement of their formation is derived from the examination of the liver. In this organ there are met with, by the side of hollow tumours, others completely analogous, but in which the vessels are not yet developed. The blood does not enter the tumour but by the perforation of the vacuities. Thus it will be seen why Rokitansky uses the term "cavernous tumours" in place of telangiectasis.

The new form of erectile tumour, described by Rokitansky, is of glandular and lobulated appearance. Each lobule is maintained in its proper relation by a sort of cellular tissue, which sometimes contains fat lobules. This form of tumour has been observed twice in the upper lid, and once in the skin of the arm. The lobules of pinkish-red colour, contain vascular loops, which resemble the substance of the kidney. Each vascular loop is formed of two fine superposed structureless membranes. Moreover, oblong nuclei and caudate cells correspond with the loops. The loops furnish ramifications which terminate in a cul-de-sac. Rokitansky regards these vessels as of new formation, and without connection with the normal canals of circulation.—*Med. Times and Gaz.* Oct. 14, 1854, from *Zeitschrift. der K. K. Gesellsch. der Aertze zu Wien. von Hebra*, 1854.

37. *Pathological Anatomy of Ranula*.—Dr. C. O. WEBER relates (*Virchow's Archiv.* Bd. vi. Heft. 2) the particulars of two cases of ranula, treated in the Surgical Clinique at Bonn, and presents some remarks to illustrate the pathology of the disease, which, notwithstanding the researches and writing of many authors, seems to be yet involved in some obscurity.

A smith, aged 33, applied, in the winter of 1851-2, at the Surgical Clinique, in consequence of a plastic membranous exudation on various parts of the mucous membrane of the mouth, following an attack of chronic diphtheritis. The use of the knife and scissors, and the application of hot iron, ultimately got rid of the affection; but, in May, 1852, there formed, close to the frænum linguæ, on the right side, a ranula, which required many operations for its complete obliteration. The patient, who was also suffering from hydrocele, left the institution in July, but returned in October, to have the radical operation performed on the tunica vaginalis. The tumour under the tongue had reappeared, and required the reapplication of the knife and cautery. In January, 1853, the ranula had formed again, having acquired the size of a hazelnut. The contents were squeezed out, and collected for examination. The greater part of the cyst was excised, the operator having seized it with hooked forceps; the small piece which remained was rubbed with caustic. This proceeding seems to have been successful, for there was no return of the disease in January, 1854, when the patient was again seen on account of the hydrocele. The extirpated piece of the cyst was examined. It was covered by the mucous membrane of the mouth, and was composed of firm areolar tissue, containing many bloodvessels. The fluid was thick, greenish, and transparent, and contained epithelium and mucous corpuscles. Acetic acid caused immediate coagulation.

Catherine Korff, aged 27, was the subject of ranula on the left side of the frænum linguæ, for which Dr. Writzer performed the following operation: He punctured the cyst with a trocar to collect the fluid pure. The anterior part of the cyst was then removed, and the remainder touched with tincture of iodine. The case terminated favourably. Dr. Boedecker examined the fluid, and pronounced that it contained neither albumen nor paralbumen in remarkable quantity, but some pyin. Its chief constituent (besides water) was mucus. The characteristic constituents of saliva, namely sulpho-cyanate of potash and phyalin were absent.

Thus, the chemical analysis of the fluid of ranula does not allow us to pronounce the disease to be the dilatation of the duct of a salivary gland; it rather shows the analogy with synovial sacs and mucous cysts. Anatomical investigation points out the following structures under the tongue capable of being developed into cysts: 1. The salivary glands and their ducts, namely,

the submaxillary and the sublingual glands and Wharton's duct. But these ducts are composed of a firm areolar tissue, with elastic and flat muscular fibres; the interior is covered by cylindrical epithelium. The cyst of a ranula is formed only of areolar tissue, and contains a little tessellated, but no cylindrical epithelium whatever. 2. The mucous glands of the membrane of the mouth. Dupuytren believed that obstruction to the duct of one of these glands might produce ranula; and this view is somewhat supported by the examination of their minute structure; but the author rejects it upon anatomical grounds. 3. The synovial sac described first by Fleischmann (*De novis sub lingua bursis*, Nuremberg, 1841), the presence of which has been confirmed by Froriep. It lies close to the frænum linguæ, on the outer side of the genioglossus under the mucous membrane of the tongue, sometimes only on one side, sometimes on both, but never between the two muscles. The author, after pointing out the necessity of taking all these structures into consideration in the examination of cystic tumours in the floor of the mouth, expresses his belief that the cysts described by Fleischmann form the ranula commonly seen in surgical practice.—*Med. Times and Gaz.* Sept. 30, 1854.

38. *Treatment of Aneurism by Compression.*—JOLIFFE TUFNELL, Esq., has published (*Med. Times and Gaz.* Oct. 14, 1854) some interesting remarks on this subject in reply to the observations made by Mr. Skey in two lectures. (See *Med. Times and Gaz.* Sept. 23 and 30, 1854.) Mr. Tufnell restricts himself to noticing two questions propounded by Prof. Skey:—

The *first* point is the difference in the success which has attended the adoption of this line of treatment in Dublin and in the metropolis, and he expresses a desire "to ascertain how far this success is to be attributed to the fortunate opportunities of repetition of the operation enjoyed by the Dublin surgeons; whether, in fact, their success is not attributable to the experience obtained by repetition, or, in other words, whether failure was not a more common attendant on their early than their later cases?" To this Mr. T. answers, no; the proportion of successful cases will be found to be as great during the first years of the introduction of compression as in that just gone by.

"Now, to enable the profession to judge of the accuracy of the statement," says Mr. T., "I here make, I beg to offer to their notice the statistics of this disease as treated in Dublin by compression, from its revival in 1842, down to the present time:—"

[The table gives the particulars of 47 cases of aneurism treated by compression, in Dublin, from Oct. 1842, to Sept. 1854; 6 were treated at Richmond Hospital; 9 at Steeven's; 6 at St. Vincent's; 3 at Jervis-Street; 7 at the patients' residences; 3 at Meath Hospital; 5 at the Royal Military Infirmary; 1 at Adelaide Hospital; 3 at the City of Dublin Hospital; 2 at Mercer's Hospital; 1 at the Artillery Hospital; and in 1 the place of treatment is not stated. Out of these 47 cases, there were 36 cures by compression treatment. In two cases the patients were females—1, 22 years of age, duration of compression 20 days; the other, 40, duration of compression 9 days; the latter right brachial, and the former right femoral—both cured. Of the 45 males, 1 died of disease of the heart, duration of compression 20 days; 1 of erysipelas; 1 of disease of heart and lungs; and 1 is reported as unsuccessful. Of the males, 15 were of right popliteal aneurism; 15 of left; and in 4 not stated; 4 of right femoral; and 3 of left; 1 of right brachial; and 1 brachial, side not specified; 1 of left radial; and 1 of right ulnar traumatic. In 7 cases they were the same patients as had been previously treated.

Most of the cases are reported in the *Dublin Medical Press*, the *Dublin Journal*, the *British Association Reports*, the *Dublin Quarterly Journal*, the *Dublin Hospital Gazette*, and 4 in *Tufnell on Compression*, pp. 125, 128.

Under Dr. Hutton . . . 5	Under Mr. O'Ferrall . . . 2
“ Mr. Cusack . . . 7	“ Dr. Macdonnell . . . 1
“ Dr. Bellingham . . . 5	“ Dr. Humfrey . . . 2
“ Dr. Harrison . . . 1	“ Dr. O'Brien . . . 1
“ Dr. Kirby . . . 1	“ Mr. Smyly . . . 1
“ Mr. Porter . . . 2	“ Dr. Orr . . . 1

Under Dr. Fox . . . 1	Under Mr. Tufnel . . . 3
“ Sir P. Crampton . . 1	“ Dr. Quigley . . . 1
“ Dr. Banon . . . 1	“ Mr. Colles . . . 3
“ Dr. Clayton . . . 2	“ Mr. Fleming . . . 1
“ Dr. Read . . . 1	“ Mr. Wilmot . . . 2
“ Dr. Hargrave . . . 1	“ Dr. Jameson . . . 1

The ages of the male patients were as follows:—

11—20—24—²⁵—26—²⁷—²⁸—²⁹—³⁰—31—³²—³³—³⁴—36—37—38—40—43—44—45—46—48—55. 1 not stated.

The shortest duration of compression was one of 7 hours; the longest 93 days; the several periods were as follows:—

²2 days—4—5—6—⁷—8—9—11—12—13—²⁰—²¹—24—28—30—31—33—37—42—⁴³—53—70—72—93. And 7 hours—10—11—16—23—30—33—39.

In 8 cases the duration is not stated.

In one case of popliteal aneurism, ligature was applied, and the patient recovered; in another, of right popliteal, cured by compression, the left femoral had been previously tied for left popliteal aneurism. Interrupted pressure was applied in one case, cured. In a case of right brachial, in which ligature was applied, there was high bifurcation, and two vessels were secured. In a case of left femoral, and in two of right popliteal, amputation was performed, and recovery took place; in another of left popliteal (male—25), ligature was applied successfully; and Dr. Fleming remarks upon the case: “It was decided that the treatment by compression should be tried, though no sanguine expectation was entertained as to its success, as well from the nature of the aneurism, as from the character of the patient.” In a case of left popliteal, cure was effected by manual compression made by the patient’s own thumb upon the artery at the groin. In another case, in which ligature was applied unsuccessfully, the knee-joint was involved, the aneurism springing from the anterior aspect of the artery. In a case of common right femoral, cured by compression, the sac sprang from the femoral so immediately below Poupart’s ligament, that one point only existed where pressure was complete, so as not to admit a drop of blood into the sac. The man was well lowered by croton oil previously.]

In submitting this statement, I would beg emphatically to call attention to one fact—viz. that it contains *every single case* that has occurred in Dublin in which compression has been employed. It has not been compiled (as the statistics of ligature have) from certain published cases only; it includes every case of aneurism where a compressing instrument has been placed upon the limb; and, to show how general has been this treatment, I believe there have been three instances only during this time in which it has not been tried, two being ligatured at once, and amputation resorted to in the third. The Profession, therefore, have here brought before them the practical working of compression in the treatment of aneurism in Dublin, and can judge of the result. It will be seen that the statement embraces a large number of names, some of whom have had only a single opportunity of treating the disease, and yet success in their hands has been equally complete with those having a larger number of cases. The “experience of repetition” is not, therefore, necessary for success. It is assuredly a great advantage to have had practical experience, especially in the management of details; but, the principles of compression once established and understood, *judgment in selection, and watchful care in the management of the cases*, are alone required.

It is a mode of treatment, however, far different in its application to the ligature. With the ligature, so soon as once applied, the surgeon has nothing further to do than wait patiently for nature to perform her part, and to guard against the dangers of hemorrhage; but, in compression, as long as the sac is pervious to the influx of blood, so long must unremitting attention be given to the case; for it is ever to be borne in mind (and the fact cannot be too strictly enforced), that, carelessly or injudiciously employed, pressure is fraught with danger to the patient. While the compressing pad is on the artery, progress toward recovery may be presumed to be going on; but the instant it shifts from

the elastic instrument of Dr. Carte for six hours, although the compression was equally complete, and pulsation prevented. In this case, after the application of the instrument, the pulsation ceased, and never after returned."

In another case, Dr. Hutton remarks: "The patient sustained the compression during seven and a half successive hours, never allowing pulsation to return to the tumour. At the end of this time he removed the instrument; pulsation had wholly ceased; the aneurismal tumour became solid, and absorption soon commenced. I feel confident that this patient could not have borne the application of the screw clamp for so long a time." These are tests of the efficiency of elastic pressure which, I think, cannot be gainsaid. These are the results of its working in the treatment of disease in Dublin. An easy mode, however, of establishing its superiority exists, which any member of the profession who doubts can test upon himself. Let him place a screw clamp upon the femoral artery at Scarpa's space, or on any other portion of the thigh, so as to stop pulsation in the vessel below. Let him then try to walk across the room; he will find himself, as it were, rivetted to the spot, totally unable to move. Let him next substitute for the clamp, a circular compressor of Dr. Carte, and equally control circulation through the limb, and then try his powers of progression. Instead of inability to move, he will discover that he can proceed freely, and walk with ease, the piston of the pad rising and falling to each step as the muscles contract and extend, keeping still the artery compressed, and allowing no blood to pass through the point of arrest. "That eventually compression will become the general mode of treating aneurism of the extremities I have not a doubt;" and the concluding sentence of Mr. Skey's able lecture confirms the opinion I thus expressed in 1851. Mr. Skey says: "Notwithstanding temporary obstructions, we may look forward with the utmost certainty to the treatment of aneurism by compression as the future law of our profession." I have replied to these two points raised by Mr. Skey, because I consider them all-important—the statistics, as indicating a success in Dublin that may stimulate others to adopt this mode of treatment elsewhere; and the form of pressure most desirable in the treatment, as so mainly influencing the well-being and successful termination of each case.

39. *Tumoral Aneurism cured by Compression.*—Mr. M. H. COLLIS records (*Dublin Quarterly Journal*, Nov. 1854) an interesting case of this, in which compression was applied close to the sac, and appears to have acted on a different principle from what has hitherto been claimed for it.

"Christopher Toole, aged 32, by trade a nailer, a tall, athletic man, of dark complexion, presented himself among the extern patients at the Meath Hospital, on Monday, July 24, 1854, with a pulsating tumour on the upper part of the right thigh. This tumour was $4\frac{1}{2}$ inches long, and $5\frac{1}{2}$ wide; the circumference of the limb over the centre of the tumour was $21\frac{1}{2}$ inches, that of the corresponding part of the sound limb being $18\frac{1}{2}$. It occupied the upper part of Scarpa's space; its upper margin was in contact with the depression which marks the position of Poupart's ligament, and it extended downwards along the course of the common and superficial femoral vessel. The pulsation was very strong and diastolic, and the action of the artery above it was so forcible as to lead some persons to conclude that it was considerably dilated; by pressing upon the abdomen firmly we could trace the pulsation in the external iliac for a distance of nearly four inches. The superficial veins of the thigh were remarkably large, knotted, and tortuous; the glands in the groin were somewhat swollen; and upon the outer aspect of the tumour two glands could be felt elongated and flattened, and partially sunk into it; the entire limb was enlarged, slightly œdematous, but of natural temperature and sensibility. There appeared to be some solidification of the tumour on its outer margin; subsequent observation showed this to be at least partially deceptive, and resulting from the varicose condition of the veins on the internal side. Pressure upon the artery in the groin readily checked all pulsation, but without any sensible effect upon the volume of the tumour. No bruit de soufflet was audible in any position, nor was there any fremissement; the whole arterial system was excited, and the man complained greatly of the throbbing; even the momentary

pressure of the thumb upon the artery gave him great relief. He stated that his general health was good, and that for the last two years he had been temperate; his occupation rendered stooping necessary, and also constant rotation of the body at the hips, the feet being kept unmoved. The floor of his workshop was uneven, and his right foot had for some weeks rested habitually on a lower level than the left. The tumour made its appearance, as far as he was aware, spontaneously, about six or seven weeks before his application at hospital; it was then about the size of a nut, and had ever since grown steadily larger.

Tuesday, 25th.—After administering purgatives, &c., compression was attempted; various instruments were tried, by none of which could the artery be commanded, even partially, for more than a minute or two. The only point available for pressure was just above the sac, at the spot where the epigastric and circumflex vessels are given off. As the brim of the pelvis is rounded just at this place, all the instruments were displaced by the ordinary respiratory movements, and slipped off the artery; a relay of pupils kept up pressure sometimes by the finger and sometimes with the clamp or leaden weight; but it was partial and irregular, in spite of the greatest vigilance and anxiety on the part of all, including the patient himself. Low diet was strictly enforced, and purgatives of croton oil were occasionally exhibited. Quiet was procured by opiates. Chloroform was administered once, but produced excessive headache. An attempt was made to draw some blood upon the second day, but it was thick and carbonaceous, and would not flow. On Saturday, after upwards of four days' ineffectual attempts at compression, the skin in the groin began to chafe and suppurate round the root of every hair, and it became evident that pressure could not be borne much longer; the aneurism had increased half an inch outwards and downwards, and the pulsation was as violent as before, compared with the general force of the circulation; no attempt at consolidation had taken place, and all parties were inclined to be disheartened.

On Sunday, the 30th, I determined to make another effort to command the vessel completely. Having procured a double clamp tourniquet, which closed by a screw at the hinge, I applied it, placing a small roll of bandage an inch thick under the pad. I screwed it home, and found that it controlled the pulsation for a moment, and, like all the rest, slipped off towards the abdomen. By screwing it a little tighter, and drawing it downwards by a tape from the knee, I was enabled at last to get the artery completely controlled. Before finally adjusting it in this position, I drew up the skin, so as to get an unchafed part under the pad. The tourniquet was not disturbed for thirty hours, and at the end of this time, upon slackening it, all pulsation was found to have ceased in the tumour and in the artery as high as it had been traced before, namely, to the spot where the common iliac bifurcates.

About twelve hours after the tourniquet was applied, the man felt a stinging pain on the inside of the knee and leg, and a kind of tingling sensation along the outside of the hip and thigh. While under pressure, he took a couple of sedative draughts, containing half a drachm of chloroform.

The spot of skin where pressure had been first attempted was beginning to slough in points, but in a few days it became healthy. The tumour became a little smaller in the first few days, although the subsidence of venous congestion and oedema gave it a more prominent appearance. He never had the smallest pain nor single bad symptom after the removal of the clamp, and upon the 12th of August he got up and walked about the ward.

On the 16th he left the hospital, and returned almost immediately to his work, having been in hospital twenty-three days; nor would it have been necessary to keep him so long but for the slough of the integument caused by the first abortive attempt at compression. I saw him on the 6th of October, and found the tumour reduced very considerably in volume; the difference between the circumference of the two limbs was less than one inch.

In the mere details of this case, there is little worthy of remark beyond the rapidity and painlessness of the cure, and the fact that it is the first instance on record of the cure of aneurism of the common femoral artery by compres-

sion. There are, however, some points of general interest to which I wish to draw attention. These are:—

First. The position of the point of pressure as regards the sac and as regards the collateral branches. The artery was compressed close to the sac; the point of pressure covered the usual origin of the superficial circumflex ilii and epigastric vessels, the latter of which could be felt pulsating before treatment was commenced; hence the nearest collaterals were practically the internal iliac on the one side, and the profunda on the other, and pressure was thus made between the sac and one of the next collateral branches.

Secondly. The fact that under these circumstances the aneurism increased in size, as long as pressure was incomplete, and upon the artery being completely controlled coagulation was rapidly obtained.

Thirdly. The simultaneous blocking up of the artery above the point of pressure, as high as the next collateral branch.

Fourthly. The non-occurrence of secondary aneurism, or of suppuration of the sac.

Fifthly. The bearing of this case upon the question of pressure at the distal side.

The immediate proximity of the point of pressure to the sac appeared to be a dangerous though unavoidable complication; it was, however, not only unavoidable, but essential to the cure. As but one point existed where pressure could be made, it was necessary that the cure should be rapid, or the skin would have failed us. Ignorant of the principle which appears to guide the cure in pressure close to the sac, we at first were satisfied with endeavouring to cut off the impulse of the heart without completely shutting off the current. As long as we did so the sac continued to increase in size, and to pulsate with as great a vehemence as ever. No sooner was the current completely intercepted from the sac by pressure between it and the nearest collateral, than coagulation of its contents took place. No current through the sac was now practicable, and the consolidation was, as we must suppose, not only rapid but complete. In compression at a distance from the sac, as in ligature similarly situated, blood is deposited layer by layer, until not only the sac but the artery at the injured part is filled up. No such process could have occurred in this case, for no branch existed between the sac and the point compressed, and therefore no current could have made its way through it. The effect of thus converting it into a *cul de sac* was the coagulation *en masse* of its contents. The same result took place in the *cul de sac* above the tourniquet, the artery becoming obliterated in a similar rapid manner. To this latter circumstance is probably to be attributed the non-occurrence of relapse, or of suppuration in the sac.

This case seems to suggest the possibility of curing aneurism by distal pressure, under favourable circumstances. If we can effect the consolidation of an aneurism by converting it into a *cul de sac*, there is reason to believe that with due regard to preparatory treatment this result might in some cases be obtained by choosing a spot between the sac and the distal collateral branch as the seat of pressure. We see in this case that not only the sac was filled up, but the artery also, which was exposed to the pulsatile wave of blood. When pressure is made on the distal side of an aneurism, we observe an increase of impulse for a few moments. If the compression is *complete, and firmly kept up*, this momentary irritation subsides, and the aneurism pulsates with perhaps less force than before. This principle of converting the aneurism into a *cul de sac* appears to act occasionally in compression at a distance. Dr. Carte, who has had great opportunities of observing the various phenomena which arise during the cure of aneurism, has stated it to me as his conviction that many, if not all, of the rapid cures are effected by a loose clot blocking up the distal portion of the artery. Certainly we can scarcely conceive it possible that a cure which occupies seven, ten, or even thirty or forty hours, could be effected by the slow process of a deposit, layer by layer, until the sac is filled. We see, also, that a similar result has been obtained purposely by manipulation of the sac, a process which is too dangerous to be other than exceptional in application. From all these circumstances, therefore, I think we may infer the pro-

bability of distal pressure being occasionally useful; it is at least worth a trial, where the position of the sac prevents our using the ordinary method; and if we can get a point for compression between the sac and the nearest collateral branch, I think the result of the cases which I have now put upon record will give us encouragement, although indirect, to hope for success."

40. *Popliteal Aneurism, cured by Compression in Twelve Days.*—The subject of this case was a coachman, 35 years of age, of thin person, middle stature, rather pale, tranquil, and somewhat feeble circulation, and good general health; admitted under Mr. T. P. Teale into the Leeds Infirmary Sept. 10, 1854. The aneurism resulted from an injury four months previously. Two months ago, a small pulsating tumour was felt in the ham, which has been steadily increasing. It is now the size of a hen's egg. The circumference of the affected knee is 15 inches, that of the sound knee $13\frac{3}{4}$ inches.

Sept. 12.—Mr. Teale ordered compression to be made in the course of the femoral artery by means of Signorini's tourniquet; the pressure to be discontinued at nights, and to be occasionally intermitted for short periods during the day, and to be practised to such a degree as greatly to mitigate the force of the circulation, but not absolutely to arrest pulsation in the aneurism; the seat of pressure to be frequently varied in the course of the artery. The patient to have meat daily, without beer, and to be kept in bed.

15th. Pulsation much diminished, and the tumour smaller. He feels much easier.

19th. Tumour smaller and firmer; circumference of knee, $14\frac{1}{2}$ inches.

24th. Tumour much smaller, feeling perfectly solid. A very slight pulsation may be felt in the course of the tumour, but it is doubtful whether this pulsation is in the aneurism, or in a small artery passing over its surface. For greater security in this state of doubt, the pressure is directed to be continued a few days longer.

26th. It is now evident that the slight pulsation felt on the 24th was due to an arterial branch in process of enlargement, and not to the aneurism, which may, therefore, be considered as cured on the 24th, or twelve days after the commencement of the treatment. The pressure is now to be discontinued.

The following table, accurately recorded by the patient, shows the duration of the pressure each day:—

Sept. 12	.	.	.	9	hours.	Sept. 20	.	.	.	13	hours.
" 13	.	.	.	12	"	" 21	.	.	.	13	"
" 14	.	.	.	13	"	" 22	.	.	.	13	"
" 15	.	.	.	12	"	" 23	.	.	.	13	"
" 16	.	.	.	$12\frac{1}{2}$	"	" 24	.	.	.	13	"
" 17	.	.	.	$12\frac{1}{2}$	"	" 25	.	.	.	13	"
" 18	.	.	.	13	"	" 26	.	.	.	$6\frac{1}{2}$	"
" 19	.	.	.	13	"						

Med. Times and Gaz. Oct. 28, 1854.

41. *Operations in Cases of Hernia when the Sac and its Contents had been reduced en masse.*—Mr. PAGET has recorded (*Med. Times and Gaz.*) two cases of inguinal hernia, in which the sac and intestine having been reduced *en masse* by the taxis, the symptoms of strangulation continuing, he laid open the inguinal canal and succeeded in reaching the hernial tumour, and relieving the constriction at the mouth of the sac; in both cases with successful results.

The best collective evidences for the reduction *en masse* appears to be—1st. The signs of strangulation continuing, or first ensuing, after the apparent reduction of a hernia; 2d. The loss of a hernial sac, more of which, according to the history of the case, ought to be felt in or beyond the inguinal canal; 3d. A feeling of fulness at the internal ring, and of an ill-defined, firm swelling in the abdomen behind or below the ring; 4th. An amount of pain at and about the internal ring, which is quite disproportionate to that in any other part of the abdomen, as well as to the other signs of strangulated hernia; 5th. The unusual drawing up of the testicle. Mr. Paget then observes:—

In these particulars, the cases I have related corroborate those already published, and confirm the rule that, when these signs are found coincident, we should act in the belief that the reduction *en masse* has taken place. But the cases presented two rare and deceptive features.

1. In each of them, the pushing back of the sac was so far incomplete, that the lower portion of it, containing fluid, remained in, and in one case projected a little beyond the inguinal canal. Feeling this portion of the sac, one might have assumed that no more had ever projected beyond the abdomen. But the cases show that we must count, among the indications of this accident, not only the complete disappearance of a hernial sac, but the marked diminution of one; that it may excite a suspicion, if we can feel, in and beyond the inguinal canal, only a much smaller hernial sac than, according to the history of the case, there should be. If, in either of these two cases, the hernia had been properly reduced, its empty sac should have been felt, not only in the inguinal canal, but along the descending portion of the spermatic cord.

2. In the second case, the sac had been pushed from its connections five or six weeks before the severe strangulation ensued, and, once in the course of that time, intestine had probably been strangulated in, and returned from, the displaced sac. Such an event has not, I believe, been previously recorded; but a single occurrence of it diminishes the confidence with which we might expect that, in all these cases, there would be the history of a very recent reduction of a hernia.

The incompleteness of the displacement of the sac, to which I just now referred, suggests a rule in operating. If, in the suspicion that a hernia has been reduced *en masse*, the inguinal canal be laid open, and no sac at all be found in it, the probability of an erroneous reduction is increased; but if, instead of no sac, one be found containing fluid, we might be apt to conclude that this is the whole sac, and that the intestine has been duly reduced from it. But the cases show that the part of the sac containing and strangulating intestine may be pushed back from the internal ring, and that another part, containing fluid, may remain in the inguinal canal: they thus establish, as a rule, that, in all such suspicious cases, we must not desist in the operation without being perfectly satisfied that all is right in the abdominal cavity, so far as it can be reached from the inguinal canal. In both the cases here related, after the exposure of part of the sac in the canal, the detection of its displaced but still constricting mouth was impossible, except by such an examination as would have been unjustifiable in an ordinary case. The sac's mouth was only just within reach of the forefinger, and the sensation received by the outstretched finger touching the intestine in the sac was at first deceptively like to that from intestine in the abdominal cavity.

I would add a remark on the after-treatment of strangulated hernia as illustrated by these cases.

The abstraction of blood by nearly a hundred leeches, in the three days following the operation, in the first case, was intended against what appeared to be an active local inflammation of the peritoneum and adjacent tissues, in consequence of the violence used in the displacement of the sac and in the operation. The giving of wine on the third day after the operation in the second case was dictated by the fear that a peritonitis would presently ensue, against which treatment might be of little avail.

The effects of both these opposite treatments appeared to be good, and they indicated, as many other facts do, that there are at least two forms in which peritonitis ensues after operations for hernia. In one class of cases it appears essentially the local consequence of injury; in the other, it is also, and rather, the local indication of a general blood-disease. In the one, it is an active local disease, amenable to local treatment; in the other, it is only one part, one feature, of a constitutional disease of low type, which local treatment can scarcely affect for good.

We may probably compare the cases of the two classes with those of phlebitis after amputation; of which, some are simple local inflammations of injured veins; others are inflammations of veins, associated with diseases of many other parts, and declaratory of diseased blood. The parallel would hold in

many respects, and, among them, in this, that the peritonitis or phlebitis of local origin and nature ensues very early after the operation, or is continuous (in the case of peritonitis) with that which preceded the operation; while the disease of constitutional origin, whether peritonitis or phlebitis, comes on later, and with an interval of two, three, or many more days, in which no signs of it may be detected. There are similar differences and correspondences in the earlier and the later erysipelas after operations; the former is mainly a local disease, the latter is the localized indication of a general disease; and, as in the corresponding cases of peritonitis and phlebitis, the one is a comparatively trivial, the other a very grave disorder.

Now, the respective indications of the earlier and the later occurrence of phlebitis and of erysipelas after operations, are, I believe, recognized by many; and the time that has elapsed between the operation and the access of the disease is often reckoned among the facts determining the plan of treatment. A similar rule should be held concerning operations for hernia. In general, if peritonitis ensue within twenty-four hours after the operation; or, if it existed before the operation, and continue or is increased after it, it may be assumed to be chiefly a local disease, a simple inflammation of injured parts, which may be treated by local or other bleeding, by abstinence from food and stimulus, and perfect rest; or which, if it be slight, may be left to spontaneous recovery. If, on the other hand, the peritonitis first manifests itself more than twenty-four hours after the operation, it is probably an indication of a general blood disease of some low type, for which opium and well-regulated food, or even stimulants, afford the best hope of remedy.

I need hardly say, that this difference in the time of first appearance must not be taken as alone sufficient for the diagnosis of the two forms of peritonitis, or for guidance to practice. I believe only that it may be reckoned among the most useful indications. Neither may twenty-four hours after the operation be fixed as an exact time by which, according to their advent within or beyond it, we might class all the cases of peritonitis; it is, I believe, a fair general estimate, but nothing more. Nor must the one form of the disease be regarded as exclusively a local, or the other exclusively a constitutional malady; each is only chiefly the one or the other; and, in different cases, we may find the constitutional and the local elements of disease prevailing in various proportions.—*Monthly Journ. Med. Sci.* Aug. 1854.

42. *Radical Cure of Hernia by Iodine Injections.*—M. JOBERT has presented to the Academy an account of three cases of inguinal hernia which were radically cured by injection of the tincture of iodine into the hernial sac.

The first case was that of a young man, aged 18, who was affected with a complete inguinal hernia of the left side. It did not, however, descend to the bottom of the scrotum, and below was separated from the testicle by an elongated constriction. The bowel was readily reducible by the taxis, but immediately the patient coughed or stood on his feet it was again extruded. The patient stated that his disorder in no way interfered with his ordinary occupations, but he was extremely desirous to be completely cured of it, as he was judged unfit for military service in consequence of its existence.

In compliance with the patient's wishes, M. Jobert determined to accomplish, if possible, a radical cure, by injecting the tincture of iodine. On the 12th of May, therefore, having made an incision over the inguinal canal, he introduced a fine trocar, and injected through it five drachms of pure tincture of iodine. The patient suffered some little pain in consequence of the proceeding, and, upon its completion, the wound was united by the twisted suture, and dressed with simple ointment. In the evening, there was slight swelling in the inguinal region, but no constitutional disturbance.

On the 14th, the swelling and redness were considerable, and, on removing the sutures, the wound was found imperfectly united. After some days, however, the redness and swelling disappeared, and cicatrization was accomplished. On the 5th June, the patient left his bed, walked about, and coughed without causing the least unnatural impulse of the abdominal viscera, or the slightest reappearance of an external tumour. The left testicle (that on the affected

side) remained of the same size as the right. He was directed to wear a suspensory bandage for some time, as a precautionary measure.

In the second case, the patient, aged 33, was admitted into Hôtel Dieu, on the 18th of November, 1853, with a hydrocele and a congenital inguinal hernia of the same side, and the two swellings being separated from each other by a kind of hour-glass contraction of the tunica vaginalis, the upper part of the sac being occupied with intestine, and the lower with serous fluid and the testicle. M. Jobert resolved, if possible, to obliterate the tunica vaginalis, and so to cure, at the same time, both the hernia and the hydrocele. With this view, having first interrupted the communication between the tunica vaginalis and the peritoneum, by means of pressure applied to the inguinal canal, he passed a trocar into the lower portion of the constricted sac, and, after having let out the contents of the hydrocele, injected a small quantity of the pure tincture of iodine. On the following day, the scrotum was red and slightly tender, and the affected tunica vaginalis distended with effusion of serum. The patient, however, manifested no constitutional disturbance, and complained of no pain. Day by day the swelling and redness diminished, and, within seventeen days after the operation, the scrotum had regained its ordinary dimensions; but, on the affected side, both it and the spermatic cord were firmer than natural. A cylindrical cord existed through the whole of the inguinal canal, and extended along the course of the spermatic vessels to the testicle. A complete cure was obtained, and the patient left the hospital, able to walk without fatigue, and exhibiting no reappearance of the hernia, in whatever position he occupied. No serious local inflammation, nor any constitutional disorder, resulted from the injection.

The third case was that of a congenital hernia of the left side, quite reducible, and equal in size to a large pear. The patient was twenty-seven years of age, and had been ruptured eight years. He had worn various bandages, in order to counteract his infirmity, only one of which had at all answered the purpose. As the patient ardently desired to be cured of his disorder, M. Jobert, in the presence of his colleagues, punctured the hernial sac, and injected into it a small quantity of the pure tincture of iodine. The patient was then placed on his back, with his legs flexed slightly on the thighs, in which position they were maintained by a bolster. Almost immediately after the injection, the hernial sac began to swell, and in fifteen hours the sac had become as large as if filled with intestine, and communicated a fluctuating half-solid character to the finger. The parts remained in this condition for eight days, after which the swelling rapidly diminished. Twenty-eight days after the operation, the track of the spermatic cord was occupied by a cylindrical substance so compact and hard that the patient was able to rise from his bed, to walk about, and to sit down, without the smallest appearance of an intestinal protrusion exhibiting itself.

The cure remained complete four months after the operation, the testicle had not atrophied, and the inguinal canal was occupied by a firm, solid cord, into which the vaginal process has become converted by its obliteration.

M. Jobert does not allow the tincture to remain permanently in the sac, but withdraws it by means of the syringe. In all cases of congenital hernia, or whenever the sac is distended with fluid, or is thickened and condensed, and has become adherent to the surrounding structures, M. Jobert penetrates it at once with a trocar, without first dividing the skin with a scalpel; but, whenever the sac is thin, movable, and easily displaced by pressure, he prefers to divide the skin with a scalpel, and expose the sac before puncturing it.

The superiority which the method by injection offers to all other proceedings which have been devised for the radical cure of hernia, depends, says M. Jobert, on its harmlessness and simplicity; and M. Velpeau deserves the credit of having, in his *Annales de Chirurgie*, published ten years ago, first noticed the advantages of iodine injections for effecting the radical cure of hernia.—*Gazette des Hôpitaux*.

43. *Efficiency of Ice combined with Compression in the Reduction of Hernia.*—In sixteen cases of strangulated hernia, in which all the ordinary means of

reduction had been unsuccessfully employed, M. Baudens has effected the return of the bowel by the application of ice associated with permanent local pressure. The degree of refrigeration is to be regulated in proportion to the amount of inflammation in the hernial tumour, and the sensations of the patient. M. Baudens commences by the application of a simple compress, which is soaked from time to time in water, whose coldness is gradually increased. Fragments of ice are afterwards placed on the compress, and the cold thus produced may be augmented, if necessary, to a very considerable intensity by the addition of increasing quantities of common salt. Sometimes the refrigeration alone is sufficient to effect the reduction, as M. Baudens has witnessed on three occasions; but when this does not take place, an elastic bandage is applied over the ice, by means of which a continual steady pressure is exercised on the tumour. Whenever the rupture is so painful that no pressure can be tolerated, ice alone must be employed at the outset, and the elastic bandage may be added as soon as the sensibility of the swelling is sufficiently diminished. In order to facilitate the action of these measures, the pelvis should be kept in an elevated position.

M. Baudens ascribes the efficacy of this treatment to the powerful influence of cold in diminishing the size of the hernial tumour, by overcoming the capillary congestion, and extinguishing the inflammation in the strangulated bowel. Hitherto, says he, the treatment by cold has been condemned, because its therapeutical effects have been neglected or misappreciated. He regards the idea that the application of cold to a strangulated rupture is likely to produce mortification in the bowel as a groundless apprehension, believing that so large an amount of heat is developed in a part during the continuance of inflammation, as to enable it to resist with facility long and powerful refrigeration, without at all endangering its vitality. He admits, however, that it is quite possible to continue the application of cold too long, but maintains that the feelings of the patient furnish the best index as to the advantageous or prejudicial operation of the remedy. As long as the patient feels that the action of the refrigerant is grateful, and productive of comfort, its application should be continued; but, as soon as he begins to experience a disagreeable sensation of coldness and moisture, it should be immediately removed.—*Med. Times and Gaz.* Sept. 30, 1854, from *Gazette des Hôpitaux*, June 3, 1854.

44. *Arrest of Hemorrhage from a Burst Varix.*—At the bedside of a patient now under his care, in the London Hospital, on account of a ruptured varix in the leg, Mr. ADAMS took occasion, a few days ago, to point the attention of his class to the practice which should always be resorted to in such accidents. The bleeding was, he remarked, from the *proximal* end of the vein. There often existed either none, or very imperfect valves between the injured spot and the heart. In such a state, the only propelling force would be the mechanical pressure of the column of blood above the laceration, the effect of which might be in a moment reversed, by the simple measure of *elevating the leg*. On referring to several surgical works, all of which mention cases of death from hemorrhage under these circumstances, and correctly explain its cause, we do not find any allusion to the method of arresting it recommended by Mr. Adams, and have, therefore, esteemed it worthy of mention here. None who have seen cases of burst varix will ever forget the fearful way in which they sometimes bleed, and most will doubtless have encountered some difficulty in securing arrest by pressure, as is usually attempted.—*Med. Times and Gaz.* Sept. 9, 1854.

45. *Cancrum Oris treated by the Application of Nitric Acid.*—A case of cancrum oris has just occurred in St. Bartholomew's Hospital, under the care of Dr. Baly, which has well illustrated the usefulness of the application of the strong nitric acid. The patient was a boy, in whose left cheek the phagedenic ulceration commenced during recovery after scarlet fever. The internal use of chlorate of potass was first tried, and persisted with, in ten-grain doses, for several days, the disease, meanwhile, being unchecked. A single free application of the concentrated acid was then made to the part, and with the effect of completely arresting the morbid action. The induration of the surrounding

part has since gradually subsided, and the sore is now almost healed. The case, although not one of the most acute class, was yet of a character sufficiently alarming.—*Ibid.*

46. *Nitric Acid in Phagedæna.*—For a considerable period past, a mild form of hospital phagedæna has been prevalent in the various London institutions. In very rare cases have fatal results ensued, and in the majority, beyond the local condition, no urgent symptoms have developed themselves. It has generally attacked superficial wounds, whether those resulting from accidents or operations, and, in a few instances, even stumps have been affected with it. The treatment which has certainly been found of most general benefit has been the application of pure nitric acid. On board the Dreadnought hospital ship, where it has prevailed severely, the acid has, we believe, been found most successful. In the London Hospital, Mr. Ward informs us, that he has carefully watched upwards of twenty cases, and in no single one of these has the gangrene spread after the application of the escharotic. In St. Mary's Hospital, some remarkable cures have also been effected by its aid. In a case under the care of Mr. Coulson, a man, in good health, admitted on account of a laceration on the dorsum of the right foot, had the wound so severely affected by phagedæna that amputation was seriously proposed. The nitric acid was freely applied, by soaking a piece of lint, and then pressing it into the sore; it was allowed to remain seven hours, and completely achieved its intention. The gangrene ceased to spread, the sore took on healthy action, and rapidly cicatrized. Neither in this nor in any one of several other cases in which the dorsum of the foot was the affected part did any sloughing of the tendons follow the use of the acid. It appears to be in cases of simple phagedæna that the local use of nitric acid is most to be depended upon, since those of syphilitic origin not unfrequently resist its influence. A case of the latter character, occurring in a young and seemingly healthy woman, was lately under the care of Mr. Stanley, in St. Bartholomew's Hospital, and, in spite of all measures, ultimately terminated in death. Nitric acid had more than once been freely applied, and opium, cinchona, chlorate of potash, &c. had been fairly tried. We mentioned briefly, at page 264 of this Journal for Sept. 9, 1854, a case of cancerum oris, under the care of Dr. Baly, in which the acid arrested the phagedænic action after the internal use of large doses of chlorate of potash had quite failed to do so. In the Middlesex Hospital, the favourite treatment for phagedæna is by opium pushed freely until its effects become apparent. Some very successful cases have been cured by this plan. It is observed, that there seems scarcely any limit to the quantity which may be borne, and that, generally, before any narcotic effects become apparent, the sore takes on a healthy action. Now and then the acid succeeds admirably in syphilitic cases; as a rule, however, they seem more amenable to constitutional than local measures. On this account, we but seldom see a simple trial either of the acid or any other one remedy alone. The following case, in which the only change of treatment to which the result could be referred was the application of the acid, is worthy of mention. A prostitute, aged 26, was admitted on Feb. 1, 1853, into St. Thomas's Hospital, under the care of Mr. Le Gros Clarke. She was suffering from a large and deep phagedænic sore on the left labium. The treatment consisted in the use of wine in liberal quantities, and of a draught containing full doses of ammonia and bark. A morphia pill was given each night, and the nitric acid lotion was applied to the sore. This plan was persisted with for three days, during which the sore slowly spread. On the fourth day, the phagedænic action had much increased, and it was determined no longer to delay the use of the strong acid. The woman was very ill, feeble, and without any appetite. By means of a piece of lint, the fuming nitric acid was freely applied to the whole sore. Only a few minutes' pain was complained of. On the following day, the sore presented an almost clean surface, and it afterwards rapidly cicatrized. Exactly the same general treatment had been persisted with throughout. This case is an example of what is very often observed, viz. that the pain produced is but very transitory, and much less severe than it is commonly esteemed.

The most convenient mode of applying nitric acid is by means of a glass brush; in the absence of which, however, a glass rod succeeds very well. To the use of wood or of lint there is at least a theoretic objection, on account of their permitting the charring effects of the fluid.—*Med. Times and Gaz.* October 28, 1854.

47. *Absorption Treatment of Bubo.*—Some cases of suppurated bubo, which have recently been treated by Mr. HILTON, in Guy's Hospital, have well illustrated the efficiency of counter-irritation, as a means of procuring the absorption of pus. The application which Mr. Hilton employs is a solution of nitrate of silver of the strength of one drachm to two of water, with the addition of about three drops of the strong nitric acid. This is painted freely into the skin over the inflamed gland. Its effect is to produce great soreness of the skin and much pain, which is usually followed by rapid diminution in the size of the enlargement. Mr. Hilton informs us, that he has long used the remedy, and now considers the cases of bubo, which require opening, as quite exceptions to the rule. In several, in order to afford proof that the fluid absorbed was really pus, it was examined by a grooved needle, prior to the treatment, the puncture being at once reclosed. Even in those instances in which ultimately it becomes necessary to make an external opening, great benefit in the limitation of the suppurative action, and the absorption of the surrounding hardness, results from the counter-irritant treatment. The plan is applicable to many other diseases besides glandular abscesses; in fact, to almost all in which the absorption of recent inflammatory deposit, either purulent or fibrinous, is desired. We reported a case in the *Medical Times and Gazette* for Dec. 17, 1853, p. 629, in which it had been effectually used to remove an induration of tissues around the femoral artery, which had so obstructed the vessel as to cause a tubular dilatation. Superficial abscess in the cellular tissue may also be so treated. Chronic non-suppurated glandular enlargements and swellings of one or other lobes of the thyroid gland may also be made to diminish by it. It is probable that the nitrate of silver has no particular virtues beyond those of a severe counter-irritant, and might, were it wished, be substituted by other remedies; as, for instance, a caustic solution of iodine. It is, however, a very convenient, manageable, and efficient means, which there is no motive for wishing changed. The substitution of nitric ether as a menstruum, instead of water, would be a great improvement. (See *Medical Times and Gazette* for Oct. 14, 1854, p. 394.)—*Med. Times and Gaz.* Oct. 28, 1854.

48. *Treatment of Gonorrhœa.* By WM. ACTON, Esq.—It is to be regretted, but still the conscientious surgeon is obliged to admit the fact, that notwithstanding all the improvements that have taken place in the treatment of diseases of the urinary and generative system during the last few years, little progress has been made in the cure of gonorrhœa. It is true that we have in this the nineteenth century a better knowledge than we before possessed of the pathology of the disease, and we have expunged many a vulgar error which was entertained about the complaint; but as to the discovery of a certain rapid cure, we are as far distant from it as ever, and perhaps it is only with a reputation such as Ricord's that a teacher dare make the acknowledgment that, after performing various experiments during a long course of years, no certain plan of cure has been discovered. This, however, is not so surprising to the old practitioner as it is to the tyro, who does not consider the situation the discharge comes from, the frequency with which the disease occurs, the liability of relapse, together with the little attention a patient will pay to the complaint; but, above all, that the urine, loaded with all sorts of stimulating substances, must necessarily pass over the inflamed surfaces many times in the twenty-four hours; and it would almost appear as if gonorrhœa would ever continue to be the *opprobrium medicorum* and the pest of the patient, notwithstanding all the science bestowed upon it. In saying this, however, I would not be understood to mean that gonorrhœa should last an indefinite length of time; far from it, for in 19-20ths of the cases the affection is to be cured readily and easily in persons who will take ordinary precautions; but every now and then cases occur which linger on many

weeks, and yield at last to change of air or abstinence from all treatment, the disease apparently wearing itself out.

During the last few years, M. Ricord has been gradually relinquishing the employment of caustic injections in the treatment of this complaint. He still thinks that in many cases this is the treatment attended with the most certain success; but every now and then instances occur in which the pain is very severe, the inflammation runs high, and then, instead of a cure being obtained, the disease relapses into a chronic state, which resists all our means of cure; in addition to this, the patient requires a great deal of watching, more than a surgeon in full practice is able to devote to each individual; so that, as I mentioned above, Ricord now employs caustic injections very rarely, and prefers prescribing capsules, and the following injection: Sulphate of zinc, acetate of lead, of each fifteen grains; tincture of catechu, one drachm; Sydenham's laudanum, one drachm; rose water, six ounces and a half. My own practice in London fully bears out this experience, and it is only in old rebellious cases of gleet, in spermatorrhœa, and in chronic affections of the bladder, that I now employ nitrate of silver injections; but in these cases the judicious employment of caustic is of the greatest value, and will at once effect a cure when all other means fail; still, even in these cases, the remedy must be employed with care, otherwise ill consequences will arise.

In the subacute stages of gonorrhœa, when there is no scalding, but a good deal of discharge, I have found the solution of lead answer best in London, together with copaiba capsules, not taken in large numbers, but at repeated intervals, so as to charge the urine constantly with the essential oil; but I still (as in the last edition of my book) continue to insist that gonorrhœa will rarely be rapidly cured, unless the surgeon takes the precaution to show a patient how to use his syringe, and see that the instrument be properly made. Having insisted elsewhere in great length on these points, I do not think it necessary here to revert to them; but to these petty matters I attribute much of my success, and not to any specific treatment. It is, however, worthy of note, that the time of year and the damp weather have a great influence in retarding a cure.—*Lancet*, Oct. 14, 1854.

49. *Carpet Tack in the Trachea for Nineteen Days; Expelled by Nature.*—JAMES CRAWFORD, M. D., Professor in McGill College, reports (*The Medical Chronicle*, Oct. 1854) the following interesting case:—

M. G., a girl of ten years of age, while sitting beside her mother, who was occupied in quilting, took one of the tacks her mother was using, and put it into her mouth, and upon a sudden inspiration the tack disappeared into the trachea. After the alarm was over, the child complained of a pain or prickling sensation in the throat, accompanied by a short frequent cough, occurring in paroxysms. The parents, in a great measure, overlooked the accident for ten days, when they brought her to me. At that time there was no very marked symptom; the cough was trifling and short, without any expectoration. There was a slight mucous rale in the trachea, and some slight pain or soreness about the region of the glottis, but probably from fear; the child did not desire to make any complaint, or allow any examination of the throat. The lungs did not afford any indication of an abnormal condition, and the voice remained unaffected. Failing in detecting any foreign body, I fancied the painful sensation might arise from the scratching of the nail, in passing. I sent the child to my friend Dr. Campbell, to make a further examination, as I knew he had a probang of a new construction. The party, however, returned back to me, saying that nothing could be discovered. I then ordered an emetic, and directed that the child should be brought to me in a few days. As she did not, however, appear to suffer, and she went about and amused herself as usual, the parents did not pay much attention to her. On the morning of the 19th day, she coughed up the tack, and took it to her father, who brought it to show me. It was an ordinary cut tack, of three-fourths of an inch long, very sharp pointed, and somewhat rusted. From that time the cough and irritation ceased.

50. *Expulsion of a Coin from the Larynx by an act of Coughing.*—Dr. T. G. GEOGHEGAN relates (*Dublin Medical Press*, Oct. 11, 1854) the following case of this description, and offers some observations on the proper course to be pursued in such accidents, which are worthy of attention.

A healthy lad of 17, admitted into the City of Dublin Hospital, stated that during a quarrel with one of his fellow-servants, having at the time in his mouth a sixpence and a fourpence, he spat out the former coin, while the latter disappeared. He was immediately seized with cough, of such violence as to produce epistaxis, and great difficulty of breathing; both of which, however, speedily subsided. He was seen two hours after the accident, and was then aware that the coin had entered the windpipe. While at rest, he suffered no inconvenience, and the breathing was quite unaffected. When desired to cough while in the erect or sitting position, a peculiar valvular or obscure rattling sound was distinctly audible in the larynx. The stethoscope placed on the organ did not render this at all more perceptible, nor could it be elicited (save on one occasion) while the patient was in the horizontal posture. It somewhat resembled that which attends an unsuccessful effort to expel a mass of tough mucus. Respiration was of equal intensity under both clavicles. During the night, he had a few fits of coughing. On the next day, the symptoms were in all respects the same.

In considering the mode in which the case should be dealt with, and reflecting on the uncertain issue of the natural expulsive efforts, the possibility of their leading to sudden suffocation of the patient, by impaction of the coin in the glottis, or the risk of the body being transferred to the bronchus, it appeared to me inexpedient to temporize.

The physical signs seemed to indicate pretty clearly that the coin was seated in the larynx.¹

I proposed, therefore, to make a suitable opening in the crico-thyroid space, and, should the foreign body be readily seized, to extract it by the forceps. In the event of this manoeuvre not proving successful, the expedient of inversion of the body, aided, if necessary, by percussion, was to be then resorted to. By this mode of proceeding, I had hoped at once to gain more direct access to the offending substance, and to save the patient the more dangerous operation of tracheotomy. In order to give full effect to this method, it would be necessary, before opening the crico-thyroid ligament, to effectually restrain any hemorrhage arising from the laryngeal branch of the superior thyroid artery, which, though small, often bleeds rather freely, and also to have the patient carefully maintained, as far as possible, in the horizontal posture, during the employment of the forceps, and subsequently, until inversion had been practised, if necessary.

Although the course above proposed received the assent of my colleagues and other surgical friends, it by no means satisfied the patient, who, notwithstanding suitable explanations, withdrew from the hospital disconcerted. He returned to his employment, and on the morning of the third day, fifty hours after the accident, while stooping in the act of washing a carriage, he was seized

¹ Morbid growths in the larynx, when pendulous, give rise, in a modified degree, to the symptoms of foreign body. An interesting example lately presented itself in an hospital patient of mine, who died of another disease. He had been subject for some years to attacks of irritative cough; uneasiness referred to the situation of the thyroid cartilage, and, latterly, almost complete aphonia. I found, on dissection, a soft, lobulated growth of rather considerable size, translucent, of a light gray colour, and strikingly similar to nasal polypus. It was loosely attached by a narrow pedicle to the middle of the lower chorda vocalis of the right side. A similar, but smaller and sessile, tumour was observed on the left upper one. I believe that growths of this nature are rather rare. If the disease admitted of an accurate diagnosis, it might possibly, in some cases, fall within the range of operative surgery. When the morbid growth becomes detached (as in the case of tuberculous mass, which I have recorded in the *Medical Press*, vol. xi. p. 103), the symptoms will of course be essentially the same as those observed in cases of foreign body introduced from without.

with a fit of coughing, and expelled the coin. This he returned to exhibit with a certain amount of triumph.¹

The instance now recorded may be considered as one of those less common cases in which a foreign body has been expelled from the air-passages by natural efforts. As such a result, however, must be altogether fortuitous, it cannot, according to my judgment, be construed as affording a precedent for withholding an operation on similar occasions. This view will, I apprehend, be readily conceded by those who are conversant with such accidents; for there is perhaps no occurrence in surgical experience, the results of which are more varied, and sometimes more unexpectedly fatal, than that in question. In some instances, where the body, in entering, has been caught in the glottis, and possesses a suitable figure, complete obstruction and immediate death result; in others, having passed the aperture, it may become engaged in the ventricle of the larynx, or the crico-thyroid hollow; or, finally, it may descend into the bronchus. In the latter case, as the interbronchial crest deviates to the left of the median plane, the foreign substance will generally occupy the right bronchus. Should it be a light substance, it may be dislodged from thence by the unassisted efforts of coughing, and be either fortunately expelled, or kill the patient by becoming impacted in the glottis; or, lastly, it may become entangled in the interior of the larynx. Should it, on the contrary, be a heavy substance, such as a coin or bullet, it may become lodged for an indefinite period in the bronchus, and having produced symptoms more or less grave and persistent, either terminate the patient's existence by suppuration or gangrene of the lung, or (as occurs much more rarely) be expelled by cough when assisted by a favourable position. Of the latter happy result, a case has been lately recorded, in which a pistol bullet, under the conditions just assigned, was coughed up from the bronchus after forty days.

I think it, on the whole, very probable that, in the present case, the coin was lodged in the *ventricle* of the larynx. Its size and figure, taken in connection with the mildness of the symptoms, and the peculiar effect of change of posture on the sound of the cough, appears to support this view. Had the body not become thus engaged, it is likely that, in a subject of the patient's age, it would, as in Mr. Brunel's case, have descended to the bronchus, especially as the diameter of a fourpence is to that of a half-sovereign but as five to six. When the foreign body occupies the situation just described, it does not, however, the less endanger life, nor is it the less subject to sudden, disastrous, and unexpected changes of position. In many cases (perhaps in most), no precise evidence can be obtained of its exact situation.

Auscultation and percussion will afford valuable aid in some instances; as, for example, where a body of extended surface or considerable size occupies the bronchus in a position favourable to nearly complete obstruction of that tube. In many other cases, however, where the body occupies the larynx or trachea, and, as so often happens, is enveloped in mucus, or where it is of small volume, little assistance can be expected from the stethoscope; still, even its *negative* indications should be invariably sought for. In estimating the diagnosis of foreign bodies in the windpipe, I may be permitted, lastly, to remark that sufficient stress, as it appears to me, has not been laid on the fact that, although the pulmonary or laryngeal symptoms are in themselves often equivocal, they have suddenly attacked an individual who had, immediately before, been engaged in some unguarded respiratory movement, and who had some foreign substance at the time in the mouth, and that although at first urgent, these symptoms often subside, to recur again with greater or less intensity.

With regard to the treatment which I had proposed to myself, it will be observed that, in its essential features, it would have been the one adopted by that distinguished surgeon and physiologist, Sir B. Brodie, in the case of Mr. Brunel. From the indications afforded by the symptoms, it appeared to me,

¹ By a singular coincidence, I removed, not long afterwards, from the rectum, by the forceps, a fourpence, which had been swallowed, and arrested by a narrow stricture of that organ. It was blackened by the sulphur of the intestinal secretions. The coin from the larynx had, of course, undergone no such alteration.

however, that, in the present case, it would have been more feasible to have opened the crico-thyroid space (by *full* crucial incision and reflection of the flaps) than to have resorted to tracheotomy; for, considering the coin to be situated either in the ventricle or in some other part of the larynx, I judged that it would have been thus more directly accessible, in the event of its lying in a favourable position for immediate extraction.

In a case in which I removed a dog's tooth from the larynx of a girl of seven years, by tracheotomy, I found it impossible to reach the body with the curved forceps, until, fearing injury to the mucous membrane of the larynx, I prolonged the incision, not only through the cricoid cartilage, but also through part of the crico-thyroid ligament. Mr. Porter, who was present, concurred entirely in the propriety of this proceeding, which, I had the satisfaction to find, left the functions of the larynx quite unimpaired. As to the practicability of removing a body of the diameter of a fourpenny piece through the crico-thyroid space, this will be sufficiently apparent when I state that I have found the transverse diameter of that space, in the adult, to be seven-eighths of an inch, and double that of its vertical. Now, as a fourpenny has a diameter of five-eighths of an inch only, ample space would be thus afforded even for a larger body, especially when the elasticity of the parts is considered.¹

I do not perceive any valid reason, anatomical or physiological, why the tube should not be divided when necessary (as in the case just mentioned) through the cricoid, or even through the *thyroid* cartilage; provided always, that the *median line* be strictly observed, and I feel satisfied that any speculative danger on that score is far outweighed by the actual mischief inflicted on the delicate and important mechanism of the larynx, by repeated ineffectual efforts to grasp the body from the *trachea*. The unossified cricoid cartilage permits of considerable divarication of its cut edges without any force; and in the event of the foreign body having passed into the bronchus, the space thus gained might be increased without danger by the removal of a thin slice from the margin, as often done in the trachea, and by prolonging the incision into the upper rings of the latter. A suitable slender forceps might be then readily introduced, while some of the dangers and inconveniences of tracheotomy would be thus avoided. I have found in the living subject, as above related, that the division of the isthmus of the thyroid body is not necessarily accompanied by any hemorrhage, nor is the latter likely to occur to any important extent, even in the adult, when the structure of the gland is not diseased. The operation just described, which, I may observe, has been proposed, and I believe performed, by some of the older French surgeons, would thus have the advantage of rendering *both* the larynx and bronchus accessible in those numerous cases in which doubt exists as to the precise situation of the foreign body.

With regard to the operation of inverting the patient's body, viewed in connection with an artificial opening in the windpipe, this well-considered manœuvre, as is well known, was crowned with success in Mr. Brunel's case, and happily illustrates the importance of that union of physiological and surgical science, of which Sir B. Brodie's career affords so distinguished an example. Had it been necessary to have resorted to this procedure in the case which forms the subject of the present observations, the question might naturally have arisen, Whether the proximity of the proposed artificial opening to the glottis, would have exerted any influence in preventing that quiescent condition of the muscles of the latter, which it is the intention of the operation to secure? Further: Whether inversion should *precede* the employment of the forceps, or be resorted to only in the event of the failure of the latter? And lastly, where the former plan has been decided on, the inquiry would suggest itself, whether any, and what interval should elapse between laryngotomy and inversion? As respects the attempt to grasp the foreign body by the forceps, should it be found tangible from the wound, I apprehend few surgeons would be disposed to forego the attempt, in favour of a preliminary inversion of the patient, which, if unsuccessful, might lead to a transference of the substance to the bronchus, &c. These are questions which, within the limited number of facts at present at our

¹ I find the diameter of a half sovereign to be six-eighths of an inch.

disposal, cannot be answered with precision. In Mr. Brunel's case, sixteen days, I believe, elapsed between the operation and the subsequent inversion, and it seems, on general grounds, not unlikely (at least where a previous attempt to grasp the body has failed) that *some* interval should elapse, in order to reassure the laryngeal muscles, and to allow the glottis to acquire the passiveness essential to the success of the manoeuvre. In confirmation of this view, I think it will be found that in most cases where food has passed into the larynx, through a glottis paralyzed by tracheotomy, this result has not taken place very shortly after the operation.

When a coin has entered the larynx, it has been proposed by Dessault, and also by later observers, to introduce a gum-elastic catheter into the glottis, in the hope that, while air would be thus applied, the foreign body would glide edgewise along the instrument when the patient was inverted, and that hence an artificial opening might be dispensed with. Such a procedure, however, appears, on obvious grounds, to be not very feasible.

Although, in the foregoing observations, I have, in common with most surgeons, advocated the propriety of not trusting to the uncertain issue of natural efforts for the expulsion of the offending substance, still, no one who has witnessed many such cases can be insensible to the dangers which invest operations on the air-tube. Some of these arise, at times, most unexpectedly, as, for instance, spasm of the glottis at remote periods, after removal of the canula, when all is thought secure. But, on the other hand, circumstances occasionally occur, which must not be hastily charged on the operation. Thus, in a case which I have already recorded, the impaction of a mass of viscid mucus in the bronchial bifurcation caused sudden death many days after tracheotomy had been resorted to. Again, congestion of the brain sometimes takes place as the result of the violent respiratory struggles which succeed the accident, and may terminate life, *after the operation*, with convulsive or comatose symptoms, analogous to those observed in the secondary asphyxia of submersion.

OPHTHALMOLOGY.

51. *Successful Operation for Congenital Cataract.*—[The following case, communicated to the Royal Medical and Chirurgical Society (Nov. 14, 1854), by G. CRITCHETT, Esq., is interesting, and the discussion which followed, respecting the use of chloroform in operations on the eye, is particularly so. The statement of Mr. Thomson, that he had noticed deterioration of sight constantly to follow operations for strabismus, is contradicted by the experience of, we believe, every surgeon, and is certainly the reverse of what we have ourselves observed.]

Jane S., aged twenty-two, was brought to the Royal Ophthalmic Hospital in the spring of 1849, suffering from cataract in both eyes, with slight internal strabismus, and considerable involuntary rolling and oscillation of the globes from side to side. The corneæ were bright; the anterior chambers large; pupils small and irregular, and filled with an opaque white substance; perception of light was good. She had frequently been operated on before. Mr. Critchett first succeeded in detaching the adherent iridis from the capsules by the usual needle operation, the puncture being made through the cornea. By a second proceeding, he made a small opening through the scleroticæ, and dragged the capsule from the sphere of vision by means of a hook in one eye, and fine forceps in the other. Then to remedy the oscillatory movement, which prevented the impression of any object distinctly upon the retina, he divided, first the internal and then the external recti muscles. The sight is now improving, and by education she is slowly beginning to make some valuable use of her newly-acquired sense.

Mr. FERGUSON observed that it was not often that papers referring to operations on the eye came before the Society, and he therefore would put a ques-

tion to the author on a subject which he thought of importance, and which had not been referred to in the valuable paper before them. Was chloroform used in the present case, and if so, with what effect? He (Mr. Fergusson) should expect that agent to be of great service in controlling the muscular movements of the eye in cases requiring operation.

Mr. CRITCHETT replied that, in the operation narrated in the paper, chloroform (at that time seldom used) was not employed. He did not think, however, that it was so important an agent in these as in many other operations, because the muscular movements of the eye could be entirely controlled, and the globe fixed by the speculum and other mechanical means. Regarding the use of chloroform generally, it had been of late occasionally used in the Ophthalmic Hospital in cases of extraction, such cases being selected for its employment in which there was fear of irritability and spasm coming on, and forcing out the lens. Mr. Bowman, who had first employed the agent, and himself, had reason to think it of great value. Its tendency to produce sickness was an objection to its employment in operations on the eye, but if it were given on an empty stomach this was not likely to take place. Even in cases in which sickness had occurred, no ill effects followed, in consequence of the lids being fixed down by strapping, and the globe of the eye collapsed from the operation. In cases also of artificial pupil in children he had employed it with much advantage.

Mr. DIXON had never used chloroform in cases of extraction. Mr. Critchett had stated that he had never seen any ill effects from the vomiting following its administration, but there was another evil which might sometimes result from its use: this was nausea, squeamishness, and loss of appetite, which might last for twenty-four hours. In very old persons it was highly necessary, in addition to keeping the eye at rest, to support the strength by nourishment. If food could not be taken in consequence of nausea, support could not be administered, and the operation would prove unsuccessful from the wound failing to heal by the first intention. He thought that great caution should be exercised in using chloroform in these cases.

Mr. HOLMES COOTE had seen chloroform administered in three cases of extraction; in one instance, both eyes being affected with cataract. The chloroform was given to the extent of complete insensibility, yet no ill effect followed in either case. It was undoubtedly a beneficial agent, but required to be used with circumspection.

Mr. HALE THOMSON had employed chloroform in very many cases, but regarded it as less useful in operations on the eye than in most other operations. Indeed, in such cases he had never seen any benefit result from its employment. The case before the Society was one of much interest, and it was really astonishing that the organ existed at all after the performance of so many operations upon it. He had himself, some years since, operated upon a boy of fourteen years of age, for congenital cataract, and though the operation was perfectly successful, it was twelve months before the eye was sufficiently educated to properly appreciate the nature of objects coming before it. In Mr. Critchett's case, as the sight was not perfectly restored, he (Mr. Thomson) thought the perfect sense of the organ would never be attained. He objected to the division of the recti muscles for the purpose of steadying the eye, for he had noticed deterioration of sight constantly follow operations for strabismus. He also objected to the mode of detaching the lens by an incision through the cornea, and considered that this might be more easily and readily effected by making the cut through the sclerotic, behind the cornea.

Mr. CRITCHETT was still of opinion that the operation through the cornea was the best, and in this he was supported by most of the authorities on eye operations. He did not think that the number of operations which had been performed interfered with the ultimate result, for it must be remembered that many of the most successful cases were those in which seven or eight operations were performed, a small portion only of the lens being removed after each operation. The division of the muscles in the case before them was no doubt an experiment, but he had to choose between the constant movements of the ball, and any bad results which might follow the operation. As he had never

seen any harm, but in all cases immediate or gradual improvement of sight follow the operation for strabismus, it was a sufficient answer to Mr. Thomson's objection. In the present instance, he did not believe any ill effects followed the operation.

52. *Chloroform Inhalations for Extraction of Cataract.*—During the last month or two, in the practice of the Royal London Ophthalmic Hospital, chloroform has been frequently employed prior to the operations for cataract by extraction. Hitherto, the rule had been to abstain from doing so, in fear that the subsequent sickness, which so frequently follows, might disturb the eye, and perhaps occasion extrusion of the vitreous. Mr. Bowman was, we believe, the first at this hospital to employ it, the case selected being one in which the patient was of so irritable a temperament that the operation seemed scarcely practicable without anæsthesia. The operation was performed, under its influence, with great ease, and the precaution being taken to support the globe afterwards by straps of adhesive plaster crossing the lids, no ill consequences ensued. It has now been employed in nearly twenty cases, and in none has there been any reason to regret its use. In some, vomiting has not followed; but in others it has been severe, without in any way occasioning damage to the eye. The performance of the steps of the operation are much facilitated by the perfect quietude of the eyeball which it secures.—*Med. Times and Gaz.* Sept. 9, 1854.

53. *Luxation of the Crystalline Lens; Capsulo-lenticular Cataract; Extraction; Wound of the Iris; Iridoplasty.*—The following very curious, we believe unique case, by Dr. ALESSI, of Rome, is related in the *Archives d'Ophthalmologie* (Feb., Mars, 1854).

"The subject of this case was affected with capsulo-lenticular cataract, with spontaneous displacement of the crystalline lens, complicated with chronic retinitis. The cataract was soft and large, and pressed the iris forwards so as almost to completely obliterate the anterior chamber. The patient suffered such acute pain that he earnestly requested an operation for his relief. Though foreseeing the difficulties he would have to encounter from the absence of the anterior chamber, and the probable softening of the vitreous humour, Dr. A. yielded to the urgent and repeated solicitations of the patient, and determined to extract the lens. Placing the patient in a recumbent position, he made the inferior section of the cornea; but in doing this, owing to the iris being almost in contact with the posterior face of the cornea, the iris was cut, and the capsule of the lens at the same time opened. The lens passed out through the incision without difficulty. Immediately afterwards it was observed that there was a transverse elliptical opening at the inferior portion of the iris, forming a true artificial pupil, which was separated from the natural pupil by a band of about three to four millimetres broad. There was also in the upper portion of the posterior chamber a portion of opaque capsule. The operator endeavoured, by slight pressure on the upper part of the eye, to force down this portion of capsule so as to extract it through the wound in the cornea. But he was unable to force it into the wound in the cornea; he could only press it into the wound of the iris. He however observed that, when the pressure was discontinued, the capsule seemed disposed to remain in the wound in the iris, and to block up the artificial pupil. Dr. A. determined to take advantage of this happy circumstance, and, after engaging the capsule as far as he could in the wound in the iris, he desisted from further attempts, trusting that adhesion between the capsule and iris would take place, and the wound in the iris thus fill up. The hopes of Dr. A. were entirely realized. At the end of eight days, Dr. A. had the satisfaction of finding that the patient had received no injury either from the operation or from the wound of the iris, that the fragment of capsule was firmly adherent to the iris, and that the artificial pupil was thus entirely obliterated.

54. *Accommodation of the Eye.*—Prof. L. FICK considers the *urea* to be the apparatus which effects, besides the regulation of the quantity of light (through the motion of the pupil), the accommodation of the eye for near and remote objects,

by means of a change in the position and form of the lens, placing varying quantities of blood now before, then behind the lens.

The facts on which this view is based are the following: 1. The very limited connection between the vessels of the choroidea and the iris. 2. The facility with which, by injection from the simple *venæ vorticosæ* and ciliary arteries of the choroidea, a considerable part of the less minute arteries and veins, forming the larger meshes of the choroidea, and a corresponding portion of the *processus ciliares* is filled, while the capillary layer of the choroidea (*membr. Ruyschiana*) is only filled by increased force, the vessels of the iris remaining empty. 3. The vascular arrangement in the choroidea and *processus ciliares* exhibits no analogy with the vascular apparatus of other organs destined for their nutrition, exhibiting scarcely any capillary system except that of the *membr. Ruyschiana*, but an almost immediate transition from the arteries into the veins. 4. The perfectly normal state of the vascular apparatus in the uvea of Albinos demonstrates that the production of pigmentum can certainly not be the only function of this apparatus. 5. The existence of so strong a junction between the *capsula lentis* and the choroidea where the prolonged folds of the choroidea project into the posterior chamber of the eye. 6. The *contractility* of the *processus ciliares*, which L. Fick assumes as well from experiments performed with A. Fick (having produced contraction of the congested ciliary process by means of electricity), as also from the circumstance of the *processus ciliares* being, after death, always found empty, except after death by strangulation, as also from pathological facts. 7. The peripheric part of the lens is softer than the central.

Fick sees in the *erectile* and *contractile* vessels of the *processus ciliares* an apparatus by which, the form of the *bulbus* remaining unchangeable, in the state of impletion of the vessels, pressure is exercised on the anterior *capsula* of the lens, while through their depletion into the *venæ vorticosæ* a pressure is exercised on the posterior *capsula*, effecting the lens slightly to advance towards the anterior chamber, and to become at the same time rather more convex—a process in which the difference in the consistency of the fluids of both chambers is likewise of importance.—*Monthly Journ. Med. Sci.* Oct. 1854, from *Muller's Archives*.

55. *Application of Coniin in Scrofulous Photophobia*.—Prof. MAUTHNER, of Vienna, extols the efficiency of the external application of coniin for the non-inflammatory spasmodic contraction of the *orbicularis palpebrarum* muscle in scrofulous children. The following is his formula for its use. R.—*Coniinæ* gr. ss; ol. amygdal. dulc. ʒj; solve. The eyelids are to be pencilled with this twice or thrice daily. He says the most obstinate cases are cured by it in from eight to fourteen days.

He also recommends this preparation for hard, indolent, glandular swellings of the neck.—*Journ. für Kinderkrankheiten*, 1854.

56. *Treatment of Wounds of the Eye*.—Modern ophthalmic surgery has very considerably deviated from what used to be the usual practice in cases of wounds, either incised or punctured, of the eye. In a large majority of cases, it was formerly considered necessary to employ pretty active antiphlogistic measures in order to obviate the much-dreaded effects of inflammation. As a consequence of this habit, frequent examinations of the organ, in order to watch what was going on, became necessary. It is now known that, in all such cases, there must of necessity follow considerable inflammation. A degree of congestion, and the effusion about the injured parts of much soft lymph, are, therefore, looked upon as matters of course, and as stages of the healing process not to be interfered with. The great principle of the treatment which we see pursued at the Royal London Ophthalmic Hospital is to keep the injured organ completely at rest. Almost every one is experimentally familiar with the degree of comfort, in cases of slight scratches of the eye, or when dust has got into it, which is afforded by gently supporting the closed lids with the hand. So long as the lids are kept close and still, the pain is absent; but the moment any movement is attempted, a sharp stab of pain is caused. By extending the knowledge thus

gained to the more severe injuries, we arrive at the principle alluded to. The following is the detail of the measures usually adopted at the institution named: On the patient's first application, the eye is carefully inspected; if any foreign matters are discovered, they are removed; if any portion of the front of the cornea have been partially detached, or if any considerable portion of iris have been protruded so as to be irreducible, the piece is snipped away by means of small scissors. It is rare that anything can be done in improving the position of the edges of the wound, or in returning protruded structures; and the next step is, therefore, to close the lids, and, having carefully padded them with layers of cotton-wool, the whole is confined by strips of adhesive plaster. A mild dose of aperient medicine is generally given, and the patient directed to live carefully on a rather less stimulating diet than ordinary. Unless urgent symptoms make it necessary earlier, which is very rare, the eye is not again inspected for several days, or perhaps a week, when the dressings are cautiously removed and replaced. It is very infrequent, indeed, for any depletory measures, either topical or general, to be deemed necessary. The symptoms which would excite alarm, and be held to indicate the necessity for further examination, are severe tensive or throbbing pain in the globe, or general fever; and, in the absence of both these, it is judged certain that interference cannot be needed, and that frequent inspections of the organ could but be productive of injury.

We may remark that the plan of padding the lids and keeping the eye closed, is by far the best that can be pursued, after the removal of dirt, etc., from the eye, or after slight injuries to it from blows or scratches. Under such circumstances, it is rarely required more than a day or two.—*Med. Times and Gaz.* September 9, 1834.

57. *Treatment of Tinea Tarsi.*—The treatment which is the favourite at the Moorfields Royal Ophthalmic Hospital in this troublesome disease, consists in carefully taking away all scales and crusts, and then rubbing the edge of the lid with solid lunar caustic. The application must be repeated twice a week, and patiently continued. It is absolutely necessary that all deposit be cleared away, so as to allow the remedy to come in direct contact with the cuticle, the opening of the hair follicles, orifices of the Meibomian glands, etc. If the treatment be steadily persevered with, the redness subsides, the thickening of the lid is removed, and the hairs previously destroyed grow again. Mr. Critchett is accustomed to relate a case in which a fair set of eyelashes were reproduced, after they had been absent for many years. The disease appears to be much more frequently secondary to measles than to any other affection, and its subjects are generally more or less cachectic. It often varies remarkably with the condition of the patient's health. Tonics are, therefore, indicated, together with a liberal diet; but it is well proved that these, without local treatment, are not competent to cure the disease. At the Hospital for Skin Diseases, in several cases recently, the hairs and their bulbs have been examined with the microscope. It does not appear that any cryptogamic sporules are ever present, the structure of the hair being healthy. In pulling out an eyelash, the hair-sheath is generally brought away too, and on its exterior are numerous pus and exudation-cells, showing that the inflammatory action has been external to the hair-sheath. In this respect, the hairs resemble those of sycosis and of impetigo occurring on hairy parts of the face. Mr. Startin's treatment consists in smearing the edge of the lid every night with a mild mercurial ointment, (ammonio-chloride, 10 grains to the ounce), and administering an arsenical tonic in combination either with iodine or mercury.—*Med. Times and Gaz.* Oct. 28, 1854.

MIDWIFERY.

58. *Perforation of the Head of the Fœtus; Labour Artificially Induced, and twice the Cæsarian Section successfully performed on the same Woman.* By Dr. R. H. BROERS.—The subject of this extraordinary case was 33 years old, and

primiparous, when Dr. Broers was called on by Herr Nuijens to assist him in her delivery, as he had already in vain attempted extraction with the forceps. Dr. Broers found the head impacted very high up, the conjugate diameter being there narrow, and assented to the performance of perforation, as the pulsation of the foetal heart was no longer audible, and the attempt to return the head had failed. After a great quantity of brain had been discharged, it was found necessary to remove several pieces of bone before the head could pass through the conjugate diameter. After the birth of the child, which was well developed, a very violent hemorrhage set in, necessitating the removal of the placenta. On the introduction of the hand, it was ascertained that the conjugate diameter did not amount to quite three inches, that the promontory of the sacrum projected strongly forward, and that the pelvis was larger on the right than on the left side, but that it was in general too narrow. Convalescence proceeded favourably, and the patient was able in three weeks to go out. After the lapse of more than a year, the woman came to report that she was again about seven months pregnant. It was determined to induce labour in the thirty-second week, after Riecke's method, viz: by introducing and leaving a bougie between the membrane and the wall of the uterus. On the third day, the os began slowly to dilate, and a second abdominal position was recognized. Turning was successfully performed, but with extreme difficulty, in particular the elimination of the head was almost impossible. The child, a female, was still-born; it was pretty well developed; $15\frac{1}{2}$ inches long; weighed 6 pounds; the short diameter of the head, $3\frac{1}{4}$ inches; the long diameter, $4\frac{1}{4}$; the perpendicular measurement, $3\frac{1}{4}$.¹ Convalescence proceeded regularly; the patient was well in three weeks. Pregnant for the third time, she did not apply for assistance until the last. It was determined, in consultation with HH. de Bordes and Hoogwinkel, to perform the Cæsarean operation, although the existence of pulsation in the foetal heart was doubtful. The child had moved a few moments before, and the mother's state was favourable. The waters had already been discharged when the operation was commenced; the opening into the abdominal parietes (linea alba) and the peritoneum was six inches long; that into the wall of the uterus and the membrane was five inches; the incision fell close to the insertion of the placenta. The child, a female, well developed, eight pounds weight, above seventeen inches long, was dead; the after-birth was removed through the wound. The latter, united by suture, healed so rapidly, that in fourteen days the woman might be looked on as recovered. Two years subsequently she again returned, in the ninth month of pregnancy. The necessity of performing the Cæsarean section was again agreed on, and the operation was on this occasion performed by Herr Hoogwinkel, in the same manner as before. At the beginning of the operation the membranes were still unbroken, the os was dilated to the size of a guilder; the head again presented. The incision, close to the cicatrix of the former, fell also on this occasion contiguous to the placenta; the membranes gave way during the operation. A well-developed living male child, 16 inches long, and weighing $7\frac{1}{2}$ pounds, was extracted, as well as the after-birth, through the incision. The uterus contracted slowly, and imperfectly; fever, with delirium, soon set in; the patient's strength diminished; there was no secretion of milk; the wound continued pale, without reaction. On the fifth day after the operation the woman died, aged 37. The child lived, and four years subsequently was in good health.—*Med. Times and Gaz.* Oct. 21, 1854, from *Nederlandsch Tijdschrift voor Verloskunde Ziekten der Vrouwen en der Kinderen*.

59. *Superfoetation*.—Dr. THIELMANN relates the following case. A peasant-woman, aged 25, had borne, at 20 and 23, girls. In July, 1852, she became pregnant a third time; menstruation appeared twice after conception. On the 26th March, 1853, the first pains appeared, and next morning she was delivered of a girl, small, but living; the after-birth came away normally. The lochia ceased in a few hours. The secretion of milk was so scanty that the child

¹ The Dutch pound is rather heavier than the English. The Rhineland foot is 12.36 inches.—*Penny Cyclopædia*; art. "Weights and Measures."

could not be supported by it. Eight days after delivery the woman returned to her household duties; but she felt in her left side the movements of a second child. On the 18th May—that is, fifty-two days after the birth of the first child—pains came on, and the birth of a second living girl, somewhat smaller, followed. From this time, the secretion of milk went on so freely that both children derived sufficient nourishment. M. Thielmann says this case was officially certified.—*Brit. and For. Med.-Chirurg. Rev.* Oct. 1854, from *Med. Zeitung Russ.* 50, 1854.

60. *Seventeen Cases of Parturition, in which Chloroform was inhaled with Injurious Effects.*—The *Medical Times and Gazette*, Sept. 9, contains an account, by Dr. ROBERT LEE, of seventeen cases of parturition, in which chloroform was inhaled with pernicious effects.

The following is a summary of these cases, with the remarks of the author:—

“In the first and second of these cases, the contractions of the uterus were arrested by the chloroform, and delivery was completed by craniotomy. Insanity and great disturbance of the functions of the brain followed its use in cases 3, 4, 5, 10, 14, 15, and 16. It became necessary to deliver with the forceps in cases 6, 8, 11, 12, and 13. Dangerous or fatal peritonitis, or phlebitis, ensued after the exhibition of chloroform in cases 7, 8, 11, and 13. Epilepsy followed in case 14, and dangerous fits of syncope in case 17.

Were I to add those cases which the reports of my medical friends have confided to me, and the still greater number which public rumour has brought to my knowledge, I should appal the Society by the amount of mischief which chloroform, given to parturient women, has already inflicted on individuals and families. The details of unfortunate cases, indeed, are generally studiously concealed; but the annals of surgery contain conclusive proofs of the mischievous and dangerous effects of this poison. However much the disasters of operations performed in private may be hushed up, the practice of hospitals cannot be concealed; and we have now a long list of calamitous cases in which the imbibition of a very small quantity of chloroform into the blood was sufficient to extinguish life in individuals of a robust habit and perfectly sound constitution. Were our knowledge of chloroform confined to this fact alone, it would suffice to remove all doubt from the mind of every intelligent practitioner as to its use in midwifery.

It might have been expected that a contemplation of the subtle action of this poison on the nervous system would alone have induced caution in its application to practice, till its influence on the system was more thoroughly understood. But we have been compelled, on the contrary, to witness the most reckless levity. Very soon after the discovery of its physiological effects, I was confounded by the announcement of its application to midwifery. It was not difficult for me to foresee that such rashness, as it could not then at least have a safe foundation, would lead to deplorable results; and I regret to say, I have not been mistaken. Yet then, as now, we were confidently assured of the perfect innocence of the remedy. The value of the present boasts may be judged of by the past.

It was not wonderful that women, doomed to bring forth their offspring in pain and sorrow, should seek to escape from one of the troubles of our race by means of this treacherous poison, particularly when presented to them with such flattering assurances; neither can we feel surprised that the instances of women who were reported to have been saved from the grievous pains of child-bearing, without bad consequences, should have for a time reduced to silence those unwelcome monitors who pointed to the possible evils of this new agent, and induced the honest but enthusiastic pursuers of novelty to turn away their eyes from the contemplation of those dropping cases of disaster which soon showed themselves, and to disturb the general jubilation. But it does seem to me strange, that, amid so wide-spread an experience as I am convinced now exists of the noxious and dangerous effects of chloroform, it should be necessary for me to assemble the proofs of the havoc it has made. Daily reports, however, convince me that this work is called for; and I have not shrunk from so sacred a duty.

Setting aside the mechanical difficulties of labour, the dangers to which par-

turient and puerperal women are most exposed may be said to be fourfold: 1, exhaustion; 2, hemorrhage; 3, fever and inflammation; and 4, cerebral disturbance. The great cause of flooding is languid or deficient contraction of the uterus. We are assured by many that the contractility of the womb is in no degree diminished by the action of chloroform. But of this important position we have as yet received not a jot of proof; nay, there are innumerable proofs to the contrary. It is expected that we should be satisfied with bare assertion; and, considering that it was made at a very early period, when not a score of women had yet been delivered under the influence of chloroform, and, moreover, that it is made by those who continue, in the face of the most painful contradiction of facts, to affirm the perfect innocence of this poison, we may be permitted to set aside this evidence without further notice. But I rely not upon *à priori* reasoning, but on the direct testimony of my own senses, and maintain, with this unerring guide, that the action of chloroform does very manifestly impede the uterine contractions, and, in some cases, put a stop to them altogether.

The wise and skilful practitioner will hardly require my evidence to satisfy him that so disturbing an agent must add greatly to the risks which arise from inflammation and fever. But they who doubt will find reason enough, in the cases I have cited, to pause and reflect; while the history here given of severe cerebral affection must surely satisfy the most stolid that all the nervous accidents which attend the puerperal condition, and complicate its risks, must be largely increased by this very active poison.

Much reflection on the physiological effects, and observation of the pathological mischief of chloroform, leave no doubt on my mind that it ought to be altogether expelled from the practice of midwifery. There are no circumstances in which it can be with utility, none in which it can be with safety, employed. I am confirmed in this opinion by conversation with the most discreet and experienced practitioners around me; yet I cannot but entertain grave doubts of the result of my present appeal to the good sense of my profession, when I consider the arts used to propagate a faith in this practice. It has become almost an extra-professional question, while there is a systematic concealment of truth by physicians. Appeals are made by others to the natural timidity of women, and the most fallacious promises of perfect safety are boldly held out. Conceited and ignorant women of fashion make a pastime of this, as of other quackeries, especially the speculum, and the cause of science and humanity is placed in the hands of the most presumptuous and frivolous part of the community, while young inexperienced mothers are decoyed to their destruction. It is no unfrequent occurrence that an accoucheur should be selected to attend a given woman, but previously told that he must use chloroform. This grave question of medical science has been predetermined by a quorum of old women, instigated, perhaps, by an itinerant duchess. There are men to whom such propositions are not at all insulting. They are quite ready to steal a march on their wiser and more manly brothers, by the adoption of any humiliating fashion. Thus, the health and lives of patients are sacrificed, and medical science is dishonoured.

If I have helped to rescue the medical profession from the dominion of a great and dangerous error, if I have placed some restraint on ignominious and disgraceful practice, I shall rest satisfied that this essay has not been written in vain."

61. *Treatment of Displacement of the Uterus by Intra-uterine Pessaries.*—This subject, which has lately created an extraordinary sensation in the Academy of Medicine, was first brought before them by Dr. Broca, who detailed a case from his own practice, where death had occurred after the use of the uterine catheter. M. Cruveilhier followed up the statement by the relation of another fatal case, and expressed himself as strongly opposed to the use of uterine instruments. A committee, composed of MM. Robert, Huguier, and Depaul, was appointed; and their report, drawn up by M. Depaul, after entering at great length into the whole subject of uterine displacements, has condemned in most severe and decided terms, the use of the intra-uterine pessa-

ries and bougies. The following is a condensed account of the cases, and the report:—

Broca's Case.—A woman, æt. 39, who had borne three children, the youngest ten years ago, was admitted to the Hôpital de Lourcine, October 4, 1853. She was thin, and wasted (flétrie), but her general health was tolerably good, and she stated that she had never had any serious disease. About a year before admission, from which period she dates her illness, the menses became suppressed, and never returned; at the same time pains in the thighs, disorder of the digestion, habitual and obstinate constipation, and frequent calls to make water came on. She stated that she had got much thinner, and suffered much. On examination, very marked anteversion was detected; the cervix was enlarged and granular, and a considerable quantity of glairy and purulent matter was discharged from the os. On the 7th October, the uterine sound was introduced; it reached to $6\frac{1}{4}$ centimetres. The uterus was easily brought to the normal direction, and kept there for five minutes; the patient complaining of no pain. On the 8th and 10th, the same manœuvre was repeated, and the patient declared herself much relieved. On the 11th, slight pain was felt on the introduction of the instrument, and the patient complained of the hypogastrium when the uterus was replaced in position; the sound was left in for two or three minutes. The pain became much more intense on the 12th, and was followed soon after by fever, nausea, bilious vomiting, and constipation; leeches and cataplasms were applied, and on the 15th the fever had ceased and the patient seemed better; but the vomiting continued, and intermittent pains came on frequently in the abdomen, apparently spreading from the uterus. On the following days, the attacks of pain were more frequent, the vomiting became incessant, the pulse small, and the abdomen tympanitic, and the patient died on the 23d, after a long and painful struggle. *Autopsy, twenty-four hours after death.*—Skull not opened. Thoracic organs healthy. In the abdomen, old and strong adhesions of the right extremity of the transverse colon, and of the great omentum to the abdominal parietes; the stomach, duodenum, and jejunum were greatly distended to about 60 centimetres above the ilio-cæcal valve. *At this point the intestine was adherent to the uterus, and became suddenly contracted (retréci); above, it measured 16 centimetres in circumference, below, it was reduced to the size of the index finger.* The whole of the large intestine, as far as the descending colon, was strongly retracted (*fortement revenu sur lui-même*) and empty; beyond this point the caliber was diminished, but not to the same degree. *Below the adhesion, the intestine was discoloured, but healthy; above it, as far as the duodenum, which was sound, there were evident traces of congestion and inflammation.* Independently of these old adhesions, there were recent ones formed by a grayish gelatinous juice, semitransparent, and not yet organized; in short, there were evidences of two attacks of peritonitis, one of old standing, the other recent.

The right Fallopian tube was enormously dilated, and connected by old adhesions to the uterus and rectum; no trace of the right ovary could be found. The cavity of the Fallopian tube contained a considerable quantity of chocolate coloured pus. At the angle of the uterus, near the insertion of the right Fallopian tube, was found a small collection of pus very near the peritoneal surface.

M. Broca supposes that the introduction of the sound caused first metritis, and subsequently peritonitis, and that the recent being added to the old adhesions, had completed the intestinal obstruction. On the other hand, M. Vallex contends, that the obstruction pre-existed before the sound was introduced, and was quite independent of it. The pathological appearances are certainly described in a confused manner in M. Broca's report; at one part it is stated "that there was no fluid in the peritoneum, and no recent false membrane, and that, with the exception of the old adhesions, the peritoneum might be called healthy." And at the end of the description we are told of the exudations of recent lymph (*des adhérences formées par un suc gelatineux grisâtre, demitransparent, non encore organisé*), showing recent peritonitis.

Cruveilhier's Case.—A young woman, æt. 24, married for four years, and extremely distressed at having no children, was found to have a slight ante-

version of the uterus, to which her barrenness was ascribed. The introduction of the uterine sound having occasioned great pain, she removed to Paris and put herself under the care of M. Valleix. During a residence of a month there, the sound was introduced five times, but could never be retained beyond a few hours, on account of the sickness, distension of the abdomen, and severe pain (angoisses), which required the withdrawal of the instrument. Her health becoming so much affected, that her mother insisted on giving up the treatment and returning home. On her arrival there, the family physician recognized the symptoms of metro-peritonitis, in an aggravated form. Her condition becoming worse, M. Cruveilhier was sent for; he found the anteversion still persistent; an extreme degree of marasmus; the symptoms of peritonitis, especially the pain of abdomen, had somewhat abated, but the uterus was extremely tender. The pulse was filiform, and numbered 120 in the minute. The chest was examined; some mucous râles existed at the apex of the right lung, but there were no signs of tubercles. A week after, the patient died. The examination of the body was not allowed.

M. Valleix is of opinion that the patient was tubercular, and had probably sunk under tubercular peritonitis.

M. Depaul's Report.—In commencing his report, M. Depaul undertakes to prove—1. That uterine affections, usually attributed to displacement, have an entirely different origin. 2. That in most of the alleged cases, another more frequent pathological condition, which produces uterine symptoms, and even sometimes causes displacement, has been overlooked. 3. That science possesses a simple and rational treatment for displacements, as efficacious, or more so, than intra-uterine pessaries. 4. That the facts adduced in favour of the treatment by these instruments, only show their entire inefficiency; and 5. That we must take into serious consideration the numerous facts which prove that the most formidable affections, and even death itself, may be the consequence of these manœuvres, which are besides, on their first aspect, *repugnant to common sense!* (*repugnant à la raison.*)

Instruments.—Before proceeding to these points, the reporter specifies the instruments used in the treatment of uterine displacements, to which allusion is made. These are of two kinds (sufficiently well known in this country), 1. The *uterine sound*, of Simpson and Kiwisch (Recamier and Amussat had previously used a similar instrument). This instrument, which is of undoubted service in diagnosis, is also used to restore the displaced uterus to the normal position; but it is never left permanently in the uterus. 2. The *pessaries*, of different kinds, which are used for the replacement and permanent support of the uterus, remaining for a considerable time in its cavity. These are known as Simpson's and Kiwisch's, although the original idea of the invention is claimed for Amussat and Velpeau. They are, 1. The wire pessary, or pubic pessary, having a stalk to support the interior of the uterus, in connection with an apparatus resting on the pubis. 2. The spring pessary. 3. The ball pessary, with the stalk for introducing it. 4. The galvanic pessary, made of zinc and copper. 5. The dilating pessaries, used in obstructive dysmenorrhœa, sterility, &c. The last three instruments consist of a metallic stalk, which is passed into the cavity of the womb, and is fixed on an oval disk or ball. Although the use of the uterine sound is not without danger, it is principally against the intra-uterine pessaries that the criticisms of M. Depaul are directed.

Pathology, &c.—The reporter then enters on the first division of his subject, viz: the Pathology of Uterine Displacements. *What is the normal direction of the uterus? What are its displacements? Do these displacements produce the symptoms which have been ascribed to them?* M. Depaul does not agree without reserve with Cruveilhier, and others who maintain that the uterus has no certain direction; and he rejects the opinion of Boulard and Verneuil, that anteversion is the normal position of the womb. He maintains that, however liable to vary from accidental causes, the uterus is placed normally in the direction of the axis of the brim of the pelvis. When the axis of the uterus is more inclined in any one direction, we have either *ante*, *retro*, or *latero-versions*. When the whole uterus is pushed from its position, we have total displacement

(*refoulement*), which may also take place *forwards, backwards, or laterally*. When the uterus is bent on itself, *ante, retro, or latero-flexions*, are produced. There are also, more rarely, *flexions of the neck on the body* of the uterus; and *inflexions* of the uterus, where it is curved once, or twice like the letter *S* italic. Another condition which may exist alone, or combined with the preceding, is the *descent* of the uterus (*abaissement*), generally the inferior extremity, but sometimes the middle, or possibly even the upper part of the uterus passing low down into the pelvic cavity. These displacements by descent appear to have been overlooked in France by those who used the "instrumental treatment" ("*traitement mécanique*").

These different displacements are so common, that more than half probably of the female sex are subjects of them, and would require treatment, if these conditions were really diseases. But such is not the case; their influence has been greatly exaggerated, and it may be shown that the *symptoms attributed to these deviations belong to some other pathological condition*. In a critical analysis of M. Valleix's cases of displacement, treated by the intra-uterine pessaries, concomitant disease of the uterus, as hypertrophy and engorgement, or granulations and ulcerations of the cervix, leucorrhœa, &c. are found to have existed in the largest number: in a few, the uterine deviation, discovered only after death, had produced no symptoms during life; and in some the morbid symptoms remained after the uterine displacement had been corrected. The cases of M. Gaussail, analyzed in the same way, are similarly complicated. The symptoms of all the patients were much alike—viz: uneasiness in walking, pains in the loins, white or sanguineous discharges, constipation, and difficult micturition, and disorder of digestion. Yet all these affections are ascribed by M. Valleix and Gaussail exclusively to the displacement, taking no account of the hypertrophy, ulceration, or other diseased state of the uterus, which existed at the same time. The facts of M. Piachaud (pupil of M. Valleix) are of the same inconclusive stamp as the preceding; but he adds that he has witnessed dangerous (*graves*) hemorrhages, and in two cases perforation of the uterus in consequence of the use of the pessaries!

If in these cases, complicated with other lesions, the symptoms cannot be with certainty referred to the deviation exclusive of the lesions, still less is there evidence to show that the deviations *alone* give rise to serious symptoms. MM. Dubois and Lisfranc declared that displacements were innocuous, unless attended with pathological lesion or chronic inflammation. In a number of cases of displacements of all kinds observed by M. Gosselin, at the Lourcine, none of the women had any uterine complaint whatever; in a number of other cases, where uterine pains existed, there was no displacement, and the pains were owing either to inflammation or neuralgia; and in a third set of cases, where both uterine pains and displacement co-existed, the pains ceased after repose, and the use of antiphlogistics, narcotics, &c. although the displacement persisted. In 27 cases of simple deviation, observed by M. Depaul, only two had any uterine symptoms whatever; the one (anteflexion) had more frequent micturition, and a sense of weight after long walks; the other (descent of uterus) had pains in the inside of the thighs, and a feeling of weight in the pelvis, and that only after fatiguing exertion. Moreover, in diseased states of the uterus, if the morbid condition of the organ is cured, the painful symptoms entirely disappear; and every day's experience proves that all the uterine symptoms may be produced by lesions of the organ, unattended by any displacement whatever, to which they could possibly be ascribed. For the truth of the latter statements the reporter appeals to his own, and to the general experience of the profession.

With regard to the influence of the dilating pessaries on sterility, M. Depaul has similar objections, viz: that in the facts observed no adequate account has been taken of concomitant pathological conditions; and he maintains that displacements and flexions could only cause sterility when of very old standing, where the uterus is almost always atrophied, and dilatation consequently useless.

Admitting, however, that in a *very small number of cases* the mere displacement of the uterus may disorder the health, or may have an injurious influence

on co-existing diseased conditions, M. Depaul believes that there exist numerous efficacious methods of treatment which do not compromise the health or life of patients. Of these means, he enumerates rest, fixation of the uterus (*immobiliser l'utérus*), taking off the weight of the intestines, avoiding tight lacing, using hypogastric belts, and lastly, the different pessaries (not intra-uterine) particularly that of M. Garriel, composed of caoutchouc distended by air; also stuffing the rectum in cases of retroversion, as practised by M. Huguier. He mentions also the pessaries in whalebone of Dr. Mayer, and that of caoutchouc of M. Joret. He discountenances the use of cauterizations to produce adhesions for the purpose of replacing the uterus.

Cases.—The most interesting inquiry in a practical point, however, is an examination of the *statistics of the treatment by intra-uterine pessaries*. The number of cases published amounts to about 180, and of these, according to M. Valleix, 129 were cured. A result, at first sight, so strongly in favour of the treatment, is, however, completely reversed by the history of the cases themselves. The reporter here gives a short analysis of M. Valleix's cases, twenty in number, of five cases by M. Gaussail, and three by M. Piachaud. With regard to Dr. Simpson's practice, he was unable to give the results, as that gentleman, when applied to, had, he said, communicated only assertions without proofs; he could only, therefore, give an idea of his success from the information received from indirect sources.

It would be tedious to give the whole of these cases; we subjoin a few to show the method of analysis, and how inconclusive the cases are, or rather how they are turned into evidence against the treatment they were published to recommend.

Valleix's Cases.—Fifth case. C., æt. 31, some years after accouchement, suffered from hysteria, leucorrhœa, fatigue in walking, and weight in the pelvis, for which she was treated by repeated leeching. Coming under M. V.'s care in 1851, she was pale and anemic; the uterus was voluminous, not painful, lying transversely forwards. *On the anterior lip, which was red and voluminous, there was a prominence of a deeper colour, presenting in the middle a little white point.* The neck was cauterized on three successive occasions with the acid nitrate of mercury. Afterwards, the catheterism was practised six times, leaving three or four days interval. At the end of a month the uterus was replaced in its normal situation. *The cauterization was then resumed and repeated thirteen times during nearly two months.* There was no accident, and the cervix was cured. That cure, ascribed to the uterine sound, ought plainly to be attributed to the cauterizations. Several of the other cases are very similar to this.

Ninth case. Antelexion with hypertrophy, the uterus being heavy, with little mobility, the sound penetrating seven centimetres. The intra-uterine pessary (*redresseur*) having occasioned dangerous symptoms, an energetic anti-phlogistic treatment was employed, in consequence of which the patient was cured.

Thirteenth case. Retroversion, and chronic metritis with ulcerations and numerous red granulations. The intra-uterine pessary was applied once, and left in for a fortnight; long after this the patient underwent treatment for anemia, neuralgia, and for a fissure of the anus which had been overlooked at first. The cervix was cauterized for several months.

In others of his cases, there appears insufficient evidence of the state of the uterus, or even of the cure which is reported to have ensued. M. Gaussail's cases are generally complicated with metritis, and such varied and prolonged methods of treatment were used, that it is difficult to ascribe the cure to any one in particular. His fifth case he concludes by admitting that "*the treatment (by intra-uterine pessaries) had produced no amelioration—the patient felt more fatigued in walking, standing, or by simple movement of the arms. The modifications in the position of the uterus were hardly recognizable. The patient and the medical man were convinced that this treatment could no longer be endured.*" M. Piachaud's cases have already been alluded to. In addition, two cases are quoted, observed by M. Gaube, in which all the morbid symptoms were cured, although the use of the pessary failed to replace the uterus.

The last case given is one of anteversion, where the patient, dismissed cured from the hospital by M. Valleix, returned next day in as bad a condition as ever. Scanzoni, in *Canstatt's Jahresbericht*, states that in twenty cases of displacement, where he tried the mechanical treatment, he did not once obtain a permanent cure. It appears, moreover, that M. Valleix has been continually *improving* the intra-uterine pessary, by diminishing the length of the uterine stalk, till at last he has given it up altogether, and uses only the temporary introduction of the uterine sound, but always, he assures us, with the same successful results.

Having examined the statements of the principal partisans of this mechanical treatment in France, viz. MM. Valleix, and Gaussail, &c., M. Depaul proceeds to show that this treatment has no greater favour or success in other countries, particularly Great Britain. In regard to Dr. Simpson's practice, he quotes from an eye-witness, that although in many cases the relief is immediate, and in a few instances the instrument can be worn with impunity for months or even years, yet, in the *great majority of females*, some morbid state of the uterus or its appendages, &c., comes on; hemorrhages, rectal fissures, ulcerations of the rectum, metritis, metro-peritonitis, pelvic abscesses were pretty frequent consequences, and more than once death was the final result. Drs. Churchill, Ashwell, and Gream's statements are quoted as to the dangers of the treatment, together with Dr. Robert Lee's case, where the pessary, or, as he calls it, the impaling machine, was extracted from the uterus. Two fatal cases of peritonitis caused by the pessaries, described by Dr. Oldham; his strongly expressed opinions against the practice; the similar views of Montgomery, of Dublin, and Matthews Duncan, of Edinburgh, are adduced as strong evidence of the unfavourable estimation in which the practice is held by a large number of eminent obstetricians.

The same objections which have been made to the pessaries are also applicable to the uterine sound or catheter (*hysteromètre*), especially when used to replace the uterus in position. According to Depaul, in the vast majority of cases it is unnecessary even for diagnosis; it may even lead into error, and its use is not exempt from serious dangers. Guéneau de Mussy has recorded a case of fatal peritonitis in consequence of passing the uterine sound; three cases of abortion in the early stage of pregnancy, and one case of abortion and death have occurred in the practice of Huguier, Nonat, and Valleix. M. Depaul then passes under a final review the cases of Valleix, Gaussail, Piachaud, Huguier, Nonat, &c., and finds in nearly every one some of the following results from the use of the intra-uterine instruments: hemorrhages nearly in every case, and sometimes of dangerous amount; uterine and abdominal pains; rigors and fever; syncope and phlegmonous inflammation around the uterus, or in the iliac fossa; metro-peritonitis. A case of enormous phlegmon in the neighbourhood of the uterus is given in detail from M. Cazeaux; one of metro-peritonitis from M. Gaube; six cases (one fatal) from M. Nonat, in none of which was the displacement benefited by the treatment, which gave rise to the dangerous complications, pelvic abscesses, metro-peritonitis, &c.; and this enumeration is summed up by the fatal cases of MM. Broca and Cruveilhier, which originated the inquiry before the academy; followed by four fatal cases from the practice of MM. Valleix, Nelaton, and Aran, and the testimony of M. Maisonneuve that similar accidents have occurred in his hands.

Conclusion.—From these detailed investigations, the reporter draws up the following conclusions as the unanimous opinion of the committee:—

In the great majority of females, uterine displacements do not injure the health, and constitute a mere deformity without importance. Where displacements co-exist with inflammation or neuralgia, when the latter affections are cured, the former disappear, or if they persist are quite innocuous. The small number of simple displacements, which occasion inconvenience, are easily and safely treated by simple methods without incurring the dangers of the intra-uterine instruments, which, even when they momentarily replace the uterus, fail to fix it in the normal position. The facts adduced to show the efficacy of the mechanical treatment have been wrongly interpreted, and the cures ascribed to it have been owing to other means employed at the same time. The

fatal cases observed both in France and England are numerous enough to show the perils of this treatment, and to warn its boldest partisans. In addition to the mortality, the other dangers are of the most alarming kind; pains, sometimes agonizing, hemorrhages, anemia and nervous disorders, rigors, fever, syncope, peritonitis, pelvic abscesses, metro-peritonitis, &c., not to mention the cases where the treatment could not be endured. The uterine sound, of great advantage in the diagnosis of certain affections, is very often of no service, and, from the great risks attending its use, should be reserved for the exceptional cases which require it; and finally, that the different intra-uterine pessaries ought to be proscribed, because they are useless, and impotent to produce the good effects expected from them, and they subject the patients to the most serious dangers.—*Monthly Journ. Med. Sci.* Aug. 1854, from *Bulletin de l'Académie*, 31st May, 1854.

[We shall endeavour to find room for an analysis of the discussion to which this report gave rise in our next number.]

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

62. *Early Live Birth, with Case.*—Dr. KEILLER exhibited to the Edinburgh Obstetrical Society, a premature fœtus which was born *alive* in the fourth month, and made some remarks on the *medico-legal* relations and importance of such a case. The following are the particulars of the case, as noted at the time of its occurrence:—

On the 17th June, Dr. Keiller was called to Mrs. R., who was about to abort. She had miscarried about a year previously (20th July), when seven months pregnant, and now considered herself only in the fourth month, having last menstruated on the 8th February, and quickened about a week ago (8th June). The pains were evidently expulsive, and, on examination, the distended membranes were felt protruding into the vagina. Dr. K. shortly afterwards ruptured them, when the liquor amnii was forcibly and fully expelled, a foot immediately presented itself by which the extraction of the fœtus was speedily accomplished. The heart and vessels of the cord were beating vigorously, which induced Dr. K. to allow the fœtal circulation through the still attached placenta to continue for some time, in order to observe the reflex movements of the limbs, face, and respiratory muscles which thereafter took place. At first these muscular reflex contractions were very marked. On touching the feet and hands, the limbs were immediately drawn up and moved about. On blowing on the face, the lower part of it was tremulously moved, and the mouth at each time opened; and three or four times an attempt to respire or gasp, accompanied by an apparently respiratory movement of the chest or thoracic convulsion, took place. The pulsations of the carotic arteries were also at first very distinctly observed. The umbilical pulsations gradually diminished in force and frequency, and when reduced to about ninety beats in the minute, Dr. K. cut the cord, and allowed about a drachm of blood to ooze from its fœtal extremity, the heart's action immediately became quicker, and one or two thoracic convulsions afterwards followed. The mouth gaped repeatedly on blowing air on the features, the limbs gradually lost their reflex actions, and the heart's action, as seen against the thin walls of the chest, became more and more feeble; subsequently a few seconds elapsed between every observed pulsation, but more or less distinct movements occurred for nearly an hour from the time the fœtus was first expelled. On being weighed, the fœtus was found to be exactly $9\frac{1}{2}$ oz., in length it measured 8 inches; the placenta, with attached cord, weighed about 6 oz. The eyelids were adherent, the nose and ears closed, the mouth, however, being open; the membrana pupillaris was entire; on opening the chest, the situation and appearance of the lungs and other organs were characteristic of its apparent age. The lungs, in colour and volume, resembled those of an early fœtus; and, with the exception of one or two ecchymosed spots, no colour

or other evidence of developed air-cells was noticed, all the appearances indicating that no air whatever had ever reached the tissue of the lungs. The brain was afterwards minutely examined, and also found to be characteristic of the apparent age, as were also the other foetal organs.

Dr. Keiller referred to the importance of such cases of early live birth, in regard to civil jurisprudence, but more especially to the questions, *Possessio Fratris, Tenancy by Courtesy, etc.*, to which the case he had now reported was in several respects related. It was frequently a matter of considerable importance to determine even the momentary existence of children at birth, and therefore the subject of *live birth*, and the evidence by which its occurrence is held to be substantiated, occasionally become of extreme consequence in a legal point of view, more especially in cases of contested lawsuits relative to the inheritance of, or the succession to property. Dr. Keiller directed the attention of the Society to the decisions that had, from time to time, been given respecting *what constituted live birth*, and referred to the more recent cases, in civil jurisprudence, in which the question of live birth was held to be established without any evidence of what was formerly demanded, *respiration or crying*. The mere muscular movements of the limbs, or the features, independent of any signs of respiration (as alone happened in the preceding case), having been ruled as sufficient evidence of a child being born alive, is therefore now held to be ample enough *proof* of what is termed in such investigations "*live birth*."—*Monthly Journ. of Med. Sci.* Sept. 1854.

63. *What are the best Means of Resuscitation from an Overdose of Chloroform?* By JOHN LIZARS, Esq., late Professor of Surgery to the Royal College of Surgeons, and Senior Operating Surgeon to the Royal Infirmary, Edinburgh.—Death from the employment of chloroform as an anæsthetic, is a consequence which has long admitted neither of doubt nor denial. Hospital reports and coroners' inquests have, indeed, furnished such frequent occurrences of that kind as to have rendered the subject one of painful and public animadversion. But although fatal effects from the anæsthetic are more to be apprehended, and will, I believe, be found more frequently to occur in individuals subjected to its influence, in whom structural lesions in the heart, bloodvessels, or lungs, exist, nevertheless, *post-mortem* examinations have shown that chloroform has proved suddenly fatal, where no trace of disease could be detected in the cerebral or circulating systems. It appears, indeed, to be very satisfactorily established that chloroform ought not to be indiscriminately used; that there are morbid conditions of the body in which it is inadmissible; and that in every case in which it is resorted to the greatest care is required in its administration, and the most vigilant attention in watching its effects.

One great cause of danger cannot be too strongly enforced, and is the more deserving of notice, that it can be easily avoided, viz: *never to induce too suddenly unconsciousness*. It is absolutely necessary for its safe inhalation that the anæsthetic vapour be mixed with a large quantity of atmospheric air. The cessation of the pulse, after a few gasping inspirations, is the first symptom indicating immediate danger. This effect upon the circulating medium is simply the paralysis of the action of the heart, produced by the rapid inhalation of the chloroform vapour in a concentrated form, and undiluted with common air. The employment, therefore, of all kinds of apparatus or bottles, by which the pure anæsthetic fumes are only inhaled, ought to be abandoned.¹ The simplest and safest mode of administering chloroform is by the application to the nostrils and mouth of a pocket-handkerchief or small napkin, into the folds of which two or three drachms of the anæsthetic have been poured. These can be speedily removed when the symptoms of insensibility threaten, or supervene

¹ I must exempt from this objection the very ingenious instrument invented by the late Mr. Ramsay, of Barnton, constructed so as to admit of the admixture of an equal proportion of atmospheric air with the chloroform vapour when inhaled. It was frequently and most successfully used in all operations required to be performed on the horse. For an account and drawing of this instrument, see *Scottish Agricultural Journal* for 17th July, 1849.

too suddenly or severely. But such symptoms will be prevented by a proper adjustment of the handkerchief, and the safe and full effects of the narcotic agent will be produced by its gradual diminution or increase, according to circumstances. Dr. Snow's plan of mixing equal parts of spirits of wine with chloroform will prevent the danger arising from chloroform too concentrated, and seems well adapted to sensitive and irritable persons.

While rapid anæsthesia from pure chloroform vapour should be avoided as being highly dangerous, the opposite extreme must not be fallen into, of keeping the patient too long under its influence. In hospital practice, I have been informed of very distressing instances of this kind. An unnecessary greater quantity of chloroform must be given, and the patient kept unsafely longer under it, if administered before the patient is brought into the operating theatre. Consequent sickness and vomiting, as remarked by Professor Miller, are more likely to ensue under such circumstances. The time and place of inducing anæsthesia should be unquestionably the operating table, and the exceptional administration of chloroform in bed ought to be limited to those patients who are timid and apprehensive.

I am decidedly of opinion that chloroform should be exclusively intrusted in its administration to individuals having great practical experience in its inhalation; and I have nothing to add to the following observations contained in my communication on this important subject, which appeared in the *Medical Gazette* for 8th June, 1849: "An experienced assistant is required to administer the chloroform, and to do nothing else. He must watch its effects, allowing fresh atmospheric air to enter the nostrils and mouth occasionally during its administration and influence, otherwise the blood may become too greatly carbonized, and death ensue." Previous to the administration of chloroform, the state of the stomach should be carefully attended to. It should not be allowed to be overloaded, and, indeed, the patient, for several hours before being operated on, should have little or no food.

I have stated that the primary effect which is produced on the circulation by a sudden overdose of chloroform, is paralyzing the action of the heart. In consequence of the blood becoming abnormally carbonized, the heart, lungs, and brain are deprived of the natural stimulus of that fluid, and life is thus rapidly extinguished. After death, little or no blood is found in the left auricle or ventricle; what of it is contained there is in a fluid state, and not liable to coagulate, when exposed to the atmospheric air. It has been observed to be of a purple-brown colour, like that found in the spleen, and not of the ordinary dark blood colour of venous blood.

Many and various are the remedial means recommended for resuscitating from an overdose of chloroform, from the simple application of errhines to the more than questionable operation of tracheotomy. Dr. Snow considers that "artificial respiration is the best means." We presume that the process is to be effected by means of the bellows. Dr. Herapath, of Bristol, has published a paper, recommending galvanism as affording "the only chance" of recovery. He says, "keep up a current of electricity through the fifth nerve, medulla oblongata, phrenic nerves, and diaphragm, as long as respiratory movements can be produced; and let the patient have plenty of fresh air or oxygen gas, and the case must do well, for the blood will remain fluid for a long time, and the circulation will go on, as long as respiration continues to be carried on artificially. The blood and the air-cells throw off their load, and in proportion as the pneumo-gastric, medulla oblongata, and motor nerves resume their functions, so respiration begins to assume a less artificial character; at length the cerebrum aids us, and respiratory movements, both voluntary and involuntary, keep up the functions of life unaided."

Professor Rigaud advocates a method, by which he says life has been on various occasions saved, and is detailed in the following case, extracted from the *Abeille Médicale*, of 3d Nov. 1851: "Professor Rigaud was on the point of removing a tumour from the chest of a female patient. After a few inspirations of chloroform, the pulse stopped suddenly, and the woman did not give any signs of life. The chloroform was at once removed, and water dashed on the face, and frictions made all over the body. These means, in a couple of minutes,

produced a few weak pulsations of the heart, which, however, ceased almost immediately, and were not accompanied by any respiratory act. Professor Rigaud now thought of using the method which had been advocated by Dr. Escalier, and passed his index finger along the dorsum of the patient's tongue, and raised the epiglottis, and drew the former out of the mouth. This had the effect of producing an inspiration, which circumstance was taken advantage of to make the patient inhale ammonia. As soon, however, as the tongue was lost hold of, it glided back again into the mouth, and respiration ceased again. The same manœuvre was now repeated; but this time Professor Rigaud kept the patient's tongue out of the mouth; the respiration then set in again, and the woman quickly recovered. The operation was then performed without chloroform, and Professor Rigaud considers Escalier's method saved the patient." The history of the case would justify the conclusion, whatever good effects were produced by the means employed, that the anæsthetic, as the operation was afterwards performed without chloroform, must have been considered unsafe to the patient.¹

Professor Miller, of this University, considers the suspended respiration in such cases to depend upon "spasm of the glottis," and he recommends opening the external jugular vein, if there be no bleeding wound, along with "hooking up the epiglottis." He says: "With every care, there is a chance of asphyxia threatening from another cause, during the passage into deep stupor, or during emergence from it; I mean (says he) spasm of the glottis. The remedy, however, is simple. Force the finger into the throat, and hook up the epiglottis with its point; have artificial respiration maintained by thoracic pressure, and if there be no bleeding wound, open the jugular."² Professor Miller must be deceived in pronouncing this state of the glottis, or rather epiglottis, as spasm; it must be paralysis of the epiglottis, from the overdose of the anæsthetic.

The opening of the external jugular vein appears only to be applicable to cases of decided asphyxia, and even in such will scarcely ever be required, as, nine cases out of ten, there is a bleeding wound. A simple respiratory obstruction, as that occasioned by palsy of the epiglottis, or spasm of the glottis, may be produced by an erect and elongated state of the uvula peculiar to some persons. Most practitioners must be familiar with instances of suddenly-threatened suffocation from such a cause. The application, in mild cases, of a stimulus or escharotic is sufficient to contract the organ; but obstinate elongations of the uvula occasionally occur, which require to be removed by the scissors. A gentleman lately consulted me, who held the uvula between his teeth, and would not open his mouth until he wrote down an account of his sufferings. Perceiving at once the nature of his complaint, I seized the elongated uvula with a pair of toothed forceps, and clipped it off close to the velum.

In addition to these means, all of which can be without loss of time and almost simultaneously employed—for, without promptitude in their application, the patient will be irrecoverably lost—I would suggest the following processes for stimulating to renewed action the paralyzed organ of the circulation.

When we see fatal results occurring without any previous form of disease, and the heart with vain efforts struggling to resume its powers, no means should be left unattempted to assist and revive its prostrated vital action. And from the beneficial results which, I have been informed, have been produced by the introduction of diffusible stimulants into the stomach, by the stomach-pump, and by venous injection in analogous conditions of the body, viz: in cholera asphyxia, which in many particulars resembles the poisonous collapse caused by chloroform, I have been induced to request a place in your Journal for this communication, in order that the proposals may be brought, through its widely circulated columns, under the notice of the profession. The first proposal I have to offer was suggested to me by a friend who had considerable experience in the treatment of cholera. He informs me that his usual practice with pa-

¹ Dr. Storer, of Boston, U. S., informs me that the use of chloroform, from its pernicious effects, has been long abandoned in that city, and that when an anæsthetic is required, sulphuric ether has been substituted.

² See "Miller's Principles of Surgery," p. 765, 1850.

tients of sober habits, affected with malignant cholera, who in a few hours were reduced from good health to a state of collapse, with the blue face, cold extremities, and without pulse, to pump into the stomach a large quantity of hot water mixed with spirits, and to administer large enemata of hot water and turpentine. The influence of the stimulants is rapidly diffused through the system, and he has had the satisfaction of restoring circulation and heat of the body on many occasions when the powers of life were all but exhausted. He thinks the same benefit might attend the employment of the practice in the hopeless condition to which a person is reduced by an overdose of chloroform; and he justly adds, it can be safely and speedily resorted to, as the stomach can at once and without danger be reached after the powers of deglutition are gone, and when the introduction of fluids into the fauces is, not to say hazardous in the extreme, but impracticable. In illustration of the practical bearing of my friend's remarks, I may refer to Mr. Lloyd's case of aneurism under operation in St. Bartholomew's Hospital, March 17, 1852, in which chloroform proved rapidly fatal. The warm bath and artificial respiration were used with "no avail." The resuscitating measures had been continued for more than an hour, ammonia had been applied to the nostrils, but no attempt was made to introduce stimuli into the stomach, as Mr. Lloyd feared any liquid placed in the mouth might pass into the larynx, and occasion instant suffocation. I may here ask: How is the stomach of the drunkard cleansed, when his life is in jeopardy?

The influence of a stimulus in the stomach is almost immediately imparted to the cerebrum. It is well known that individuals employed in bottling spirits, from the inhalation of the spirituous evaporation, suddenly fall down in a state of insensibility, an effect which seldom, if ever, happens, if a glass of the stimulant be taken previously to commencing the operation. When all other means fail, I would try, as a last resource, injecting the veins with a saline solution, a practice so successfully employed, in 1832, by the late Dr. Latta, of Leith, Dr. Sibson, now of London, and Dr. Robertson, of the First Royals. Delpech justly observed, that the blood in cholera "ceases to be arterialized;" and the state of the blood of a patient labouring under this disease is nearly similar to that in an individual rendered insensible by chloroform, the symptoms in both cases being a deathlike countenance, imperceptible breathing, and a pulseless circulation. In such a state, "*melius anceps quam nullum remedium.*" In my treatise on cholera asphyxia, at page 73, I have stated: "When the patient is unable to drink, decomposition of the blood has either commenced, or is on the verge of undergoing that process, and, in such a desperate state, nothing can save the patient but injecting the veins with a saline solution. It should be done as follows: Let a solution be properly prepared, consisting of 10 lbs. of hot water, at the temperature of 110°, in which is dissolved a drachm and a half of bicarbonate of soda, and half an ounce of the muriate of soda. This solution is to be strained through chamois leather. One of the veins at the bend of the arm is then to be opened, as in phlebotomy, into which is inserted a common injecting-pipe of the dissecting-room, and then the elbow-joint put in a basin of warm water, to exclude the atmospheric air. The common enema apparatus was employed, the tube extremity of which was adapted to the small dissecting-room pipe, and now held under the water in the basin. The syringe was now plunged into the saline solution, and two or three syringefuls pumped out at the extremity of the tube, still held under the warm water. The tube was carefully inserted in the nozzle of the injecting-pipe, and the pumping begun and performed slowly. During the injecting this solution, the dissecting-room pipe should be held by one assistant while another slowly works the syringe, holding it perpendicularly and steadily, watching the extremity of the tube inserted in the injecting-pipe. After a few ounces were injected, the pulse became perceptible; the breathing stronger; the skin warm; the countenance natural; the eye lost its ghastly, sunken appearance; the face became full and plump, and even rosy; old age assumed the expression of youth. The cramps, the restlessness, and the thirst disappeared."¹

¹ See the Investigations regarding Cholera Asphyxia in 1832. By Delpech and Lizars. Second edition.

It is almost unnecessary for me to advise (for the advantage of having the necessary remedial reagents ready, when dangerous symptoms demand their immediate application is so apparent), that the saline solution, and the instruments necessary to be used in the two modes of treating patients which I suggest, labouring under an overdose of chloroform, should be prepared, and always kept at hand in every hospital, whenever an important surgical operation is to be undertaken, and the patient put and kept in an unconscious state, under anæsthetic influence, during its performance.

Although I have not had an opportunity of proving the utility of venous infusion under the circumstances for which I have recommended it, yet I consider it my duty to direct attention to it, the more especially that death from chloroform appears to be on the increase; and it will form a necessary subject of grave deliberation, if the lamentable occurrence of so many fatal cases among persons otherwise in good health cannot be prevented, by the greatest caution having been taken in its administration, and after every possible means have been speedily, actively, and perseveringly employed, to effect resuscitation, whether the use of chloroform as an anæsthetic should not be greatly curtailed.

For a considerable time past I have declined to urge upon parties, before being operated upon, the advantage of taking chloroform, and have lately performed several capital operations, such as lithotomy, without it. Indeed, the operative rule which I have lately adopted in all cases, except when structural diseases in the heart or other vital organs, or in the lips, nostrils, and fauces, render its application unsafe, is to leave the matter entirely in the choice of the patient, who is best able to decide, whether he is willing to submit to the unavoidable pain of the operation, or to be reduced to a state of unconsciousness during its performance.—*Med. Times and Gaz.* Sept. 9, 1854.

64. *Apparent Death.*—On the 24th of December, 1830, M. ANCELON was sent for in great haste to visit a person about eight miles off. On arriving he was informed that the patient, a woman 34 years of age, had been dead for more than half an hour. On examination, no signs of respiration could be detected; the pulse could not be felt, the surface was livid, the belly soft and placid, and the extremities cold. On being informed that the woman had been suffering from uterine hemorrhage for a length of time, and that it had continued up to the moment of her death, M. Ancelon made a vaginal examination. He found in the neck of the uterus the head of a fœtus of four months old; the soft parts were entirely absent. At the moment that he touched the head of the fœtus the midwife thought she perceived a bubble of air at the woman's mouth. Encouraged by this appearance, M. Ancelon proceeded to disembarrass the uterus of its putrefied contents, and in proportion as he advanced in this the woman appeared to be restored to life. The respiration, manifested at first by air-bubbles, which succeeded one another on the lips, and then the circulation became established with extreme slowness. She did not recover her intelligence for more than three days. During this time, frequent injections were employed to clean out the putrid matters from the uterus, and ammonia was administered internally. During convalescence, she suffered much from an immense number of abscesses over the whole body. Eighteen months afterwards, she was delivered of her tenth child.—*Monthly Journ. Med. Sci.* Oct. 1854, from *Bulletin Gén. de Thérap.* Nov. 15.

[This case shows the importance of an accurate acquaintance with the signs of death. Of these the only one on which confidence can be placed, in very recent cases, is cessation of the beatings of the heart as determined by the stethoscope. The other unequivocal signs, such as cadaveric rigidity, putrefaction, &c., do not occur for a longer or shorter time.]

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Radical Cure of Hydrocele without Injections. By A. S. HUDSON, M. D.
—Nearly a year ago, my attention was called to a case of hydrocele of the left side of the scrotum. For three months only palliative treatment was adopted, and by puncture the sac was four times emptied of its serous contents.

The patient, having listened to a lively picture of suffering which a neighbour had sustained while under treatment for this disease at Chicago, became prejudiced against the mode of treatment with irritating injections. He looked upon it with trepidation; and was, therefore, solicitous for some final though less formidable therapeutic plan.

Accordingly he was furnished with the ointment of tart. ant., and directed to courageously apply it to the diseased part until prominent pustules appeared. The first crop covered less than half of the tumid surface. These were allowed to run their course and decline. Then other remaining portions of the surface were topically medicated, and at length eroded. The lowest part of the scrotum resisting, the ointment was here lastly applied.

Altogether ten days or two weeks were consumed in this cautious tentative medication; yet it worked well. The artificial inflammation dipped deep, and caused the two surfaces of the tunic to adhere firmly together; and now for more than six months the patient has enjoyed a *useful* gland and a sound scrotum.

It may be remarked, that two weeks prior to the adoption of the above-mentioned measures, he was treated for inflamed testicle, which, from exposure to atmospheric vicissitudes, followed the last tapping. The reduced inflammation left an enlarged testicle surrounded with a little water. With this state of the parts, the inunction was commenced. The tumid gland lessened with the fluid's disappearance.

STERLING, ILL., Dec. 10, 1854.

Poisoning by Strychnia. By J. J. LESCHER, M. D., of Mount Carmel, Ill.
—On the morning of the 19th of October, my brother, Dr. Geo. Lescher, was requested to visit E. W., a German labourer, aged 30 years, of a powerful frame, at the "Lock of the Grand Rapids," situated on the Indiana bank of the river, two miles from this place. The messenger represented the patient as having been seized violently with a "congestive chill." When my brother reached the "Lock," he found the patient very ill, though not in a condition such as is usually met with in persons suffering from the cold stage of a malignant intermittent. However, as the patient's extremities had been wrapped up in strong mustard cataplasms before my brother saw him, he regarded the singularity of the symptoms as indicative of a partial reaction, and accordingly directed his efforts to establish it fully. As the patient was exceedingly restless, manifesting a constant disposition to convulsions, my brother administered nothing but opiates and brandy, which did not seem to control the tremours in the slightest degree. At the expiration of an hour, a violent convulsion came on, affecting the entire frame, which it was supposed would terminate fatally. I was immediately sent for, and saw him about three and a half hours after he was taken with what they termed a "fit."

When I first saw him, he was lying on his back, with his upper and lower extremities extended and *rigid*; his arms and legs cold; face flushed, and bathed with cold and clammy perspiration; eyes highly injected, and in constant motion, as from seemingly great affright—as, indeed, his mind fully evidenced; pupils contracted; pulse 120, small, regular and *corded*, or *tense* and *strong*. He was making constant motions with his lips, as though he desired drink. His answers to my inquiries were rapid and unconnected, from which, in connection with the rigid state of all the voluntary muscles, and a seeming tendency to episthotonos, I was led to suspect a serious lesion of the brain and spinal marrow—a condition of cerebro-spinal congestion. Having repeatedly treated the patient for serious and grave attacks of intermittent, and taking into consideration the malarious locality in which he then was, I made but few inquiries in regard to the present attack. His employer not being able to give me any definite information relative to the invasion of what he regarded as an aggravated intermittent, under which he had repeatedly seen him suffer, I was thrown off my guard respecting the *tetanic* condition of the muscular system, though I was struck with the *anomalous* symptoms present in the case. I immediately corded the arm; the movement in doing this threw him into a frightful spasm, causing him to elevate his body, whilst he seemed to rest on his heels and the back of his head. As soon as he became somewhat quiet, I abstracted thirty-two ounces of blood. Whilst the blood was flowing, his pulse lessened in frequency and increased in volume. He seemed more quiet, though every attempt to move *threatened a convulsion*. I then gave him—S. quinia gr. xii, et hydrarg. sub. mur. ℥j, mixed with syrup, which he swallowed with *extreme difficulty*. In half an hour he took a like quantity of s. quinia, joined with cal. ℥ss, et s. morph. gr. ½.

I left him then in charge of my brother, and went to the house of the patient's employer, and expressed my doubts in reference to the nature of the patient's disease. He asked me whether the cause of the ailment might be poison. I replied that the symptoms pointed to such a cause; though I supposed, from all the facts which I had elicited, it could not be possible. He then reminded me that he had purchased six grains of strychnia of my partner, Dr. Miller, only a few days before. I asked him what became of it. He said he had placed it on the *mantel-piece* in his dining-room, but, upon looking for it, he could not find it.

My suspicions were at once aroused. I was told that the patient, after eating a *hearty* breakfast, took out of his pocket, in the presence of the family, a paper containing what he supposed to be quinia, and poured a part of the contents into a tumbler containing half a gill of water, and drank it; as part of the powder adhered to the bottom, he added some more water to it, and swallowed what remained. Fifteen minutes after, whilst milking, he fell backwards in a "fit," in which state they conveyed him to the "Lock House."

Upon receiving this clue to the case, I went to the patient, and, upon inquiry, ascertained that he had placed the remainder of the powder into his pocket; and, on searching, found a paper marked "Poison" in his coat-pocket. He assured me that he had taken the "quinia" out of the paper so marked, and observed that he had been taking it for some days past in order to avoid a return of ague. He persisted that he had found it on a shelf in his sleeping-room, and believed it quinia, though its taste differed from any he had used previously. We, however, believed that he had taken it from where his employer had placed it, from motives of *economy*. The family are in the constant habit of keeping quinia about the house, which the labourers knew; and hence E. W.'s filching, under the impression that he would save the expense of a

dose of quinia. Upon weighing the remnant of the powder, there were but two grains left, the patient having taken four grains.

Four hours had now elapsed since swallowing the poison, and the patient had not vomited. I made a mixture of warm water and flour, and, with great difficulty, *drenched* the patient, at short intervals, with about three pints of it, alternating it with a solution of tartar emetic. We next made him swallow the whites of about half a dozen eggs, when free emesis followed. I then left him, with directions that he should continue to take the whites of eggs freely, and also use injections. During the night he was purged copiously, and on the *third* day succeeding the poisoning he came to town, and has been quite well up to this period.

Death caused by Water-melon Seed. By B. ROHRER, M. D., of Germantown, Penna.—Major W. of Columbia, Penna., a man of strong and robust constitution, blessed with digestive organs of great power, was addicted to drinking, and being a natural gormandizer, he would swallow anything placed before him, regardless of the consequences. Fish in particular, a favourite dish of his, he would devour bones and all. During the months of May and June, 1852, he frequently hailed me in the street, stating that he had violent cutting pains in his bowels, but as there was no constitutional disturbance, I simply directed him to take a dose of oil. A few weeks after this, he presented me with the cause of all his troubles; he said he felt something working gradually lower and lower down in the rectum, until at last he could reach it with his fingers and remove it. It was a thin hard bone, 1 inch in length, and $\frac{3}{4}$ inch in width; its edge slightly serrated, and very sharp; whilst it was passing along the rectum, he had frequent discharges of blood; it was supposed to be the side bone of the head of a catfish.

On the 12th of September, upon my return to the office, I found a message from the major. I immediately called, but was informed that he had been complaining all morning of colic, and had just left in the train of cars, to visit his niece some distance in the country. I heard nothing more of him until the 22d, when I was requested to visit him with Dr. Jones, of Bainbridge. When we arrived, he was delirious, muttering to himself—pulse frequent, irregular, small and wiry; abdomen much distended, and tense as a drum; vomiting a vitiated mucus, commixed with bile; extremities cold—death soon followed. Had no evacuations from the 12th up to his death, although all the usual means had been employed.

Post-mortem 36 hours after death.—Abdominal cavity containing a quantity of offensive gas and fecal matter. Gangrene of the ileum near the ileo-cæcal valve, containing 20 water-melon seeds, with the slippery surface destroyed, and adhering firmly to each other, making a strong ball. Also, gangrene of the sigmoid flexure containing over 100 seeds, forming an exceeding hard ball.

Notice on Adipocire. By CHARLES WETHERILL, Ph. D., M. D.—The question as to the formation of fat in the body possesses a physiological interest, and has engaged attention from the earliest period of the science. Scientific men are divided in their opinions whether fat is formed from the nitrogenized or the non-nitrogenized elements of food. The decomposition of sugar, starch, and alcohol, giving rise to compounds having relations with the class of fatty acids, and especially the butyric acid fermentation, by Pelouze and Gelis, inclined belief in the latter origin; while the later experiments of Liebig, Bosppe, Guckelberger, &c. in obtaining the lower members of the series of fatty acids appeared to give colour to a probability that the former

might also conduce to the end. But of the actual formation of such fatty acids as constitute the mass of fat in the body from proteine compounds—if we except the experiments of Blandin (*Comptes Rendus*, Sept. 6, 1847), which have received neither confirmation nor refutation—we have no chemical proof. I have been engaged for a year past upon the study of adipocire, in hopes that it would perhaps throw some light upon this subject; and, after having worked upon it for several months, I received Lehmann's *Lehrbuch*, in which I found a call upon chemists to investigate adipocire for this very reason, and my results may pass for an answer to this call.

I have examined two specimens of adipocire from sheep buried for ten years, two specimens of human adipocire, and a specimen of adipocire from a metacarpal bone of a fossil ox (*Bison Americanus*, Big Bone Lick); palmitic acid has been detected in the fat, thus confirming Heintz's researches on fat.

Experiments have also been tried upon the decomposition that flesh-fibrin (the heart of the ox) undergoes in water, and I have formed artificial adipocire by imitating the circumstances of a body buried in moist ground. This adipocire has also been chemically examined.

The conclusion at which I have arrived, and which is held by many chemists, is, that adipocire proceeds from the fat originally in the body.

For its analysis, and for the experiments upon which this conclusion is founded, I must refer to my paper, which will shortly be published.

DOMESTIC SUMMARY.

Fracture of the Neck of the Humerus, with Dislocation of its Head.—Dr. G. W. NORRIS read before the College of Physicians of Philadelphia, Oct. 4, 1854, the following example of this:—

"T. B., aged 32, was admitted into the Pennsylvania Hospital, October 31, at mid-day. He was under the effects of liquor when admitted, and stated that on the previous night he had received an injury of his shoulder by falling down a flight of stairs. On examination, the soft parts round the left shoulder were found to be very much contused, swollen, and discoloured, by effusion of blood; the extremity was readily movable in any direction, but the slightest motion produced exquisite pain. No perceptible depression was noticed below the acromion; the elbow could be brought close to the side without force, and on raising the arm and moving it for the purpose of examination, the grating of a fracture was felt.

The presence of these symptoms led to the belief that a fracture of the neck of the humerus existed, and the case was treated by means of a pad in the axilla, with a pasteboard splint moulded to the shoulder on the outside of the arm and bound to the body, with the forearm supported by a sling.

This treatment was continued till the 26th of November, the dressing being occasionally changed. On the removal of the dressings at the last-mentioned date, the swelling having by this time almost entirely disappeared, it was discovered that the head of the bone had been thrown from its socket into the axilla. At the first glance, I thought that my original diagnosis had been incorrect, and that, in fact, I had mistaken, and had been treating a dislocation for a fracture, but, upon closer examination, it proved that such was not the case, but that both a fracture and dislocation existed. The appearances presented by the parts at this time were as follows:—

To the eye there was evidently a slight vacuity immediately below the acromion process, though there was not that deep depression which exists in uncomplicated luxation. On rotating the arm, the broken shaft of the humerus was perceived to move under the acromion at the same time that the rounded

head of the bone could be easily felt in the axilla, and the distance of this fragment from the upper end of the shaft, which partially occupied the glenoid cavity, was, as nearly as could be ascertained, from two to two and a half inches. The shoulder was not firmly fixed as is commonly seen in luxation, particularly when the member has been kept quiet, as was the case under examination; but, on the contrary, was easily movable, and when the patient stood erect, the elbow came in contact with the side of the chest. In order to prevent any mistake as to the nature of the accident, I requested my colleagues in the hospital to visit the case, and, after careful separate examinations, they all agreed as to its being one such as I have mentioned, viz: of fracture of the neck of the humerus conjoined with a displacement of its head into the axilla.

In consequence of the comparatively good use of the limb, which the patient seemed likely to attain, and the slight probability there was of the head of the bone ever being replaced, together with the risk of giving rise to severe inflammation about the joint, and consequent firm ankylosis, if any attempts at reduction were tried, it was decided that no efforts to replace it should be made. Gentle frictions to the part with passive motion to the shoulder, after all traces of the great contusion and consequent inflammation which attended upon it had been subdued by a state of rest, was the treatment afterwards resorted to, and the patient left the hospital on the 1st of January, with a strong and useful extremity, but with inability to elevate the arm beyond an obtuse angle from the body.

The dislocation of the head of the humerus which accompanied the fracture of the neck of the bone in the above case, it will be observed, was not at first detected, although the seat of injury was carefully examined. The accompanying swelling and contusion of the soft parts, the distinct crepitus of a fracture felt upon handling the limb, and the other marked symptoms of that accident which it presented, together with the absence of the ordinarily marked ones of luxation, viz: the fixity of the extremity, the hollow at the shoulder below the acromion, and the ease with which the elbow was brought to the side of the body, all caused it to be overlooked. The difficulty of diagnosis in connection with the great rarity of the injury, alone makes the case worthy of notice; but, in addition to this, the proper plan of treatment to be pursued in these accidents, supposing the diagnosis to be made out, does not seem to be well determined by surgeons.

In the professed treatises on Injuries of the Bones, of Sue, Petit, Boyer, Smith, of Dublin, and the recent valuable, but still unfinished work of Malgaigne, no mention is made of luxation, accompanied with fracture of the neck of the humerus, and but few cases of it are to be found detailed in any of our surgical records. Delpach, in his *Chirurgie Clinique* (tom. i. p. 241), furnishes an accurate account of the dissection of an injury of this kind, which was observed by M. Houzelot, but it had not been ascertained during life. Dupuytren has reported a single case which he saw at the Hôtel Dieu, which was not reduced. In 1832, an instance of it was witnessed in the service of Mr. Earle, of London, which was replaced by his dresser a short time after the accident had occurred; and two cases are to be found in the *Journal de Chirurgie* (tom. iv. p. 180), which are very accurately described by Peyrani, neither of which were reduced. In the last edition of Sir Astley Cooper's *Treatise on Dislocations and Fractures of the Joints*, but five cases of a similar nature are noticed. Of these five, the character of the accident was not discovered in three of them, until revealed by *post-mortem* examinations. In a fourth, the fracture was comminuted, and no attempts at reduction were made, and the fifth, which was an injury of six weeks' standing, and observed in Guy's Hospital, in 1834, by Sir Astley himself, in conjunction with Mr. Key, was mistaken for an ordinary luxation, and the fracture was not suspected till after the pulleys had been applied, when, its true nature becoming apparent, all further efforts to reduce it were desisted from.

In an experience of twenty years at the Pennsylvania Hospital, where a very large number of accidents are treated, I have known of but one other instance besides that just given, of the occurrence of this injury. This was observed in my service, in 1842, and in it the head of the bone was happily

reduced by the resident, Dr. Edward Hartshorne, who published an account of the case in the *Medical Examiner*. The patient was a young circus rider, whose shoulder had been injured by a severe blow from a horse; and the head of the bone, in this instance, was thrown forwards under the clavicle. It was reduced without difficulty, by a little manipulation with the hand, while the patient was feeble and relaxed, and a good cure followed.

In regard to the treatment to be pursued in these cases, I have stated that surgeons are not well agreed. Delpech, after inspecting the parts in the patient of Mr. Houzelot, reasons upon it, and concludes that the reduction of the head of the bone in these accidents is impossible. Sir Astley Cooper (p. 427, Lond. 1842), thus expresses himself: 'Extension is of no further use than to bring the broken shaft of the os humeri into the glenoid cavity, where it forms a useful articulation; but no extension, however violent, disturbs the broken head of the bone, for no proper force could bring it into the glenoid cavity of the scapula;' 'let the surgeon do what he will, the head of the bone will probably remain in the axilla, and the upper motions of the arm will be in a considerable degree lost.' Dupuytren says: 'When luxation is accompanied with fracture of the surgical neck, art and nature can do almost nothing.' (*Leçons Orales*, tom. iii. p. 116.) We have seen, however, that in two of the instances I have alluded to, success has followed the efforts made to thrust back the head of the bone into the glenoid cavity, and these should lead us, I think, to make some efforts to accomplish it in all cases when seen early, though, for the reasons already mentioned, I should myself be disinclined to attempt it, except when it is very recent, and then by no other force than such as can be exerted by the pressure of the hand."—*Summary Trans. Coll. Phys.*, Phil., Vol. II. N. S., No. VI.

Laceration of the Perineum and Sphincter Ani during Parturition, cured by Division of the Sphincter, and subsequent Closure of the Perineum by Sutures. By WILLARD PARKER, M. D., Professor of Surgery in the College of Physicians and Surgeons, N. York.

"I was called on the 24th of October, 1849, to see Mrs. M., who had recently been delivered of her first child. Her labour was terminated with instruments, and an extensive laceration of the perineum, involving the sphincter ani, followed their employment. I was called in consultation a day or two after her confinement, to relieve, if possible, by an operation, her unfortunate condition.

On examining the parts, I found the laceration very extensive, extending fairly through the sphincter ani, the edges being widely separated, and the torn ends of the muscle drawn upon either side toward the coccyx. The appearance of the wound resembled that which is produced on dividing the orbicularis oris, the edges of the wound being drawn widely and permanently asunder by the contraction of the muscle.

The appearance of the wound suggested the method of cure. It would be difficult to retain the approximated edges of the lacerated parts of the anus in apposition while the sphincter remained in its present condition, strongly retracting its lacerated edges towards the coccyx. I accordingly proposed to divide the sphincter subcutaneously, but thoroughly, on each side of the coccyx, and then, after trimming the edges of the perineal laceration, approximate the lips of the wound, and retain them by deep clamp or quill sutures. The operation was acceded to by the gentlemen in attendance, and I proceeded at once to operate. The sphincter was thoroughly divided at the points above indicated, and the edges of the wound, pared, were easily approximated and retained by the quill suture. The bowels were confined by opiates for several days, and finally moved with injections of sweet oil. Union readily took place, and the result was most satisfactory.

Remarks.—Rupture of the perineum has been justly considered one of the most serious accidents of parturition, so difficult have these wounds proved of cure. It has, indeed, been advised to leave them wholly to nature, as not susceptible of treatment. The success of the operation in the preceding case, one of the most formidable that can occur, demonstrates the fallacy of such an opinion. These lacerations of the perineum may vary from a simple rupture

of the fourchette to a complete division of the vulvo-anal septum, involving the sphincter ani. This latter is far the most serious, as presenting the greatest obstacles to cure.

Two principal methods of cure have been proposed. The first is, to effect a reunion by rest and cleanliness, without the resort to surgical means; and the second, to promote union by surgical interference. Surgical and obstetrical writers seem to be about equally divided in the adoption of these methods. Among the French, Boyer and Duparcque favour the former, and Dubois and Roux the latter. Mr. Brown, in his recent work,¹ concludes in regard to the opinions of English writers: 'On the whole, the prevalent opinion in England appears to have been that, from the uncertain, and, most frequently, unsuccessful, results of the operations devised, and from the apparently insuperable difficulties to be contended with, it was better merely to aid the efforts of nature in narrowing the wound, and in lessening the evils attendant on it.' The method of treatment by rest and cleanliness is generally all that is required when the laceration is slight, and it is adopted immediately after the accident has occurred. But in those severe cases, when from the use of the forceps or mismanaged labour, the rent extends completely through the sphincter ani, the suture offers the only reliable means of cure.

The different forms of suture used have been the interrupted, the twisted, and the quilled, and of these the latter is generally preferred. Mr. Higginbotham reports a case in the *Lancet*, vol. ii. 1849, in which the laceration extended through the sphincter ani; he used the interrupted suture in two places, and applied the nitrate of silver to the skin along the edge of the wound. 'The wound united by the first intention; the eschar surrounding the laceration, made by the caustic, had the power of fixing the parts as if adhesive plaster had been applied.' The operation was performed thirteen years before the date of the report, and during that time the patient had remained well, and borne nine children without any return of her trouble. Several cases have been reported cured with the harelip pins and twisted suture, but the quilled suture is much more frequently used at present than any other, and is decidedly preferable to those mentioned. In repeating this operation, however, I should prefer to use the clamp suture of Dr. Sims, of this city, which I believe would be admirably adapted to these cases, and for the introduction of which into surgical practice he is deserving of great credit.

There was one step in this operation to which I would call particular attention, as contributing not a little to its success, viz: the complete division of the sphincter ani upon each side of the coccyx. By this simple measure, the tension was entirely taken from the posterior lips of the wound, which had previously been widely separated and drawn outward and backward, and the edges were in consequence readily approximated and easily retained in apposition. The necessity of this step to overcome the principal obstacle to the retention of the opposed surfaces in easy apposition, was impressed upon my mind while contemplating the appearance and anatomy of the parts engaged. The sphincter ani muscle having a strong and unyielding attachment to the coccyx, acted powerfully and constantly in separating its anterior lacerated portion, and, of course, in drawing asunder the rent in the perineum. The remedy for this difficulty very naturally suggested itself. By dividing the coccygeal attachments of this muscle, its action would be destroyed, and its power to prevent the perfect and undisturbed contact of the wound would be neutralized. The result of its division justified this conclusion, for not the slightest difficulty occurred in accomplishing this hitherto formidable task, and obtaining union throughout by the first intention. To relieve the strain upon the sutures, where the perineum is rigid and tense, Dieffenbach made elliptical incisions upon each side of the median line; and recently, Mr. Hilton, of London, has, by subcutaneous incision, divided the coccygeal attachments of the sphincter and levatores ani muscles, for the purpose of changing the direction of their action from the coccyx towards the pubes.

¹ On some of the Diseases of Women admitting of Surgical Treatment. By Isaac Barker Brown, F. R. C. S., &c. London, 1854.

At the time the above operation was performed in 1849, I was not aware that the division of the sphincter had ever been recommended, much less practised in these cases. It appears, however, according to Mr. Brown, that Saucerotte once operated in a similar manner, and Mr. Erichsen states (*Science and Art of Surgery*) that it was advised by Copeland, and approved by Bransby Cooper. Within the last year or two, Mr. Brown, of London, has operated in this manner very successfully, and in his work, above referred to, the different steps of the operation are finely and accurately illustrated by engravings. He is a warm advocate of the operation thus modified, and reports eighteen cases thus successfully treated.

It will be observed that my operation was by subcutaneous incision, while that performed by Mr. Brown involves the external, as well as internal parts. In regard to the subcutaneous incision, he remarks [p. 41]: 'A subcutaneous incision of the sphincter has been suggested, but it will not furnish the results aimed at. The muscular fibres of the sphincter must be completely severed, and also the investing integument, to annihilate all traction.' This objection is without foundation, as the puckered, folded integument of the anus, when once the sphincter is divided, is capable of indefinite extension, and exerts not the slightest traction upon the flaps of the wound. As to the results which subcutaneous incision furnish, we need but refer to the above case to prove that they are satisfactory."—*New York Journ. of Med.* Nov. 1854.

Treatment of Ulcers by Anaplasty.—In a paper published in the *New York Journ. of Med.* for September last, Prof. F. H. HAMILTON, of Buffalo, reports a case of ulcer on the leg cured by an anaplastic operation performed by him in January last, and claims to have proposed this method of treating ulcers as long ago as 1846; and to have at that date, in his clinic at Geneva Medical College, "anticipated most of the results" which he has "now actually obtained."

In the succeeding number (Nov.) Dr. JOHN WATSON, of New York, states that he has anticipated Dr. Hamilton both in his proposal and in his results, on at least two occasions, once in April and once in July, 1844.

"The first of these operations," he remarks, "was for the cure of an old ulcer on the forehead, the result of syphilis, and complicated with necrosis and exfoliation of nearly the whole external table of the os frontis. A report of it appeared in the *American Journal of Medical Sciences* for October, 1844 (page 537); and the details of it, under the head of 'Melopoplasty, or Forehead-mending,' were again published in the first volume (page 711) of Townsend's translation of *Velpéau's Surgery*, which appeared in this city in 1845.

In this case, after vivifying the edges of the ulcer with the knife, and quickening its surface with a wash of concentrated aqua ammonia, I succeeded in covering the diseased surface by means of three large flaps; one of which was taken from the integuments of the temple on either side of the ulcer, and the other from the scalp above it. The recovery was rapid and complete. The patient afterwards officiated for some years as a nurse at the New York Hospital. The improvement in his appearance effected by the operation, was beyond all expectation. To the observer standing at a little distance from him, the lines of cicatrization on the forehead were barely visible; and his elevated and open countenance gave hardly any traces of its former loathsomeness and deformity.

Since this first operation, I have had but one occasion to recommend a repetition of it on the forehead. But in that instance, the patient died of an intercurrent and acute disease, before the operation was attempted. My second operation of the sort was for the cure of an ulcer on the foot. I cannot at present refer to any printed notice of it. But the case, at the time, was much talked about, at the Hospital and elsewhere, and has often been the theme of conversation since. I give the particulars of it from my own note-book. It is also recorded in the N. Y. Hospital Case-Books (First Surgical Department, vol. iii. case 50, page 291).

G. C., a young English seaman, was admitted by an order from the British consul, in or about the year 1834, having, by exposure to severe weather at

sea. lost the greater portion of his left foot, and all beyond the tarsal bones of the other foot. The injury, at the date of his admission, had been of several months' duration. The ulcerated surfaces left by the sloughs had already partially cicatrized. But the loss of substance was so great that the patient could no longer use his feet in walking, and was obliged to move about upon his knees. After remaining for some months under treatment, the cicatrices becoming firm, he was supplied with an apparatus for each foot, somewhat after the manner of a Scarpaso shoe, having a firm wooden portion in front to supply the lost portion of the foot. By means of this apparatus, and subsequent modifications of it, which were the result of his own ingenuity, he was soon able to walk about upon his feet; and, being an active and intelligent man, he was appointed nurse in one of the surgical wards, in which capacity he still continues at the Hospital.

But, from time to time, the pressure of the shoes gave rise to much inconvenience, and occasionally to ulceration on the face of the stumps; obliging him at intervals to lay aside his shoes, and take to his knees again in walking. He had been for some months in this condition, in the early part of 1844. The principal uneasiness at that time was from a callous ulcer on the stump of the right foot. The annoyance from this was such as to compel him, for a season, to remain in bed. After more than two months' confinement in this way, failing to effect a cure by milder means, I proposed to him an anaplastic operation. To this he readily consented, and it was accordingly performed on the 30th of July, 1844. It consisted in carrying the point of a bistoury through the callous integument completely around the circumference of the sore, and at about the distance of a line from its outer border; in shaving off the surface of the sore, and quickening the whole of it, up to this line of circumvallation; in dissecting up a flap of integument from the top of the foot, and by the twisting and gliding process, bringing this down and securing it by sutures over the surface of the ulcer, so as to leave no raw surface exposed; and finally, in approximating the integuments at the gap, on the top of the foot, from which the flap had been taken.

The operation was a painful one, and gave rise to considerable inflammation. A narrow border of integument along the lower edge of the flap, sloughed away; and for a day or two the case seemed to threaten a disastrous result. But the inflammation subsiding, the reparative process soon followed; and, in less than three weeks from the day of the operation, the line of cicatrization was completed, and the ulcer closed. The patient, however, did not immediately afterwards resume the use of the shoes. But in the following November he was again about the house on his feet, and attending to his usual occupation. Since that period, his right foot has given little or no uneasiness, but he has at times some trouble from tenderness in the other foot.

In connection with these cases, I might refer to several others of more or less importance, in which similar anaplastic measures have been adopted at the Hospital by my colleagues and myself, for the removal of deformities, the closing of fistulous ulcers, in the covering of ulcerated stumps. But proceedings of this sort are sufficiently common. They are to be looked upon only as special applications of the already well-established rules of anaplastic surgery; and in this light they have no claim to be spoken of as novelties.

The only marked differences of treatment between the case reported by Dr. Hamilton, and those spoken of above, so far at least as principles are involved, are that in his the flap was taken from the opposite limb, and in mine, from the immediate vicinity of the ulcer. He used the knife in quickening the ulcerated surface; whilst in one of my cases the knife was used, and in one, a wash of concentrated aqua ammonia. I do not see the force of the argument in favour of taking the flap from the opposite limb, so long, at least, as available integument can be procured near by. The difficulty of transplanting a flap from a remote point is hardly to be over-estimated; and when from a different limb, the restraint to which the patient must be subjected during the process of adhesion is, to make the best of it, frightful; and never to be imposed upon any one, except under circumstances of the most urgent necessity.

But so far as the mere closing of simple ulcers is involved, thanks to the

elasticity and yielding power of the skin, the anaplastic method is rarely necessary. In those of moderate size, and in some, too, of immense size, nature does the work of reparation with very little assistance, provided, always, that this little be judicious. And on those, again, of greater size—such, for example, as result from extensive burns and scalds, or from diffuse gangrenous erysipelas—a flap of sufficient dimensions to cover them could hardly be removed with safety from any part of the body.”

Rupture of the Uterus; Gastrotomy.—Dr. Mason read before the Philadelphia College of Physicians (Sept. 6, 1854) the following interesting account of a case of rupture of the uterus, in which the operation of gastrotomy was successfully performed by Dr. JOHN NEILL.

“Monday, 24th July, called to Mrs. John McDevitt, South above Twentieth Street, in labour with her sixth child; reached her at 9 P. M.; had been in active labour for about an hour; of a florid complexion, somewhat fleshy, large muscular development, with all the appearance of possessing a constitution of more than ordinary strength and vigour.

Upon examination, found the os uteri about half open, membranes presenting unbroken, the head to be felt high up above the superior strait; the pains were good, but by no means violent, with distinct remissions of five or six minutes; left her, and returning in about an hour, found the os uteri fully open, pains somewhat stronger, but still the presenting part did not descend. I now ruptured the membranes in the expectation that, as there was neither contraction nor rigidity, the head would come down into the pelvis without further delay. In this I was disappointed, and it was evident that the head had some difficulty in entering the superior strait. Still it advanced a little, and I thought I could detect the anterior fontanel looking towards the left acetabulum, giving the fourth position of Baudelocque. I was however by no means certain on this point, but resolved to wait. I made a visit in the next street, and returned to the patient in less than half an hour; the pains were now much stronger, and I thought that the head had advanced slightly. At this time Mrs. M. was obliged to get up, for the purpose of relieving her bowels, and I went down stairs, still without the slightest anxiety as to the result of the labour, for the spirits were good, the countenance cheerful, the woman well formed and vigorous, and a state of active labour had not existed for more than two hours and a half. While at stool, the patient had two pains; during the latter she suddenly complained of intense agony, with a burning sensation in the right side; the woman hurried her to bed, and called me into the room; I found her on her back in great torture, which she assured me was no longer the pain of labour, but that something had gone wrong inside of her.

I examined her pulse and found it but little altered; this, added to the circumstance of there being neither vomiting nor cold clammy skin, nor anything like an approach to syncope, made me hope that matters were not so bad as I had at first apprehended; but after administering some forty drops of laudanum, using hot fomentations, and waiting for some time, finding that the uterine contractions were completely suspended, that the presenting part had receded, and that there was a sanguineous discharge, though not profuse, from the vagina, I felt convinced that the uterus was ruptured.

Before, however, proposing any operation, I called upon Dr. Hollingsworth for his advice and assistance. He immediately came in the kindest manner, and, after careful investigation, the diagnosis was distinctly made out. The placenta had *not* passed into the cavity of the abdomen, for it could be distinctly felt with the cord passing from it. The head of the child could be detected through the abdominal parietes occupying the lower part of the abdomen on the right side, near the inguinal region, but no portion of it remained in the uterus.

Under these desperate circumstances, we deliberated with sad forebodings on the treatment to be adopted, in order, if possible, not to lessen the little chance of life remaining. I am aware that all the best authorities recommend introducing the hand through the torn womb, into the bowels of the victim, seizing the feet, and first dragging the infant back into the womb, and from

thence, *per vias naturales*, into the world. They say that it gives the child a better chance; it may be so, I am not prepared to dispute that point, for I thank God I have had no experience in such a procedure; but this I do know, that the description of the operation has always filled me with unutterable horror. Another thing, which I am bound to confess, though it may appear very unprofessional and unnatural to some, is, that I never thought of the child or its life, or anything about it, except to wish, that as it had pleased God to place it there, it would please him, in his infinite mercy, to assist me in getting it away, without tearing the poor devoted woman to pieces, and entailing upon myself the terrible conviction, that I had made almost certain death a certainty.

After due consideration, however, we determined to explain the nature of the necessary operation, by turning to the patient, and propose it as a dernier resort. This we did, but she absolutely refused to submit to it; and, from the hydrocephalic condition of the head, afterwards ascertained, we had reason to be thankful that she did so. At this time there was no vomiting, the expression of her countenance was good, the pulse firm, and the skin natural; the pain in the abdomen, at first very severe, had now much abated; and after administering a powerful dose of morphia we left her, determining to see her in the morning, and then be guided by circumstances.

Next morning, the 25th, when Dr. Hollingsworth and myself visited her, we found her much better than we anticipated; pulse firm and strong, countenance bright, and mind unclouded; longer to leave her undelivered was out of the question; professional duty and common humanity alike demanded that an effort, however desperate, should be made to save her. We therefore determined to propose gastrotomy, as that operation, in our opinion, afforded her the best chance. To this, encouraged by feeling better than she anticipated, she at last consented; and, after consulting Dr. Neill, who undertook the performance of the operation, it was determined on.

Operation by Dr. Neill.—The patient was placed upon a stout table covered with blankets, her shoulders and head supported by pillows; and, as a preliminary step, about four ounces of ether were administered by inhalation. The incision was made in the linea alba, commencing about two inches below the umbilicus, and extending towards the pubis full six inches. The moment the opening was made, large quantities of mingled blood and clots escaped, the omentum seeming saturated with blood, and both the visceral and parietal peritoneum being deeply stained. A dead child's back presented, its head lying low down towards the right groin, its feet to the left. It was immediately removed, and found to be hydrocephalic; the bi-parietal diameter of the head measuring, I should suppose, six inches, the occipito frontal probably seven. Its entire weight I should judge to be not less than ten pounds.

The rent in the uterus appeared to be enormous, and perfectly uncontracted, for the operator passed both hands through it, right down into the organ, and, as it were, scooped up the placenta, with all the coagula within his reach. Upon the removal of his hands, the womb instantly contracted to about the size of a man's fist. The blood, fluid as well as coagulated, was then removed from the cavity of the abdomen as far as practicable, disturbing the viscera as little as possible. The incision was then closed by five sutures, and afterwards by long adhesive straps, leaving an opening at the lower part of the wound to favour the escape of fluids; a compress and binder completed the arrangement.

The patient's strength was less exhausted than could have been anticipated; spirits good; pulse 120—firm and equal. The time occupied by the whole operation did not, I should think, exceed five minutes. In half an hour she was placed in bed, and an enema of laudanum administered; grain doses of opium were directed to be given by the mouth every three hours, in order to keep her, if possible, in a perfect state of repose, and prevent any action of the abdominal viscera; at the same time the system was supported by nourishing fluids, beef tea, &c. Shortly after the operation, the patient began to vomit a greenish watery fluid, which continued several hours, but was checked by the exhibition of small quantities of brandy with ice; the opium treatment seemed to agree with her, for she slept, and complained of but little pain.

On the morning of the 26th, I visited her in company with Dr. Hollingsworth. Found her tolerably easy; mind cheerful; tongue clean and moist; pulse 120; the abdomen was tympanitic, and very much distended; the breathing much embarrassed by the accumulation of gas. Ordered her to continue the opium pills, and to have an injection containing turpentine. On making my evening visit I learned that the bowels had been slightly opened, and that she had passed large quantities of flatus, by which the tympanitic distension of the abdomen was much lessened; breathing easy and natural; pulse rapid and weak. Directed brandy to be continued with the opium.

27th and 28th.—Continued in much the same condition—occasionally vomiting.

On the 29th, Dr. Neill visited her with Dr. Hollingsworth and myself. Removed the stitches from the wound, which was healthy and closing extremely well; she was now ordered milk punch *ad libitum*. At this time there was a very copious, dark, offensive discharge from the vagina, which was kept continually syringed with warm water and soap. The bowels had been moved once copiously; pulse 120; tongue moist, but slightly furred. The patient looked so hopeful and strong, that we began to feel encouraged.

On the morning of the 30th, I understood that she had passed a restless night. Looked very much worse; the lips were pale; countenance dejected; pulse 130; vomiting of green matter without effort—in fact a regurgitation of the fluids contained in the stomach. I began to lose hope. Still her mind never wavered—day nor night—and when spoken to she replied quickly and clearly, but without anything like an unnatural elevation, a condition which I have sometimes observed in bad cases of uterine phlebitis. The discharge from the vagina was less copious and less offensive. When I saw her in the evening, she was labouring under the worst possible symptoms; so much so, indeed, that I thought it possible she might die before the morning. Her pulse was from 135 to 140—very weak; her feet and legs were cold, also the lower part of the belly; her wrist and arms to the shoulders in the same condition, and bedewed with a clammy sweat.

I confess I regarded her as moribund, and the priest in attendance told her that she was dying, and must make her peace with God; the poor woman replied that she would make her peace with God most willingly, but that the Revd. Father was wrong, that she was not dying yet, she did not feel like dying. And as it proved, she was right, for the next morning, the 31st, I was agreeably surprised to find that her skin had regained its natural temperature, that her strength had improved, and that she was altogether better than on the previous day. There was no pain on pressure of the abdomen, but still there was considerable tympanites, and the pulse continued at 135.

On the first of August, the vomiting continued, but only occasionally.

On the 2d, vomiting had ceased entirely. I watched with great anxiety for a diminution in the frequency of the pulse, as indicating some favourable change, but as yet in vain. Notwithstanding the steady pursuance of the opiate treatment, the patient's bowels, on the 1st, were largely opened, three or four times. She complained of great pain, before each evacuation: opiate enemata checked this, and on the 2d she had but one stool, perfectly natural in colour, and of the consistence ordinarily produced by a dose of castor oil.

August 3. Pulse somewhat slower—about 120; dressed the wound in the abdomen; did not think it looked quite so well; some discharge from one of the suture openings: was suffering from great uneasiness of the bowels, they having been opened several times; before each movement, considerable pain was complained of, somewhat resembling the termina of dysentery; colour perfectly natural. In the evening, there again appeared great coldness of the extremities. Ordered an enema of starch and laudanum, with hot bricks to the legs and feet, brandy and milk to be given freely.

On the 4th, found the patient warm; pulse 130; tongue foul; complaining of great pain in the bowels, which had been moved several times during the night; the breathing high and laboured. Both Dr. Neill and myself thought her prospect of recovery worse than usual. Dr. Hollingsworth had left town, and I was obliged to be absent from the city for some hours; Dr. Neill, there-

fore, undertook to see her for me. In the evening, I found her symptoms the same as in the morning. Ordered her a large teaspoonful of laudanum, as an injection, and desired the attendant to give her all the nourishment she could take, with a continuance of the brandy and milk.

Saw her, with Dr. Neill, on the morning of the 5th. Breathing decidedly improved; countenance and spirits better, though the pulse was weak and continued at 130; tongue cleaning; dressed the abdominal wound, which looked much healthier, though there was considerable discharge from another of the suture wounds; had passed a comfortable night, slept well, and had had no pain nor trouble with her bowels; but there had been a considerable discharge from the womb, described by the nurse as being of a clear red colour, and devoid of smell. At half-past nine in the evening, saw her again. Condition unchanged; pulse the same; uterine discharge still copious; bowels had been opened once.

On the 6th, found her much improved; pulse 120; tongue clean; expression of face natural; heat of skin almost natural, with very little thirst; the abdominal wound nearly healed. In the evening, found her easy, but showing more weakness. This I attributed to the uterine discharge, which continued copious.

Morning of the 7th, stronger and better; pulse 110; discharge from the womb much lessened; had slept soundly all night. At ten o'clock, evening of the same day, great change had taken place; pulse 100, firm and steady; uterine discharge nearly suppressed; had taken her food regularly, and with appetite; bowels had been opened once naturally during the day.

From this time, she improved so rapidly that, on the 15th, she came down stairs; on the 24th, just a month from the time of the rupture, was at the washtub; and, on the 2d of September, I met her in the street, when she told me she felt as well as she did before the accident."—*Summary of Trans. Coll. of Phys., Phil., Vol. II. N. S., No. VI.*

Ovariectomy successfully performed.—Dr. T. F. CRAIG reports (*New Orleans Med. News and Hospital Gazette*, Dec. 1, 1854) a case of this, performed by Dr. John Craig, of Stanford, Kentucky. The subject of it was 26 years of age, the mother of one child. The operation was performed on the 22d April, 1854. Sulphuric ether having been previously administered, an explorative incision was made, three inches long, between the pubis and umbilicus; after carefully dissecting down to the peritoneum, this was opened, giving exit to about twenty ounces of serum, and leaving exposed the firm, irregular, silvery looking tumour. The exploring finger could now detect adhesion of the tumour to the peritoneum in the left lumbar region; no other being found after a careful examination, it was decided to extend the incision to the pubis and umbilicus. At this time, about a pint of serum again escaped.

A large trocar was passed into one of the sacs of the tumour, but the contents were so thick that none passed through the canula. The incision in the abdomen was then enlarged, being made to extend to the pit of the stomach. The omentum was found extensively adherent to the superior surface of the tumour, and it was necessary to dissect it carefully loose. One-half of the tumour now protruded through the opening, and, being large and unwieldy, three of its largest sacs were opened, and their contents evacuated. The mass was then raised from its bed, when it was found to be adherent to about fifteen inches of the small intestine; this was dissected loose, and the tumour turned out through the incision, but left attached by its pedicle, which consisted of the left broad ligament and the Fallopian tube. At this juncture, the patient's pulse failed; her face assumed a death-like aspect, and she gasped as if expiring. By the prompt application of stimulants and excitants, she was restored. Her dangerous symptoms were attributed to the removal of the pressure caused by the tumour, and the substitution of chloroform for the ether.

The tumour was now raised by Dr. Craig sufficiently to allow Dr. Pierce to pass a needle armed with a double ligature through the base of the pedicle; the threads were separated near the needle, tightly secured on each side of the pedicle, and the tumour removed. The ends of the ligatures were secured in the lower angle of the incision. No bleeding vessels were found on an exami-

nation, and the edges of the wound were brought together and maintained by means of the interrupted suture and adhesive plaster. Cold water dressings were applied, and a bandage placed round the abdomen, the patient put to bed, and forty drops of laudanum administered.

The time occupied by the operation was forty minutes. The intestines caused no trouble during the operation, lying quietly in the position in which they were found when the abdomen was laid open. Two hours after the operation, the pulse was 108, and feeble; the surface cool, and a sensation of sinking experienced. Warm gruel, with one-fourth grain sulph. morphine was prescribed. Six hours after, the patient was more comfortable; morphine repeated. In ten hours, pulse the same; tendency to sleep.

On the 23d, at 8 o'clock A. M., patient comfortable; had several refreshing naps through the night; urine drawn off before day; pulse 128; some appetite; slight hemorrhage from wound; dressings allowed to remain. At 12 o'clock M., pulse 135; urine has been drawn off twice; abdomen tympanitic; has taken one-fourth grain sulph. morphia every six hours. At 6 P. M., pulse 140; abdomen still tympanitic; no pain; tongue coated and brown; slight nausea; prescribed light diet; continued morphia.

On the 24th, at 12 o'clock M., pulse 100; no thirst or nausea; tongue beginning to clean; considerable appetite; slept well during the night; feels much better; dressings removed; wound healthy; adhesion progressing. From this time forward, no unfavourable symptoms occurred.

She continued, from this time, to improve; on the fifth week, one of the ligatures applied to the pedicle came away, and soon afterwards the other. Four months after the operation, her health appeared to be perfectly restored.

The tumour weighed eleven and three-fourth pounds after its fluid contents were discharged. It was what is called multilocular, and consisted of five large cysts and numerous smaller ones. It appears to have commenced in the left ovary, and one section presented numerous fibrous septa, with a highly injected glandular looking substance intervening. The largest cyst appears to have commenced immediately beneath the proper covering of the ovary. The contained fluid was viscid, and had floating in it large masses of fat. On microscopic examination, quantities of oil globules were found, together with simple isolated cells filled with granules, and large plates of cholesterine crystals. The tumour contained but little blood.

Vesico-Vaginal Fistula.—Dr. GURDON BUCK records (*New York Medical Times*, Oct. 1854) a case of vesico-vaginal fistula, successfully treated by the method devised by Dr. J. Marion Sims, and described in the number of this Journal for Jan. 1852.

Another case of the same distressing lesion, treated with like success by the same method, by Prof. CHAS. A. POPE, of St. Louis, is recorded in the *St. Louis Med. and Surg. Journ.* for Sept. 1854.

Both surgeons accord due praise to Dr. Sims for his ingenious and improved method of treating this deplorable lesion, which was formerly an opprobrium of surgery.

Prof. Pope remarks: "Too much praise cannot be awarded Dr. Sims for his persevering and ingenious efforts in this department. Indeed, there is no achievement in American surgery, or in the surgery of any age or country, which deserves higher regard than this; and when we reflect on the frequency of such cases, and on the sad fate of those patients who, until within comparatively a few years, were abandoned to their hopeless condition, we have reason to claim for our country and art a vast boon to humanity. Under no circumstances are patients more grateful than when relieved of this loathsome and painful state."

THE
AMERICAN JOURNAL
OF THE MEDICAL SCIENCES
FOR APRIL 1855.

CONTRIBUTORS TO THIS VOLUME.

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- ROBERT BRIDGES, M. D., *Professor of Chemistry in the Philadelphia College of Pharmacy.*
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- WM. RANKIN, M. D., of Shippensburg, Pennsylvania.
- F. M. ROBERTSON, M. D., *Lecturer on Obstetrics in the Charleston Summer Institute.*
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- RICHARD M'SHERRY, M. D., of Baltimore.
- HENRY H. SMITH, M. D., *one of the Surgeons to St. Joseph's Hospital.*
- MARK STEPHENSON, M. D., *one of the Surgeons to the New York Ophthalmic Hospital.*
- ALFRED STILLE, M. D., *Professor of the Practice of Medicine in the Medical Department of Pennsylvania College.*
- E. WARREN, M. D.
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TO READERS AND CORRESPONDENTS.

THE following works have been received:—

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XXXVII. London, 1854. (From the Society.)

What to Observe at the Bedside and after Death in Medical Cases. Published under the authority of the London Medical Society of Observation. Second American from the second and enlarged London edition. Philadelphia: Blanchard & Lea, 1855. (From the Publishers.)

The Principles and Practice of Obstetric Medicine and Surgery in reference to the Process of Parturition, with sixty-four plates and numerous wood-cuts. By FRANCIS H. RAMSBOTHAM, M. D., Fellow of the Royal College of Physicians of London, Obstetric Physician to the London Hospital, etc. etc. A new American edition, revised by the Author. With notes and additions, by WILLIAM V. KEATING, M. D., A. M., Lecturer on Obstetrics and Diseases of Women in the Philadelphia Medical Association, Physician to St. Joseph's Hospital, etc. etc. Philadelphia: Blanchard & Lea, 1855. (From the Publishers.)

A Practical Treatise on the Diseases peculiar to Women. Illustrated by cases derived from Hospital and Private Practice. By SAMUEL ASHWELL, M. D., M. R. C. of P. L., and late Obstetric Physician and Lecturer to Guy's Hospital. Third American from the third and revised London edition. Philadelphia: Blanchard & Lea, 1855. (From the Publishers.)

A Treatise on the Practice of Medicine. By GEO. B. WOOD, M. D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania, President of the College of Physicians of Philadelphia, etc. etc. Fourth edition. Philadelphia: Lippincott, Grambo, & Co., 1855. (From the Author.)

Outlines of Chemical Analysis Prepared for the Chemical Laboratory at Giessen. By Dr. HEINRICH WILL, Professor of Experimental Chemistry in the University at Giessen. Translated from the third German edition, by DANIEL BREED, of the U. S. Patent Office, etc., and LEWIS H. STEINER, M. A., M. D., Professor of Chemistry in the National Medical College. Boston and Cambridge: James Munroe & Co., 1855. (From the Publishers.)

Autobiography of Charles Caldwell, M. D. With a Preface, Notes, and Appendix. By HARRIOT W. WARNER. Philadelphia: Lippincott, Grambo, & Co., 1855. (From the Publishers.)

Illustrated Manual of Operative Surgery and Surgical Anatomy. By MM. CH. BERNARD, D. M. P., and CH. HUETTE. Edited, with notes and additions, and adapted to the use of the American Medical Student, by W. H. VAN BUREN, M. D., Professor of Anatomy in the University Medical College, etc., and C. E. ISAACS, M. D., Demonstrator of Anatomy in the University Medical College, etc. Illustrated with steel engravings, from drawings after nature, by M. J. LEVEILLE. Designed to serve as a companion to the ordinary Text-books of Surgery. New York and London: H. Bailliere. Paris: J. B. Bailliere, 1855. (From the Publishers.)

The Medicinal, Poisonous, and Dietetic Properties of the Cryptogamic Plants of the United States. Being a report made to the American Medical Association at its Sessions, held in Richmond, Va., and St. Louis, Mo. By FRANCIS PEYRE PORCHER, M. D., etc. From the Transactions of the American Medical Association, Vol. VII. (From the Author.)

Report on the Epidemics of Ohio, Indiana, and Michigan, for the years 1852 and 1853. By GEO. MENDENHALL, M. D. (From the Author.)

Medicine a Scheme or Disease a Unit. By H. BACKUS. Selma, Ala., 1855. (From the Author.)

Puerperal Fever as a Private Pestilence. By OLIVER WENDELL HOLMES, M. D., Parkman Professor of Anatomy and Physiology in Harvard University. Boston: Ticknor & Fields, 1855.

Report of the Board of Health of the City of Washington for the year ending June 30, 1854.

Excision of the Head of the Femur and Removal of the Upper Rim of the Acetabulum for Morbus Coxarius. By LEWIS A. SAYRE, Surgeon to Bellevue Hospital, etc. etc. (From the New York Journal of Medicine.) New York, 1854. (From the Author.)

History and Observations on Asiatic Cholera in Brooklyn, N. Y., in 1854. By JOSEPH C. HUTCHISON, M. D., late Physician to the Brooklyn Cholera Hospital, etc. (From the New York Journal of Medicine.) New York, 1855. (From the Author.)

Report of the Board of Health of the City of Brooklyn to the Common Council, December, 1854. Brooklyn, 1854. (From Joseph C. Hutchison, M. D.)

Second Annual Report of the Surgeons of the New York Ophthalmic Hospital for the year 1854. New York, 1855.

Report of the Inspectors of the Western Penitentiary of Pennsylvania for 1854. Pittsburg, 1855. (From Dr. Pollock.)

Physician's Report on closing the Roper Hospital after the Yellow Fever Epidemic of 1854. By WM. S. WRAGG, M. D., Physician to the Hospital. Charleston, 1854.

Sanitary Reports of the City of Buffalo for the year 1854. Printed by order of the Common Council. Buffalo, 1855.

Report of the Sanitary Commission of New Orleans on the Epidemic Yellow Fever of 1853. Published by authority of the City Councils of New Orleans. New Orleans, 1854. (From Dr. Barton.)

Lectures in reply to the Croonian Lectures for 1854, of CHARLES WEST, of London, on the Pathological Importance of Ulceration of the Os Uteri. By HENRY MILLER, M. D., Professor of Obstetric Medicine in the University of Louisville, etc. Extracted from the Western Journal of Medicine and Surgery. Louisville, 1855.

The Relative Merits of Empiricism and Rationalism in the present state of Medical Science. By ALEXANDER HENRY, M. D. Read before the Medical Society of London, March 25, 1854. Reprinted from the Association Medical Journal, 1854. (From the Author.)

A Brief Sketch of the History of Lexington, Ky., and of Transylvania University. Delivered as an Introductory Lecture to the Winter Course in the Medical Department of Transylvania University. By ROBERT PETER, M. D., Professor of Chemistry. Lexington, 1854.

Address delivered before the Philadelphia County Medical Society. By THOS. F. BETTON, M. D., M. A. N. S., etc., December 13, 1854. Published by the Society. Philadelphia, 1854.

Anniversary Discourse before the New York Academy of Medicine. Delivered in Clinton Hall, November 22, 1854. By JOHN H. GRISCOM, M. D. (Published by the Academy.) New York, 1855. (From the Author.)

Memoir of John A. Swett, M. D., late Professor of the Theory and Practice of Medicine in the Medical Department of the University of the City of New York. By B. W. M'CREADY, M. D. Read before the New York Medical and Surgical Society, and published by its order. New York, 1855. (From Dr. Du Bois.)

Address to the Graduates of the Medical Department of Pennsylvania College. By J. M. ALLEN, M. D., Professor of Special and Surgical Anatomy. With a list of Graduates. Philadelphia, 1855.

Oration delivered before the Physico-Medical Society of New Orleans, at their Anniversary Meeting, held December, 1854. By A. MERCIER, M. D., P., Principal Surgeon to the Circus Street Hospital. New Orleans, 1854. (From the Author.)

Annual Reports of the Commissioners and Superintendents of the Indiana Hospital for the Insane, to the General Assembly. November, 1854. Indianapolis, 1855.

Twelfth Annual Report of the Managers of the State Lunatic Asylum. Transmitted to the Legislature, January 17, 1855. Albany, 1855. (From Geo. W. Bradford, M. D.)

Report of the Pennsylvania Hospital for the Insane for the year 1854. By THOMAS S. KIRKBRIDE, M. D., Physician to the Institution. Published by order of the Board of Managers. Philadelphia, 1855. (From the Author.)

Report of the Board of Managers of the State Lunatic Asylum to the Eighteenth General Assembly. Jefferson City, 1855.

Nineteenth Annual Report of the Managers of the New York Institution for the Blind to the Legislature of the State. Made in conformity to law. January, 1855, for the preceding year. New York, 1855.

Fourth Biennial Report of the Trustees of the Illinois State Hospital for the Insane, December, 1854. Jacksonville, 1854. (From Dr. Frye.)

Annual Report of the Trustees and Superintendent of the State Lunatic Asylum of Pennsylvania, located at Harrisburg, for 1854. Harrisburg, 1855. (From Dr. Curwen.)

Second Annual Report to the General Assembly of Kentucky, relating to the Registry and Returns of Births, Marriages, and Deaths. From Jan. 1, 1853, to Dec. 31, 1853. Frankfort, 1854. (From Dr. Sutton.)

The Transactions of the New Hampshire Medical Society. (Sixty-fourth Anniversary.) Held at Concord, June 6 and 7, 1854. Concord, 1854. From Dr. Hubbard.)

The following Journals have been received in Exchange:—

Archives d'Ophthalmologie. Par M. A. JAMAIN, Docteur en Médecine de la Faculté de Paris. Sept., Oct. 1854.

Le Moniteur des Hôpitaux. Dec. 1854, Jan., Feb. 1855.

Revue de Thérapeutique Médico-Chirurgicale. Par A. MARTIN-LAUZER. Dec. 1854.

Gazette Médicale de Paris. Dec. 1854, Jan., Feb. 1855.

Annales de la Sociedad de Medicina, Montevideana, Nov. 1853, Jan., June, July, 1854.

The Glasgow Medical Journal. Oct. 1854, Jan. 1855.

Medical Times and Gazette. Dec. 1854, Jan., Feb. 1855.

Dublin Medical Press. Dec. 1854, Jan., Feb. 1855.

The Dublin Hospital Gazette. Dec. 1854, Jan., Feb. 1855.

The Edinburgh Medical and Surgical Journal. April, July, Oct. 1854, Jan. 1855

The Journal of Psychological Medicine and Mental Pathology. Edited by FORBES WINSLOW, M. D. Jan. 1855.

Association Medical Journal. Edited by JOHN ROSE CORMACK, M. D. Aug., Sept., Oct., Nov., Dec. 1854, Jan. 1855.

The Retrospect of Medicine. Edited by W. BRAITHWAITE. July, Dec. 1854.

Monthly Journal of Medicine. Dec. 1854, Jan., Feb. 1855.

The British and Foreign Medico-Chirurgical Review. Jan. 1855.

The Half-Yearly Abstract of the Medical Sciences. Edited by W. H. RANKING, M. D., and C. B. RADCLIFFE, M. D. July—Dec. 1854.

The Dublin Quarterly Journal of Medical Science. Feb. 1855.

The Medical Chronicle or Montreal Monthly Journal. Edited by W. WRIGHT, M. D., and D. C. MACCALLUM, M. D. Jan., Feb., March, 1855.

The Boston Medical and Surgical Journal. Edited by J. V. C. SMITH, M. D., and GEO. S. JONES, M. D. Jan., Feb., March, 1855.

The Peninsular Journal of Medicine. Edited by A. B. PALMER, M. D., and E. ANDREWS, M. D. Jan., Feb., March, 1855.

Southern Medical and Surgical Journal. Edited by L. A. DUGAS, M. D. Jan., March, 1855.

The Ohio Medical and Surgical Journal. Edited by JOHN DAWSON, M. D. Jan., March, 1855.

Nashville Journal of Medicine and Surgery. Edited by W. K. BOWLING, M. D., and PAUL F. EVE, M. D. Jan., March, 1855.

The Medical Counsellor. Edited by R. HILLS, M. D. Jan., Feb., March, 1855.

New York Dental Recorder. Edited by CHARLES W. BALLARD, D. D. S. Jan., Feb. 1855.

Charleston Medical Journal and Review. Edited by D. J. CAIN, M. D., and F. PEYRE PORCHER, M. D. Jan., March, 1855.

The American Journal of Dental Science. Edited by CHAPIN A. HARRIS, M. D., D. D. S., and A. SNOWDEN PIGGOT, M. D. Jan. 1855.

The New Orleans Medical and Surgical Journal. Edited by BENNET DOWLER, M. D. Jan. 1855.

The New Hampshire Journal of Medicine. Edited by GEO. H. HUBBARD, M. D. Jan., Feb., March, 1855.

The Stethoscope. Edited by Drs. ATKINSON, HAXALL, BOLTON, LEWIS, MERRITT, and WILSON. Jan., Feb., March, 1855.

The American Medical Monthly. Edited by E. H. PARKER, M. D. Dec., 1854, Jan., Feb., March, 1855.

The American Journal of Science and Arts. Edited by Professors B. SILLIMAN, B. SILLIMAN, Jr., and J. D. DANA, in connection with Professors GRAY and AGASSIZ, and Dr. GIBBS. Jan., March, 1855.

Iowa Medical Journal. Conducted by the Faculty of the Medical Department of the Iowa University. Dec. and Jan. 1854-5.

American Journal of Pharmacy. Edited by WM. PROCTOR, Jr. Jan., March, 1855.

The Medical Examiner. Edited by SAMUEL L. HOLLINGSWORTH, M. D. Jan., Feb., March, 1855.

The American Journal of Insanity. Edited by the Officers of the New York State Lunatic Asylum. Jan. 1855.

The Western Lancet. Edited by T. WOOD, M. D. Nov. 1854, Jan., Feb., March, 1855.

The American Medical Gazette. Edited by D. MEREDITH REESE, M. D. Jan., Feb., March, 1855.

New York Medical Times. Edited by H. D. BULKLEY, M. D., and J. G. ADAMS, M. D. Jan., Feb., March, 1855.

The Western Journal of Medicine and Surgery. Edited by LUNSFORD P. YANDELL, M. D. Dec. 1854, Jan., Feb. 1855.

Memphis Medical Recorder. Edited by A. P. MERRILL, M. D., and C. T. QUINTARD, M. D. Jan. 1855.

The New York Journal of Medicine. Edited by SAMUEL S. PURPLE, M. D., and STEPHEN SMITH, M. D. Jan., March, 1855.

St. Louis Medical and Surgical Journal. Edited by Drs. LINTON, MCPHEETERS, MOORE, and DEMING. Nov. 1854, Jan., March, 1855.

The North-Western Medical and Surgical Journal. Edited by N. S. DAVIS, M. D., and H. A. JOHNSON, M. D. Dec. 1854, Jan., Feb., March, 1855.

The New Jersey Medical Reporter. Edited by S. W. BUTLER, M. D. Jan., Feb., March, 1855.

The Virginia Medical and Surgical Journal. Edited by Drs. J. B. McCAW, J. F. PEEBLES, and G. A. OTIS. Jan., Feb., March, 1855.

New Orleans Medical News and Hospital Gazette. Edited by Drs. CHOPPIN, BEARD, SCHLATER, and VANCE. Jan., Feb., March, 1855.

Buffalo Medical Journal. Edited by AUSTIN FLINT, M. D., and SANFORD B. HUNT, M. D. Jan., March, 1855.

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All remittances of money, and letters on the *business* of the Journal, should be addressed *exclusively* to the publishers, Messrs. Blanchard & Lea.

 The advertisement-sheet belongs to the business department of the Journal and all communications for it should be made to the publishers.

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THE
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ART. I.—*Observations upon the Kidney and its Excretions in different Animals.* By JOS. JONES, Student of Medicine in the University of Pennsylvania. (With 20 wood-cuts.)

THE object of this paper is to present a general view of the kidney and its excretions, and to direct attention to certain facts and principles which have been much overlooked. For the thorough investigation of this subject many years of hard labour will be required; since not only must correct quantitative analyses of the urine be made, but also of the blood of different animals. Our knowledge of the blood and of the secretions and excretions of animals, with the exception of those of the mammalia, is as yet very limited and indefinite; indeed, we know nothing whatever about the chemical and physical properties of the blood and urine of the cold-blooded animals.

I. *Comparative Anatomy of the Kidney in Vertebrate Animals.*

1. MAMMALIA.—The urinary apparatus in mammalia consists, with a few exceptions, of four distinct portions :—

1. The glands which separate the urine from the blood.
2. A duct which conveys this excretion to a reservoir.
3. A receptacle for the urine.
4. A canal leading from the reservoir to the exterior of the body, through which the urine is discharged out of the animal economy.

These parts exist in all mammalia except the monotremata, in which the fourth division of the apparatus is absent, and the ureters empty into the cloaca. The separation in these animals of the uro-urethral from the semino-urethral passages demonstrates conclusively that the existence of the penis is essentially and subordinately related to the sexual organs, and not to the renal.

The kidneys are situated, in all cases, in contact with the posterior walls of the abdominal cavity, on either side of the vertebral column and aorta, resting partly upon the psoas magnus muscle. The right kidney, owing to the position of the liver, is generally situated lower than the left. The exterior border of the kidney is convex, and looks outwards towards the parietes of the abdomen. The interior border looks towards the vertebral column, and is concave, being excavated by a deep fissure, in which enter the nerves, arteries, veins, and excretory ducts. When a section is made, it is found to consist of two distinct portions—an external, vascular (cortical), and an internal or tubular (medullary) substance.

The substance of the kidney is divided into pyramidal masses, having their bases towards the surface, whilst their apices extend into the pelvis or the branches of the ureter. The number of these pyramids varies in different animals. In many of the quadrumana, cheiroptera, insectivora, and rodentia, there is only a single pyramid, whilst in the seals there are one hundred, and in the dolphins two hundred. These characters are possessed in general by the kidneys of all vertebrata.

The exterior form of the organ differs in the various orders. In man, its shape is oval, resembling a kidney-bean, whence its name. An external form similar to this is found in the kidneys of the apes, cheiroptera, and many of the rodentia and ruminantia. In most carnivora they are much rounded. In the mink they are much elongated.

In several animals which have only a single pyramid of Malpighi, the apex is prolonged, gradually coming to a point, thus forming a vermiform appendage, which projects through the pelvis of the kidney into the ureter. This structure may be seen in the kidneys of the red bat (*Nycticeus Noveboracensis*), common mole (*Scalops Aquaticus*), and the house mouse (*Mus Musculus*).

In many cats and weasels, the kidneys are, upon their external surface, incompletely lobed, resembling those of new-born infants. Many animals, especially those inhabiting the water, have these organs divided externally into many distinct lobes. Of this form, the kidney of the common cow presents the most familiar example. This is elongated, and has from eighteen to twenty-four distinctly-marked lobes; each of these is in reality a separate kidney, having a cortical and medullary substance, forming a pyramid of Malpighi, the apex of which is surrounded by a branch of the ureter.

The enlarged dilated superior portion of the ureter, called the pelvis, does not exist in the cow; the ureter continues of a uniform size during the whole course, from its junction with the bladder to the point where it sends off branches to the different lobes. This peculiar lobulation of the kidney is well marked in other animals. In the otter there are twelve, and in the bear from forty to fifty lobes. The seals have this organ divided into from seventy to one hundred lobes; in consequence of this minute division, its surface presents a tessellated appearance. The kidneys of the true cetacea have a racemose appearance, and in those of the dolphin there usually exist two hundred lobes. The

external characters of the kidneys of these last-mentioned animals resemble those of some birds, as the turkey-buzzard (*Cathartes Aura*), and of all the reptiles, and several fishes, as the catfish (*Galeichthys Marinus*), the toadfish (*Batrachus Tau*), and the stingray (*Trygon Sabina*).

Above each kidney is situated a small body, called the supra-renal capsule, the office of which is as yet unknown. The form of this varies with different animals: thus, it is of the shape of a cocked hat in man, and the general shape of the kidney in the mole; and lobulated in the cetacea and other aquatic mammalia. This would seem to indicate that some relation exists between these two organs. In what that relation consists, we are at present unable to say, because we are ignorant of the office which the supra-renal capsules perform in the animal economy.

a. Minute Anatomical Structure of the Kidney in Mammalia.—The minute anatomical structure of these organs, in all the mammalia thus far examined, resembles in every respect that of the human kidney. We have examined carefully, under the microscope, the kidneys of various animals, as the cow, raccoon, rabbit, squirrel, and others, both injected and uninjected, and in every case the structure was the same. The tubuli uriniferi were seen, consisting of a basement membrane, lined with epithelial cells, and ending in club-shaped extremities and dilatations, which embraced the convoluted mass of a vein and artery, thus forming the corpuscles of Malpighi.

The microscopical anatomy of the kidney of man is described in every recent work on anatomy, and it is therefore unnecessary to do so here.

b. Development of the Tubuli Uriniferi.—The kidneys of a small foetus of the common mouse, when examined under the microscope, were seen to consist, in large measure, of innumerable, elongated, and spheroidal cells. Amongst these, only a few short portions of the tubuli uriniferi were seen, and appeared to have been formed by the coalescence of these elongated cells. The kidneys of a foetus of the same animal, in a more advanced state of development, was also examined. The ureter was distinctly seen entering for a short distance the substance of the kidney, connected with which were many short tubuli uriniferi. The ends of these short tubes were, in many instances, connected with chains of elongated cells, which had coalesced together. Some of the urinary tubes terminated in closed, rounded extremities, resembling the free extremities of cells which had not as yet united with others. The substance of the kidneys was composed, in great measure, of spheroidal and elongated cells, amidst which short portions of the urinary tubes were seen, having continuations and branches formed by the union of these elongated cells. These facts indicate that the urinary tubes are formed from spheroidal cells, which, elongating in opposite directions, coalesce, and the walls, where they come in contact, are removed by absorption.

c. Relative Size of the Kidneys in the different Orders of Mammalia.—Although our investigations of this subject are by no means complete, still,

we hope that the following facts and conclusions will interest and excite careful observers to investigate this subject for themselves. The relation existing between the weight of the kidneys and the weight of the body of an animal was obtained by accurately ascertaining the weight of the animal, and then of its kidneys, and dividing the former by the latter. The average weight of the bodies of several raccoons was found to be eighty times heavier than the average weight of their kidneys.

Compare this result, obtained from carnivorous animals, with the relative weights of the kidneys and bodies of animals which subsist entirely upon vegetable food. The rabbit was found to weigh one hundred and fifty-eight, and the gray squirrel one hundred and ninety times as much as their kidneys. The kidneys of the raccoon, a carnivorous animal, are relatively twice as large as those of the rabbit and squirrel, herbivorous animals.

If the character of the food, the structure and size of the digestive apparatus, and the habits of the two classes of animals be attentively considered, we will understand at once why this relative difference in size should exist in organs fulfilling the same office in both. The food of the carnivora, as the name implies, consists of flesh and blood, which is capable of ready digestion and absorption, and rapidly supplies the wants of the animal economy. The intestinal canal is in all cases short, when compared with that of the herbivora, and in some individuals is not much longer than the body.

Carnivorous animals have voracious, and, in some cases, almost insatiable appetites. They gorge themselves with food, which is capable of entering the circulation with little or no alteration, and rapidly supplying the wants of the economy. There must be some organ to act as a safety-valve, and remove rapidly the large, unnecessary quantities of nutriment which are so often received into the circulatory system. To accomplish this effectually, there must be correspondingly large organs.

In the herbivora, on the other hand, the intestinal canal is very long; in the case of the sheep, it is twenty-eight times the length of its body. In this class of animals, the food consists of vegetables, which require minute subdivision, and the principles which they contain must go through many metamorphoses before they are ready to supply the wastes and wants of the system. Consequently their introduction into the circulation is gradual, the elimination of the products of the disintegrations of the tissues, whose place they supply, must be correspondingly slow and gradual, and a much smaller-sized organ will perform the same office in them than in the carnivora.

2. *AVES*.—The kidneys of birds are impacted within deep cavities of the sacrum, and extend from the lungs to within a short distance of the anus. They retain the impressions of the last ribs, and the transverse processes of the sacrum. They are, in all cases, lobulated; each kidney having from three to five principal lobes. In several orders, their general form is determined

secondarily by that of the sacrum, and primarily by the habits of the birds. Thus, in cranes having a long slender body, they frequently come in contact in the median line, and even coalesce, as in the *Ardea Candidissima*.

In the chimney-swallow (*Chætura Pelasgica*) and in the woodpeckers, where the sacrum is broad, for the attachment of the strong muscles of the tail and legs, the kidneys are correspondingly broad and short.

In birds of prey, as the fish-hawk (*Pandion Haliaetus*), which have a broad, deep sacrum for the support of the powerful muscles of the lower extremities, the kidneys are thick, broad, short, and deeply impacted within the cavities of the sacral bone. As far as our observations have extended, we may say that, as a general rule, the more powerful the exertions of a bird, the more active and energetic its habits of life, the more carefully will its kidneys be stowed away in the cavities of the sacrum. The necessity for this provision is evident when we consider the delicate nature of these organs, and the serious disturbance of the animal economy which immediately follows the infliction upon them of direct or indirect injuries. In the three other classes of vertebrate animals, the great Architect has placed them in contact with the posterior walls of the abdominal cavity, a position least of all liable to injury. In mammalia, reptiles, and fishes, the kidneys are protected by the vertebral column, and large and powerful dorsal muscles. In birds, these muscles are absent, because they are unnecessary in their peculiar structure and modes of locomotion. What better arrangement for the protection of these organs could have been devised, than that they should have been covered with bone at the part where they are most exposed? The peculiar form of the sacrum in birds, appears then to have been intended for the reception and protection of the renal organs.

Fig. 1 represents the kidneys of the small white crane (*Ardea Candidissima*), reduced to one-fourth its natural size, showing their union and coalescence in the median line. Fig. 2 represents the kidneys of the wood ibis (*Tantalus Loculator*), also reduced to one-fourth its natural size. The kidney of this bird presented, exteriorly, numberless small lobules, which are represented in the shaded portion of the drawing. These figures will give a better idea of the shape and positions of the renal organs, testicles, and supra-renal capsules, than a long and elaborate description.

The supra-renal capsules are small, flat, orange-colored bodies, always found on the mesial line of the anterior extremities of the kidneys. They are in contact externally with the large vascular trunks, and are generally covered by the testicles in the male, and the ovaries in the female. These organs are represented in Fig. 2 (*b*), and are seen lying above the testicles (*c*), which were remarkably small in this bird, in other respects an unusually large and fine specimen. In Fig. 1, we see that the supra-renal capsules are completely covered by the testicles (*a, a*).

The kidneys of struthious birds have small pyramids of Malpighi surrounded by calyces, and in the ostrich is found a true renal pelvis.

The ureters descend along the anterior surface of the kidneys, and terminate by perforating the cloaca posteriorly and superiorly. A true urinary

Fig. 1.

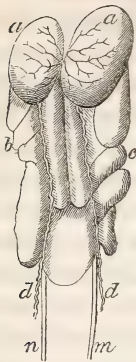


Fig. 2.

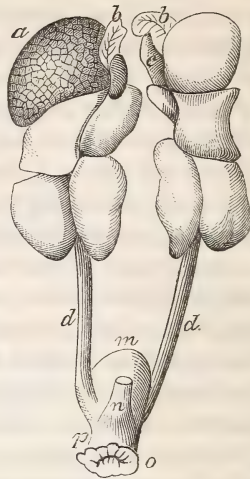


Fig. 1. Kidneys and testicles of the small white crane (*Aredea Candidissima*), one-fourth the natural size. *a, a*. Testicles. The supra-renal capsules lie between these and the anterior surface of the kidneys. *b, b*. Right kidney. *c*. Left kidney. These coalesce at their inferior portion. *d, d*. Spermatic cords. *n, m*. Ureters.

Fig. 2. Kidneys of the wood ibis (*Tantalus Loculator*), one-fourth the natural size. *a*. Superior lobe of right kidney, upon the surface of which are represented the small lobuli. *b, b*. Supra-renal capsules. *c*. Testicle. *d, d*. Ureters. *m*. Bladder-like expansion of the cloaca, considered by many anatomists a rudimentary bladder. *n*. Inferior portion of the rectum. *o*. Anus. *p*. Cloaca.

bladder is absent, although in many birds the ureters open into a distinct pouch-like dilatation of the cloaca. This may be seen in Fig. 2 (*m*). It is bounded above and below by a valvular fold, which separates it from the urethro-sexual cavity and the rectum (*n*).

a. Minute Anatomical Structure.—The kidneys of birds are composed of minute lobuli, which, in some individuals, as the *Tantalus Loculator*, can be distinguished externally. (Fig. 2, *a*.) In the turkey-buzzard (*Cathartes Aura*) they are convoluted, giving to the surface a tessellated aspect. In other birds, they are polyhedral. These lobules are connected with separate branches of the ureter, and have the shape of the pyramids of Malpighi. Towards their apices, they consist of converging tubuli uriniferi forming the medullary portion, whilst the cortical or outer parts consist of the branches and convolutions of these urinary tubes. These tubuli uriniferi, in all the birds whose kidneys we have examined under the microscope, end, as in mammalia in the Malpighian bodies, in loops, or in simple club-shaped extremities.

The absence of a pelvis and calices in the kidneys of most birds, appears to be absolutely necessary, when we consider the character of their urine,

which is semi-solid, consisting of innumerable small globules of the urate of ammonia. If there were any cavities in which this could accumulate, renal calculi would be of frequent occurrence. The existence of cilia in the cells lining the interior of the urinary tubes, has not as yet been accurately determined. Gerlach thinks that he has seen them on one occasion in the fowl. Although they were not noticed by us in our microscopical examinations of the kidneys of the *Pandion Haliaetus*, *Ardea Discors*, and other birds, still, we think that it is highly probable that they do exist, because they would aid materially in the movement and expulsion of these minute globules of the urate of ammonia.

b. Relative Size of the Kidneys of Birds.—So far as our observations have extended, the same law holds good with birds as with mammalia. The kidneys are relatively larger in the carnivorous than in the frugivorous or granivorous birds. A glance at the following weights will render this evident:—

Weight of <i>Ardea Candidissima</i> , 86 times the weight of its kidneys.			
"	" <i>Discors</i> ,	100	" " " "
"	<i>Garrulus Cristatus</i> ,	115	" " " "
"	<i>Picus Auratus</i> ,	121	" " " "

The first two birds are carnivorous, feeding upon fish and crustacea, whilst the last two live upon a mixed diet of berries and insects. In the carnivorous birds, the intestinal canal is much shorter than in those living upon a vegetable or mixed diet. Their food is capable of more rapid digestion and introduction into the circulation, and, as a necessary consequence, the organ which regulates the amount of the solid and fluid materials of the blood, and eliminates all waste and useless matters, must be correspondingly larger.

According to our investigations, the average weight of the kidneys of birds is relatively greater than that of mammalia. That this should be the case, might be expected from the constitution of birds. Their circulation is more rapid than that of the mammalia; the temperature of their bodies is some ten degrees higher, and their mode of life is, in all respects, more active and energetic. They consume relatively more food, and the skin is not so active in its functions. The necessary consequence of all these actions is, that the materials of their bodies will more rapidly undergo change, and need a corresponding rapid supply of nutriment, and organs adequate to perform the offices of secretion and excretion.

The weight of the turkey-buzzard is not more than seventy times the weight of its kidneys. Here we have a bird which lives exclusively upon animal food, which gorges itself on all occasions with an unnecessary amount of food: the consequence is that it must have large organs, capable of eliminating from the blood a large amount of useless materials.

3. REPTILIA.—The kidneys of reptiles are situated, like those of mammalia and fish, in contact with the posterior walls of the abdominal cavity, and lie upon each side of the vertebral column. In this class they are always lobu-

lated, but differ widely as to length, breadth, and general form, and position in the different orders.

In the chelonia and batrachia, their general form may be described as elongato-oval. Fig. 3 represents the kidney of the *emys terrapin*, natural size.

In the sauria they are triangular, broad, and thin, with numerous convoluted lobules. Fig. 4 represents accurately the kidneys of the *Alligator Mississippiensis*, reduced to one-ninth the natural size. Fig. 5 is the kidney of the six-striped lizard (*Ameiva Sexlineata*).

Fig. 3.



Fig. 4.

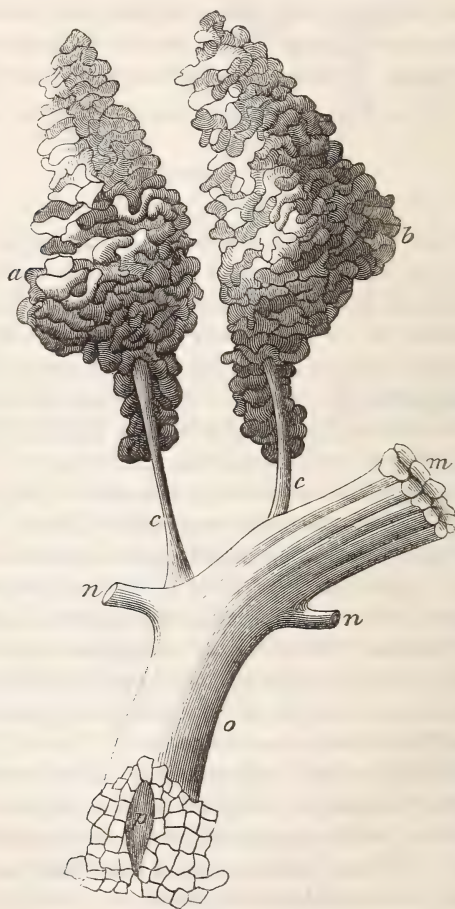


Fig. 5.



Fig. 3. Kidney of the *emys terrapin*, natural size.

Fig. 4. Kidneys of the *Alligator Mississippiensis*, one-ninth the natural size. Length of alligator six feet. *a*. Right kidney. *b*. Left kidney. *c, c*. Ureters. *m*. Rectum. *n, n*. Oviducts. *o*. Cloaca. *p*. Anus.

Fig. 5. Kidney of the six-striped lizard (*Ameiva Sexlineata*), natural size.

In the ichthyodea and tailed batrachia they are narrow, and run to a point anteriorly.

In the ophidia they are narrow and much elongated, and often situated asymmetrically. In this order, the effect of the general form of the animal upon that of its kidneys, and in fact upon all the internal organs, is very perceptible. In the coachwhip snake (*Psammophis Flagelliformis*), a long, slender serpent, constructed for swift motion, the kidneys are correspondingly long and slender, and are situated asymmetrically, the one above the other, the abdominal cavity not being sufficiently capacious for both, lying parallel. This form and arrangement of the kidneys may be said to be due, primarily, to the habits and mode of life of this snake. It subsists upon lizards and insects, which are active and swift. Their enemy must be correspondingly constructed for activity and swiftness of motion. The same observations apply to the black-snake (*Coluber Constrictor*) and other reptiles having the same habits.

The rattlesnakes, bull-snake (*Pituophis Melanoleucus*), and others, which live by force and stratagem, rather than by activity, have a large stout body, and correspondingly broad and thick kidneys. Fig. 6 represents the kidneys of the coachwhip snake (*Psammophis Flagelliformis*) reduced to one-fourth their natural size.

In the ophidia and sauria, a urinary bladder is absent, the ureters open directly into the cloaca, as may be seen in Figs. 4 and 6.

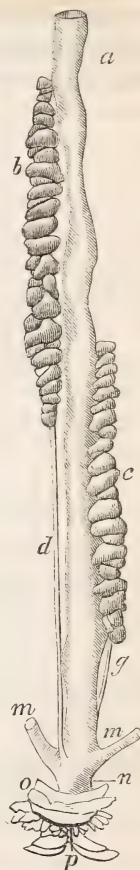
The chelonia have a large bladder, and the ureters, instead of penetrating its walls, empty themselves directly into the cloaca in front of it, so that the urine has to reascend to the urinary reservoir.

The ureters in ophidia are situated upon the internal inferior edges of the kidneys.

The supra-renal capsules are found in the sauria, chelonia, and ophidia, and are generally of a yellow colour, and elongated, lobulated form, and situated above the kidneys, either within the folds of the peritoneum, or more freely near to the sexual organs.

a. Minute Anatomical Structure.—The essential structure of the kidneys is nearly the same throughout all the orders of reptiles. The distinction between the cortical and medullary substance does not exist in this class.

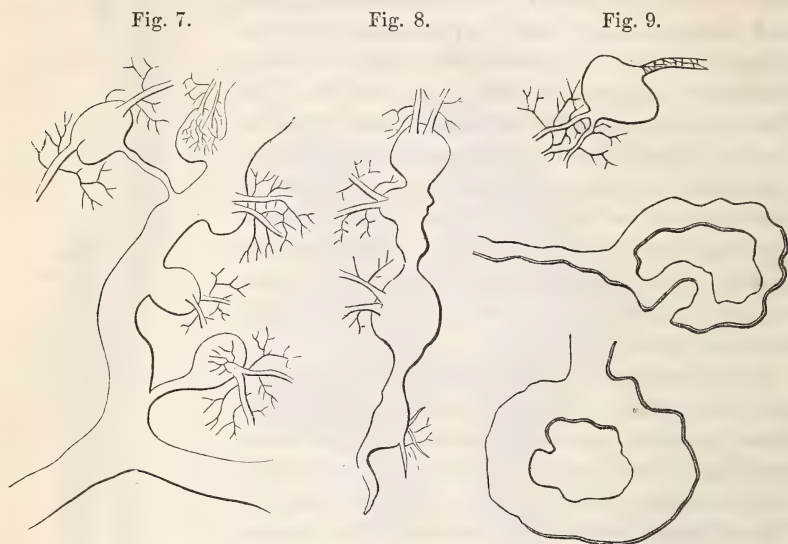
Fig. 6.



Kidneys of the coachwhip snake (*Psammophis Flagelliformis*), one-fourth the natural size. *a.* Large intestine. *b.* Right kidney. *c.* Left kidney. *d.* Right ureter. *g.* Left ureter. *m, m'.* Inferior portions of the ducts of the ovaries. *n.* Cloaca. *o.* Ventral scales. *p.* Anus.

The ureter, which runs along the entire length of the organ, sends off numerous canals, which are short in the lower orders, and long and convoluted in the higher. These urinary tubes form groups constituting lobules, which receive branches from the portal trunk that supplies the organ with blood. In the animals of this class, whose kidneys we have examined under the microscope, we have invariably found the tubuli uriniferi terminating in club-shaped extremities, loops, and Malpighian corpuscles.

Figs. 7, 8, and 9, represent a magnified view of the tubuli uriniferi of



Tubuli uriniferi, and corpuscles of Malpighi, from the kidney of the glossy black pine-snake (*Pituophis melanoleucus*). Magnified about 190 diameters.

the glossy black pine-snake (*Pituophis melanoleucus*), which were injected with the chromate of lead. It will be seen, from this injection, that the dilations of the urinary tubes occur all along their course, the Malpighian corpuscles being not restricted, as in mammalia, to the cortical substance, but distributed throughout the entire substance of the kidney. The ciliary motion has been discovered, by Bowman and other observers, in the neck of the Malpighian bodies, and in the commencement of the tubuli uriniferi of serpents, and also of the frog, salamander, triton, bombinator, and bufo. The ciliary motion is in the direction of the urine, towards the ureter. The utility of this arrangement is evident, when we consider that in many of these animals, as the ophidia, the urine is in a semi-solid condition, containing numerous globules of the urate of ammonia.

b. Relative Size of the Kidneys of Reptiles.—The kidneys of the sauria and ophidia are larger than those of the chelonia and batrachia. The

former are carnivorous, and much more voracious than the latter. The kidneys of the ophidia are relatively much larger than those of either mammalia or birds. The body of the pine-snake (*Pituophis Melanoleucus*) was found to be not more than sixty times as heavy as its kidneys. The kidneys of carnivorous birds and mammalia are not relatively so heavy.

The question immediately arises, why should cold-blooded animals, whose circulation is sluggish, who eat but seldom, often abstaining from food for months, and even years, and whose habits are indolent, be provided with large urinary organs? In birds, we have seen that an elevated temperature, rapid circulation, and active, energetic mode of life, called for large organs, capable of removing in an efficient manner from the blood the products of the disintegrations of the system; and here we have a condition of things, directly contrary in every respect, calling for still larger organs to perform the same office.

In attempting to explain this phenomenon, we must consider the following facts and laws of the animal economy. The blood contains a definite amount of materials to be eliminated by the kidneys, diffused through its entire mass. That all these materials should be separated, the entire mass of the blood must be presented to these organs; and the rapidity of the elimination will depend upon the rapidity of the circulation, the activity of the excreting cells, and the size of the organs.

The temperature exerts a great influence upon all the actions of the animal economy. It is a well-known fact that seeds will not germinate or eggs hatch; or, in other words, life action will not commence or continue below a certain temperature. If the temperature of the cold or warm-blooded animals be reduced much below the normal standard, their functions will gradually become more feeble, and finally cease altogether. It may be stated, as a general law, that the lower the temperature of any organ, the more feeble will be its action. Nervous force exerts a great influence upon secretion. It is known to every naturalist that cold-blooded animals have much less muscular and nervous power, in proportion to their size, than the warm-blooded animals.

From these considerations, then, it is evident that to perform the same office, to secrete precisely the same amount, larger organs are required in the cold than in the warm-blooded animals.

The character of the alimentary canal, and the habits of serpents, also render the existence of large kidneys necessary. These animals are carnivorous, and have a short alimentary canal, which, in many instances, is almost completely straight. They take their food in large quantities, often swallowing animals heavier and larger than themselves. All this animal matter is capable of digestion and absorption into the blood, a large portion of which is superfluous. The great channel for the elimination of this is the kidneys; the lungs being mere sacks, and performing the function of respiration in an imperfect manner.

4. PISCES.—In the different orders of this class, we find many diversities in the general form and situation of the kidneys. There is no special type, and the development of the kidney does not, by any means, correspond with the position which the fish occupies in the classification of naturalists. The kidneys of fish of a low order, as the stingray, resemble, in position, structure, and appearance, those of the chelonia and batrachia, and appear, in more respects than one, to be of a higher organization than the kidneys of many of the acanthopterygii and other fish occupying relatively much higher ranks. We will give a few examples, to show the general position, character, and diversities of the kidneys of different fish.

The kidneys of the garfish (*Lepisosteus Osseus*) commence immediately beneath the heart and opening in the œsophagus (through which air passes into the lung), on each side of the inferior portion of the cranium, and extend throughout the length of the abdominal cavity to within about one-fourth of an inch of the anus. Their posterior surface is in contact with the vertebral column and the posterior walls of the abdominal cavity, while their anterior surface lies in contact with the posterior surface of the lung. They are covered with dense fibrous tissue, which is reflected from the lung over the kidneys, and unites with the fascia of the posterior walls of the abdominal cavity.

The vertebræ upon their anterior face have a prominent central keel, which is perforated. Through this perforation pass portions of the kidneys on each side, and coalesce together. In this respect, the kidneys of this remarkable fish, the connecting link between reptiles and fish, differ from all others that we have yet examined. The kidneys fill up the depressions in the vertebra, and those in the posterior walls of the abdominal cavity between the ribs; they are therefore lobulated.

Their length is very great, whilst their breadth is comparatively small, as will be seen in the following measurements :—

Length of fish	2½ feet.
Length of each kidney	12 inches.
Breadth “ “	$\frac{3}{10}$ ths of an inch.

A section of the kidney, showing its position, appearance, and the perforation of the keel, is shown in Fig. 10.

The ureters run along the edges of the kidneys next to the vertebral column. They dilate into a bladder, which is situated posteriorly to the rectum.

The lung extends from an opening in the œsophagus, throughout the length of the abdominal cavity. It is capable of great distension, having, when inflated, a diameter almost equal to that of the fish.

The salt-water catfish (*Galeichthys Marinus*), belonging to the same order as the garfish, malacopterygii abdominales, has both kidneys coalesced into one; thus differing entirely in appearance, relative dimensions, and position,

from those of the garfish. The general shape of the body of the kidney may be said to be semi-lunar. Superiorly it is wide, and gradually tapers until it

Fig. 10.

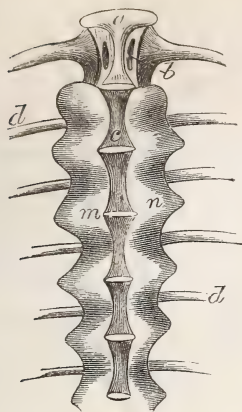


Fig. 11.

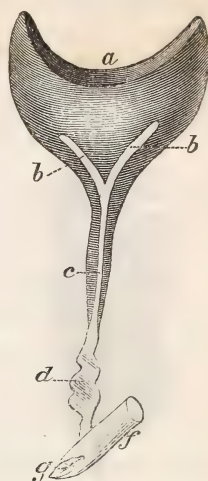


Fig. 10. Section of the kidneys of the garfish (*Lepisosteus Osseus*). Natural size. *a*. Articulating surface of the vertebra. *b*. Aperture in the anterior central prominent portion or keel of the vertebra, through which the kidneys pass and coalesce with each other. *c*. Anterior surface of the vertebral keel. *d, d*. Ribs. *m*. Right kidney. *n*. Left kidney.

Fig. 11. Kidneys of the salt-water catfish (*Galeichthys Marinus*). Natural size. Length of fish 10 inches. *a*. Superior concave border of the kidneys, which receives the inferior convex border of the air-bladder. *b, b*. Right and left branches of the ureter. *c*. Ureter. *d*. Bladder. *f*. Rectum. *g*. Anus.

reaches the bladder. The superior border is concave, and embraces the posterior convex border of the air-bladder. Fig. 11 will give a much more accurate idea of its appearance than any description.

In many of the acanthopterygii, as the whiting (*Umbrina Alburnus*), the yellowtail (*Leiostomus Xanthurus*), the young drum (*Pogonias Fasciatus*), the croaker (*Micropogon Undulatus*), the sheephead (*Sargus Ovis*), and the angel-fish (*Ephippus Faber*), the general position and arrangement of the kidneys are similar, in most respects, to those of the garfish. The toadfish (*Batrachus Tau*), placed by naturalists in this order, has kidneys differing in position, size, appearance, and, in fact, in every respect from these individuals. They are short, thick, elongato-oval, resembling, in every respect, the same organs in the plagiostomi, fish of a much lower organization, according to the classification of naturalists, and also those of many reptiles, as the chelonina, sauria, and batrachia.

The kidneys of the stingray (*Trygon Sabina*), resemble, in structure, appearance, and situation, those of the chelonina and batrachia, and, among fishes, those of the toadfish. Fig. 12 represents the kidney of the stingray. By comparing this with the kidney of the emys terrapin (Fig. 3), or those of the alligator (Fig. 4), the resemblance will immediately be seen.

Fig. 13 represents the kidneys of the whiting (*Umrina Alburnus*), reduced one-third their natural size.

Fig. 12.



Fig. 13.

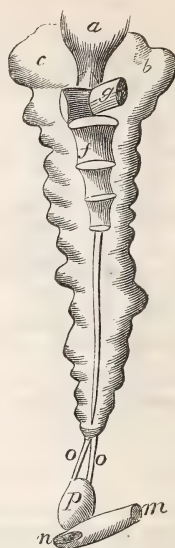
Fig. 12. Kidney of the stingray (*Trygon Sabina*).

Fig. 13. Kidneys of the whiting (*Umrina Alburnus*). One-third the natural size. Length of fish 12 inches. *a*. Inferior portion of the cranium. *b*. Left kidney. *c*. Right kidney. *g*. Muscular attachment of the oesophagus to the vertebral column. *f*. Vertebra. *o, o*. Ureters. *p*. Bladder lying posterior to the rectum. *m*. Rectum. *n*. Anus.

This may be taken as the type of the kidneys of nearly all the fish belonging to the order Acanthopterygii. By comparing with each other Figs. 10, 11, 12, and 13, we will see at once that there is no definite type of kidneys for each order, or for fish as a class. A bladder is present in many individuals of this class, and in all cases it lies posterior to the rectum, differing in this respect from the arrangement of the bladder in mammalia and reptilia, and resembling in its position the rudimentary bladder of birds. This one fact will serve to distinguish fish from all other animals.

The supra-renal capsules are very small in fishes. In some orders, they are small, bean-shaped bodies, of a pale colour, connected by vessels with the apex of the kidneys. In others, they are small, reddish-white corpuscles, placed behind the kidneys, against the vertebral column.

a. Minute Anatomical Structure.—The kidneys of fish consist of tufts of urinary tubes, which are sent off transversely at intervals from the long ureter, and are connected together by the loose fibrous tissue which supports the network of vessels distributed upon their walls. When portions of the kidney of the garfish (*Lepisosteus Osseus*) were carefully viewed under the microscope, numerous tubuli uriniferi were seen, dividing and subdividing into dis-

tinct branches, in some cases terminating in loops, in others, in club-shaped dilatations, and in others still, in cup-like dilatations, which embraced convoluted masses of bloodvessels. These corpuscles of Malpighi, although small, were distinct and well formed. The urinary tubes in most fish are long and tortuous, but seldom ramified. The corpora Malpighiana are generally present; they are, however, in some of the lower orders, as the cyclostomi, imperfectly formed. The minute anatomical structure of the kidneys of fish reminds us of that of the corpora Wolfiana, the temporary renal organs of the higher vertebrata.

b. Comparative Size of the Kidneys of Fish.—Owing to the great difficulty of separating, thoroughly and accurately, the kidneys from the vertebral column, to which, in most individuals, they closely adhere, this subject has not been investigated far enough to allow of general conclusions.

The following weights are not without some interest:—

Weight of catfish,	90	times	heavier	than	that	of	its	kidneys.
“ “ toadfish,	114	“	“	“	“	“	“	“
“ “ stingray,	177	“	“	“	“	“	“	“

II. Comparative Anatomy of the Kidney in the Invertebrate Animals.

In this class, the kidneys, like all the other organs, are much more simple in their structure than those of the vertebrata. The following are the most important results of the recent researches of Siebold and other anatomists.

1. ARTHROPODA.—In the class insecta, the kidneys consist of small tubes, varying in length, number, and modes of grouping in the different orders. These tubes open upon the posterior or pyloric extremity of the stomach, by means of one or two common excretory ducts. These ducts are sometimes dilated, bladder-like, at their point of insertion. The free extremities of the uriniferous tubes generally terminate cœcally or anastomose with each other. When very long, they form numerous convolutions around the digestive canal, and sometimes creep with their anterior extremities between the coats of the stomach, or by their posterior between those of the colon. They are composed of a homogeneous external tunic or basement membrane, filled internally with large cells, disposed in rows, so that nowhere can there be perceived in the interior of the vessel a glandular canal defined by a special epithelium. Each cell contains a colourless nucleus, and a multitude of very fine yellow or brown granules. These cells are ruptured, and the granular bodies, which constitute the urinary secretion, pass through the spaces between the cells, and flow gradually into the intestinal canal. The urinary organs of the larvæ and pupæ of this class resemble somewhat those of the perfect insect.

The urinary organs of the arachnoidæ consist of small, usually multiramoso glandular tubes, which open into the cloaca, and exactly resemble the Malpighian vessels of the insecta, both by their structure and the nature of the fluid which they secrete.

In the crustacea, up to the present time, urinary organs have been observed only in the myriapoda, and like those of the insecta, they consist of very small, brownish, cœcal vessels, describing many convolutions around the stomach and intestines, and open into the digestive canal at the boundary between the stomach and intestine.

2. MOLLUSCA.—The urinary organs of the cephalopoda present the appearance, externally, of variously ramified glandular tufts, connected with the venæ cavæ, and sometimes with the principal veins. Careful examinations have shown these organs to consist of an internal parenchyma, composed of a contractile fibrous tissue, through which are spread branches coming from the venæ cavæ. This parenchyma is surrounded by a structureless membrane, covered with several layers of nucleated granular cells. The urine, of a dirty yellow colour, is secreted from the external surface of this cellular layer, and escapes immediately into the peritoneal cavities, and then is discharged externally through their orifice. The peritoneal cavities in this function resemble urethral canals.

With most of the cephalophora, the kidney consists of an uneven lamellate gland, which is usually situated near the bronchial or principal pulmonary vein. The excretory duct accompanies the rectum, and often opens near the anus. The gland is surrounded by a sac-like envelop, which is continuous with the internally ciliated excretory duct. Upon the exterior of this membranous envelop, which appears to act the part of a basement membrane, ramify bloodvessels. In the gland itself, no bloodvessels have been observed. Each renal lamella is composed of thickly-set, delicate cells, loosely bound together, containing a transparent liquid, in which float brown or violet-coloured nuclei. These nuclei are in form round and embossed; they have a dense crystalline structure, and are undoubtedly a product of renal secretion, because they contain uric acid.

A very remarkable organ, found in all the lamellibranchia, known as the gland of Bojanus, has been demonstrated to be undoubtedly of a renal nature. This organ is always double, and consists of a large, long sack, with glandular walls of a dirty yellow or dark green colour. It is situated on each side of the back, between the pericardium and the inferior abductor muscle, and extends usually upon the sides of the abdomen to the base of the branchiæ. These glands communicate with the cavity of the mantle, by two small openings, which have swollen borders, and are situated sometimes at the upper and sometimes at the lower end of the sac. The usually very thin walls of these two sacs have numerous folds or plicæ, which form compartments or areolæ, all of which are covered with a very delicate ciliated epithelium. The parenchyma of these walls is composed of a very loose tissue, which, upon the least disturbance, separates into small granular cells. Many of these cells contain a blue-black, round nucleus, which is very solid, and sometimes so large as to be visible to the naked eye as an inorganic concretion.

As these nuclei contain uric acid, they may be compared to renal calculi. The walls of these kidneys are surrounded by a distinct network, which arises from the large venous reservoir in which the afferent blood of the body is accumulated.

No special organs for the excretion of the urine have as yet been discovered in the vermes, zoophyta, or protozoa.

III. *The Excretions of the Kidney in the Vertebrata and Invertebrata.*

The offices of the kidney in the animal economy are—

1. To separate from the blood those products of the breaking-down and metamorphosis of the tissues which can be of no further use in the animal economy, and which, if retained, would be productive of evil consequences.
2. To remove an excess of nutritive materials, when they have been introduced into the blood in an amount beyond what is necessary to supply the wants of the animal economy.
3. To draw off from the blood any extra, unnecessary amount of water.

The active agents in these processes, in all animals, both vertebrate and invertebrate, are the epithelial cells lining the basement membrane of the tubuli uriniferi, or the renal sacs. These cells possess a power of separating certain materials, as urea and uric acid, from the blood, whilst they allow its other constituents to pass untouched. That this power of depurating the blood is resident in them alone, is conclusively demonstrated by the fact that, when they are removed from the basement membrane of the tubuli uriniferi, immediately albumen and other constituents of the blood, not normally excreted by the kidneys, appear in the urine. The basement membrane of the urinary tubes, then, does not possess this power of selection. The power which these cells possess, of preventing certain materials of the blood from passing through them, is definite, and capable of being overcome by pressure. When the walls of the heart are preternaturally thickened, so that the blood is propelled with great force through the circulatory apparatus, it often happens that the power of selection in the cells is overcome, and albumen and other necessary constituents of the blood are forced out along with the excrementitious matters, and appear in the urine.

The distinction between an excretion and a secretion should here be drawn. These acts have been confounded by almost every physiologist.

The circulatory apparatus not only carries nutriment to the different organs and tissues, but also removes from them the products of their disintegration and metamorphoses, which can be of no further use in the animal economy. The act of separating these materials from the blood is called excretion. The process is one of simple separation. The amount and character of an excretion depend entirely upon the amount and character of the excrementitious materials existing in the blood.

If the kidneys be extirpated, their office will be assumed by the skin, and even the gastro-mucous membrane. In like manner, if the function of the skin be checked, it will be assumed by the kidneys. The act, then, of separating certain materials from the blood can be transferred from one excretory organ to another.

A secretion does not pre-exist in the blood. We do not find the gastric juice or the salivary fluid existing in the blood before they are elaborated by special organs. Each secretion must have a special set of cells, which alone can produce the peculiar material. We never find one organ elaborating the secretion of another distinct organ. This is a fixed law. The salivary gland never secretes gastric juice, nor the mamillary gland bile. The stomach of a rattlesnake never secretes the deadly fluid of its poison-gland. Thus, in two essential respects, an excretion differs from a secretion.

In both excretion and secretion, the active agents are organic cells. Under the microscope, these cells present very much the same appearance; there are no distinctive characters corresponding to their different offices. The most accurate means of investigation which we possess have failed as yet to give us any satisfactory interpretation of a great phenomenon, which lies at the foundation of all our knowledge of living beings. How do organic cells elaborate particular products, or separate certain materials from the blood? Medicine can never, in truth, be considered a science until this question is definitely settled.

A series of experiments, which I performed last summer, throws a faint glimmering of light upon this subject. Animal membranes were filled with solutions of certain chemical substances, as oxalate of ammonia, and immersed in solutions of other substances, as chloride of calcium, which would form uncrystallized, insoluble precipitates when mingled. It was found, when endosmose and exosmose of the solutions had taken place, that the precipitates were crystalline, and differed in the physical arrangement of their particles on the opposite sides of the membranes. The physical influence exerted by the membranes differed with the direction of the current. By extending the experiments, it was found that membranes from different parts of the same animal, or from relatively the same parts in different animals, effected different physical arrangements of the molecules of the same body. When live frogs were allowed to remain for a certain length of time in a solution of the oxalate of ammonia, and carefully washed and transferred for a similar length of time to a solution of the chloride of calcium, beautiful octohedral, dumb-bell, cubical, and ellipsoidal crystals of the oxalate of lime were found, by the aid of the microscope, in the blood and different tissues. Similar results were obtained when the cut ends of growing plants were treated in a similar manner. These experiments were repeated, with similar results, with various other chemical substances.¹ Here, then, we have animal and vegetable

¹ For further details of these experiments, the reader is referred to the department of "American Intelligence" in this number.—Ed.

membranes and cell-walls, both living and dead, altering the physical arrangement of the molecules of inorganic compounds. In the dead animal membranes, all vital influence was absent. This change was brought about entirely by their physical constitution. That it was not due to the presence of blood, or putrefying organic matters, or some other fluid, was proved by numerous experiments, carefully performed.

It is remarkable that the physical forms impressed both by the living and dead animal membranes upon the oxalate of lime, were precisely those which occur in the urine of various animals, and even in that of birds and fishes. Is the conversion of albumen into albuminose more wonderful, or does it require more power than this influence exerted by a dead animal membrane upon an inorganic substance? Do not the results of these experiments render it probable that many secretions and excretions in the animal economy are the results of purely physical causes? It is highly probable that membranes, by minutely dividing and separating the ultimate particles of substances passing through them, communicate to them certain motions, thus changing their molecular arrangement, and with it their physical and chemical properties.

In considering the causes which influence the physical and chemical properties, and relative amounts of the excretions of the kidney in different animals, we should investigate carefully the following subjects:—

1. The Amount, Force, and Rapidity of the Circulation of the Blood. When definite substances in limited proportions are required to be eliminated from the blood by any organ, the activity of that organ will depend upon the activity of its excreting cells; the amount of blood supplied to those cells, and the force with which it presses against them. The influence of arterial pressure upon the function of the kidneys is conclusively proved by M. Bernard's experiments. A dog having an arterial pressure of 76 millimetres, its kidneys secreted 0.80 grammes of urine in a minute. When the pressure of the blood was increased to 112, by tying all the arteries of the limbs, 12.23 grammes were secreted in the same length of time. Another animal, in whom the pressure was reduced by bleeding to 57, passed in one minute 2.06 grammes. The pressure was raised to 122 by the injection of blood into the circulation, and then 19.34 grammes of urine passed. These experiments were varied and performed in several instances, and always with the same result; the quantity of urine increased or diminished with the increase or diminution of the force of the circulation.

2. The Activity of the Glands of the Tegumentary System, which are connected with, and materially modify the function of the kidneys. They both secrete from the blood, substances closely allied in physical and chemical constitution. The quantity of these substances is limited. The sluggishness, or increased activity of the one, therefore, must necessarily produce a state directly opposite, in the other.

3. The Action of the Colon. In some animals, the colon performs an im-

portant office in the depuration of the blood, whilst in others, as reptiles, its function in this process is exceedingly feeble.

4. The Activity of Respiration. An intimate connection exists between the function of the lungs and the materials eliminated by the kidneys. Upon the function of respiration depends, in a great measure, the production of heat, and upon the temperature depend many of the chemical changes taking place in the animal economy.

5. The Physical and Vital Constitution, and Modes of Living, influence materially the size and activity of the kidneys, and the amount and character of its excretions. Birds, whose circulation is rapid, whose temperature is high, whose exercise is frequent, swift, and violent, need large kidneys, to eliminate in a corresponding manner the incessant wastes of their tissues. Serpents who seldom take food, but who, when they do, gorge themselves with large and unnecessary quantities, need an organ which will be able to eliminate this superabundant mass of nutritious matter, which has been received by absorption into the blood. In cold-blooded animals, the temperature is low, the circulation sluggish, respiration imperfect, and, as a necessary consequence, all the operations of secretion and excretion are correspondingly slow, and require relatively much larger organs to accomplish the same amount of labor than those of the more highly organized.

1. *Urine of the Vertebrata.*

1. *Urine of the Mammalia.*—As the character of the urine is affected greatly by the nature of the food, animals may be divided into three great classes: Carnivora, Herbivora, and Omnivora. The urine of the first two, will have specific characters, whilst that of the latter will resemble both, or partake of the characters of one or the other exclusively, according to circumstances.

a. *Urine of the Carnivora.*—The urine of this class of animals, as a general rule, is of a much higher specific gravity, and more scant in quantity than that of the herbivora, for several important reasons. Their nutriment is of a much more concentrated form, and does not contain so large an amount of fluids. Their mode of life is more energetic and active, necessarily leading to a greater wear and tear of the tissues. And also the glands of their tegumentary system are not so numerous or active. Hieronymus examined the urine of lions, tigers, and leopards, and found it to be alike in all.

At a temperature of 50° F. to 55° F., the specific gravity of the urine from the same animal varied from 1059 to 1076.

The mixed urine of these three animals gave the following result:—

Water	846.00
Solid constituents	154.00
Urea, alcohol-extract, and free lactic acid	132.20
Uric acid	0.22
Vesical mucus	5.10

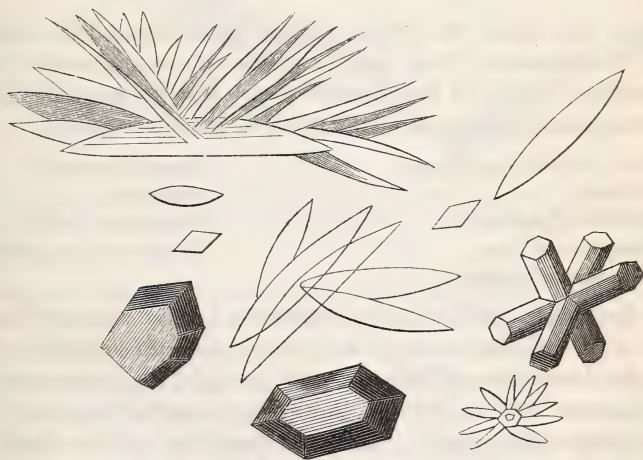
Sulphate of potash	1.22
Chloride of ammonium, and a little chloride of sodium	1.16
Earthy phosphates	1.76
Phosphates of soda and potash	8.02
Phosphate of ammonia	1.02
Lactate of potash	3.30

Hieronymus also examined the urine of the hyena and panther, and found them to contain the same ingredients.

Specific gravity of the urine of the hyena at 50° F.	10.61
“ “ “ “ panther at 48° F.	10.45

The following is a qualitative analysis of the urine of the red bat (*Nycticeus Noveboracensis*), which I made during the month of August. This animal being very small, only a few drops could be obtained from its bladder, consequently our instrument in its analysis was the microscope. After standing for a short time upon a glass slide, there made their appearance, under a magnifying power of 210 diameters, conglomerated masses of lozenge-shaped crystals of uric acid, small globules of the urate of ammonia, prismatic crystals of triple phosphate, and a remarkable looking crystal, composed of three hexagonal columns, crossing each other at right angles. This crystal revolved over and over in the urine upon the slightest motion. Fig. 14

Fig. 14.



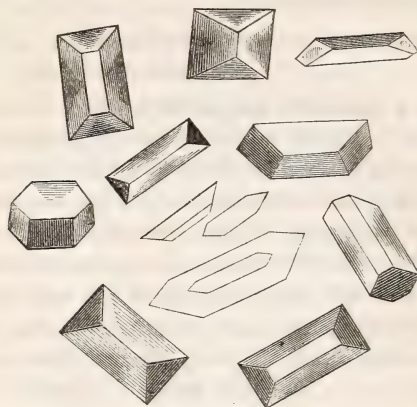
Crystals of uric acid and triple phosphate deposited in the urine of the red bat (*Nycticeus Noveboracensis*), a short time after its removal from the bladder. Magnified 210 diameters.

represents these crystals. When treated with hydrochloric acid, cubical and octohedral crystals, and crosslets of the chloride of sodium, and lozenge-shaped crystals of uric acid made their appearance under the microscope. In a speci-

men of urine from another red bat, innumerable beautiful prismatic crystals of the triple phosphate were deposited a short time after it was taken from the bladder.

These are represented in Fig. 15. When treated with a drop of concentrated nitric acid, the whole mass of urine immediately became solid, pre-

Fig. 15.



Crystals of triple phosphate, deposited in great number in the urine of the red bat, when slightly evaporated. Magnified 210 diameters.

senting to the naked eye a white silvery, opaque, crystalline appearance. Under the microscope, it was seen to consist of a conglomerated mass of lozenge-shaped crystals of the nitrate of urea. The following are the principal constituents of the urine of the *Nysticeus Noveboracensis* :—

Urea, in great abundance.

Uric acid.

Chloride of sodium.

Phosphate of lime.

Phosphate of ammonia.

Urate of ammonia.

Purpurine.

Phosphate of soda.

Phosphate of magnesia.

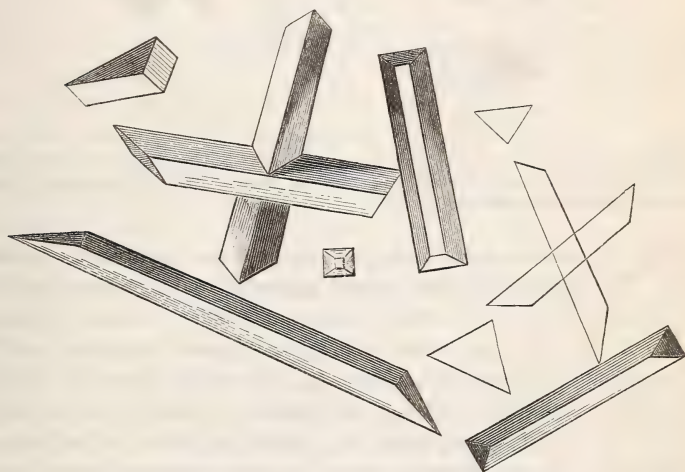
The fact that the urine of the bat contains so large an amount of urea may be due in some measure to its high temperature and rapid circulation. According to the observations of Tiedemann and Rudolphi, the temperature of the bat varies from 106° to 105° F. According to another observer, its heart beats 200 times in a minute. This rapid circulation must be attended by a correspondingly rapid disintegration of the fluids and tissues.

It is to be regretted that specimens of the urine of wild carnivorous animals can rarely be obtained. This is the case with not only the small, but also the large animals. From the bladders of ten raccoons and one mink, I was unable to obtain a single drop of urine for microscopical or chemical examination.

b. Urine of Herbivora.—In the urine of animals of this class, uric acid is replaced by hippuric acid. The amount of the solid constituents, in proportion to that of the water is much less, and the specific gravity, as a necessary consequence, much lower than in the urine of carnivorous animals.

Qualitative Analysis of the Urine of the Gray Squirrel (*Sciurus Carolinensis*).—Owing to the small capacity of the bladder of this animal, it is impossible to obtain a sufficient quantity of urine for a quantitative analysis. The urine is of an amber colour. When allowed to stand for a short time after its removal from the bladder, beautiful prismatic crystals of the triple phosphate were deposited, and were visible to the naked eye as a silvery-white precipitate. Fig. 16 represents a deposit of very long and delicate crystals of this salt.

Fig. 16.



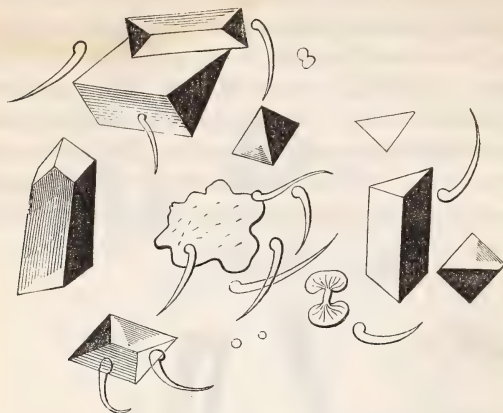
Prismatic crystals of triple phosphate, precipitated from the urine of the gray squirrel (*Sciurus Carolinensis*), which was placed upon a glass slide, and allowed to remain for one hour. Magnified 210 diameters.

Under the microscope, these differed somewhat in form in different individuals. In several instances, two of the prisms, as represented in the figure, were united so as to form a cross.

Fig. 17 represents the same deposit which took place in the urine of several squirrels collected in a test-tube. It was rutting season, and the spermatozoa are distinctly seen, many of them adhering to the crystals. A well-formed dumb-bell crystal, of the oxalate of lime, is also seen. The precipitation of these crystals results from the formation of ammonia, one of the products of the disintegration of the nitrogenized constituents of the urine. The ammonia thus formed neutralizes the small amount of free acid in the urine, which renders the triple phosphate soluble. When the urine was treated with aqua ammonia, stellate crystals of this salt made their appearance in great numbers.

The presence of urea was determined by slightly evaporating a portion of the urine, and treating it with a drop of concentrated nitric acid; this was

Fig. 17.



Prismatic crystals of triple phosphate, spermatozoa, and a dumb-bell crystal of the oxalate of lime, deposited from the urine of several gray squirrels, which was placed in a test-tube and allowed to remain for two hours. Magnified 210 diameters.

followed by the production of numberless lozenge-shaped crystals of the nitrate of urea. The quantity of urea present appeared to be greater than that which normally exists in the urine of man, and smaller than that of the red bat. When the urine was treated with nitric acid and evaporated over the spirit lamp, and then treated with ammonia, but a slight change to the purplish-red colour took place, denoting the presence of uric acid in very small amount. This acid, however, exists in a larger proportion in the urine of some squirrels. In one instance, where the urine was spread upon a glass slide and slowly evaporated, crystals of uric acid made their appearance in moderate abundance.

The presence of hippuric acid was rendered evident, by slightly evaporating the urine and treating it with acetic or hydrochloric acids. Under the microscope, it presented the form of delicate acicular crystals. When the urine, thus treated with hydrochloric acid, was still further evaporated, chloride of sodium crystallized in cubes and crosslets. In the urine of one individual, which was spread upon a glass slide and allowed slowly to evaporate, hippuric acid made its appearance in considerable abundance. The following are the principal constituents of its urine :—

Urea.
Uric acid, in small amount.
Hippuric acid.
Soda, probably combined with hippuric and uric acids.

Oxalate of lime.
Phosphate of lime.
Phosphate of ammonia.
Phosphate of magnesia.

Qualitative Analysis of the Urine of the Wood Rat (*Néotoma Floridána*).

—This animal lived about a corn-house and stable. When the urine was simply spread upon a glass slide and allowed slowly to evaporate, prismatic crystals of triple phosphate, numerous acicular and arborescent crystals of hippuric acid, and cubes and crosslets of chloride of sodium, successively made their appearance under the microscope. When treated with nitric acid, heat, and ammonia, no trace of uric acid was discovered. Urea appeared to be present only in small quantities. The addition of aqua ammonia precipitated stellate crystals of triple phosphate. The following are its principal constituents:—

Urea, in small amount.	Chloride of sodium.
Hippuric acid.	Phosphate of magnesia.
Phosphate of lime.	Phosphate of ammonia.

Qualitative Analysis of the Urine of the Common Rabbit (*Lépus Sylvaticus*), slightly alkaline in its reaction. Specific gravity 1020.—When first deposited, the urine presented a turbid, milky appearance. After standing for a time, a white, chalk-like precipitate settled to the bottom. Under the microscope, this deposit, in some instances, consists of minute globules often conglomerated together, resembling, in all respects, the urate of ammonia; in other cases, it consists of large globules. That this deposit does not contain urate of ammonia, or any uric acid whatever, is proved by the fact that it gives no purplish-scarlet colour when treated with nitric acid, heat, and aqua ammonia.

When mixed with the hydrated oxide of potassium, no ammonia was evolved; and, when heated in water, it was not dissolved. When treated with nitric, hydrochloric, or acetic acids, effervescence takes place, and the deposit dissolves, and, under the microscope, numerous acicular crystals of hippuric acid make their appearance. By close microscopical and chemical investigation, this deposit was found to contain the hippurate of soda. In the deposits, which consisted of large globules, were found dumb-bell crystals of the oxalate of lime.

The following are the principal constituents of the urine of the *Lépus Sylvaticus*:—

Urea.	Oxalate of lime.
Hippurate of soda.	Phosphate of lime.
Carbonate of soda.	Phosphate of magnesia.
Carbonate of lime.	Phosphate of ammonia.

Quantitative Analysis of the Urine of the Hare.—The urine of this animal has been examined on two occasions by Von Bibra, who collected three pints from seven or eight hares, by external pressure on the region of the bladder. The first analysis was made in December. Hippuric acid was found to be present in small quantity, forming 0.007% of the urine.

The ash contained :—

Chloride of sodium, with a little chloride of potassium	7.12
Sulphate of soda	16.82
Carbonate of soda	9.84
Phosphate of soda	53.05
Phosphates of lime and magnesia	13.17
	100.00

The following is the analysis of the urine obtained in a similar manner in the month of June. Faint alkaline reaction. In the course of six hours, crystals of ammoniaco-magnesian phosphate were observed upon the surface. Specific gravity 1050; 1,000 parts contained :—

Water	912.86
Solid constituents	87.14
Urea	8.54
Hippuric acid	microscopic crystals.
Alcohol-extract	9.58
Water-extract	32.68
Soluble salts	23.70
Insoluble salts	12.64

The ash consisted of—

Chloride of sodium, with a little chloride of potassium	22.49
Sulphate of soda	29.97
Carbonate of soda	8.73
Phosphate of soda	4.39
Phosphate of lime	12.00
Phosphate of magnesia	22.42
	100.00

The urine of Guinea-pigs resembles in many respects that of the hare, and herbivorous animals generally.

The urine of beavers, according to the qualitative analysis of Vauquelin, contains carbonate of lime and magnesia, urea, hippurate of soda, and other ordinary ingredients of the urine of herbivorous animals, but no phosphates or uric acid. He also detected in the urine the undecomposed colouring matter of the willow, one of the constituents of the ordinary food of the beaver.

The urine of elephants has been examined by Vogel, who found it turbid from carbonates of lime and magnesia floating through it. According to his analysis, it contained no trace of hippurate of potassa, but, on the other hand, urea is more abundant than in that of the rhinoceros. Brandes, who afterwards examined the urine of elephants, found hippuric acid, combined both with an alkali and urea.

The urine of the rhinoceros was also examined by Vogel. He found it turbid and yellow, resembling clay-water mixed with ochre, having a peculiar smell, not unlike that of mashed ants. When exposed to the air, it becomes

darker, and is covered by a pellicle of small crystals of carbonate of lime. It effervesces with acids, and becomes clear. When allowed to stand, it deposits a yellow powder, amounting to $2\frac{3}{4}$ per cent. of the urine, and consists of the carbonates of lime and magnesia, combined with a nitrogenized animal matter mixed with sesquioxide of iron and silica. The addition of hydrochloric acid to the urine, previously evaporated to one-third of its volume and filtered, precipitates hippuric acid to the amount of 0.45 per cent., which was previously combined with potassa. The urine of this animal also contained urea and the ordinary salts.

The urine of the pig has been analyzed by Van Setten, Von Bibra, Bous-singault, and Lassaigue. Considerable and important discrepancies exist between the different analyses, which may be due to various causes, as difference of food, relative amounts of fluids consumed by the animals, time of the year, &c. The following is the analysis of Van Setten. Urine, colour yellow, inodorous; specific gravity 1003; 1,000 parts contained—

Water	990.028
Solid constituents	9.972
Urea	0.750
Uric acid	0.195
Water-extract	1.708
Alcohol-extract	1.105
Resinous matter	0.425
Albumen and mucus	0.721
Lactic acid	0.490
Stearin	0.092
Sugar	0.375
Phosphate of soda	1.376
Sulphate of potash, chlorides of sodium and potassium	2.075
Sulphates of lime and magnesia	0.425
Sulphate of ammonia	0.196
Chloride of ammonium	0.010

The following is an analysis of the urine of a pig feeding upon potatoes and water, slightly impregnated with salt, by Boussingault. Urine alkaline, limpid, and of a pale-yellow colour; specific gravity 1013.6; 1,000 parts contained—

Water and indeterminate organic matter	979.14
Urea	4.90
Bicarbonate of potash	10.74
Carbonate of magnesia	0.87
Carbonate of lime	traces.
Sulphate of potash	1.98
Phosphate of potash	1.02
Chloride of sodium	1.28
Alkaline lactates	undetermined.
Hippuric acid	absent.
Silica	0.07

In all the analyses of the urine of the pig, that have thus far been published, hippuric acid is absent. It is a little remarkable that this acid should not be present in the urine of animals living entirely upon a vegetable diet.

The urine of the horse has been analyzed by Fourcroy, Vauquelin, Chevreul, Simon, Boussingault, and Von Bibra. It is of a straw-colour, not unfrequently turbid; at first acid, but soon becomes ammoniacal, and then emits a peculiar, penetrating odour. It deposits, on standing, a mixture of carbonate of lime and magnesia. The specific gravity varies, with different individuals, from 1030 to 1050, according to their life, whether one of labour or quietude, and also with the relative amounts of fluids and solids consumed. The mean specific gravity of the urine of horses used for agricultural purposes may be stated to be about 1045.

The following analyses of Von Bibra and Boussingault are the most recent and perhaps the most correct.

In two analyses of the urine of the same horse, made at different periods, Von Bibra found—

	(1)	(2)
Water	885.09	912.84
Solid constituents	114.91	87.16
Urea	12.44	8.36
Hippuric acid	12.60	1.23
Water-extract	21.32	19.25
Alcohol-extract	25.50	18.26
Mucus	0.05	0.06
Salts, soluble in water	23.40	40.00
Salts, insoluble in water	18.80	

The individual salts in one hundred parts of saline residue, determined upon two occasions, were—

	(1)	(2)
Carbonate of lime	12.50	31.00
Carbonate of magnesia	9.46	13.07
Carbonate of potash	46.09	40.33
Carbonate of soda	10.33	
Sulphate of potash	13.04	9.02
Chloride of sodium	6.94	5.60
Silica	0.55	0.98
Loss	1.09	

Traces of iron were observed, but he was unable to determine the presence of fluorine. The deposit which falls after the urine has stood for some time was found in three analyses to consist of—

	(1)	(2)	(3)
Carbonate of lime	80.9	87.2	87.5
Carbonate of magnesia	12.1	7.5	8.2
Organic matter	7.0	5.3	4.3
	<u>100.0</u>	<u>100.0</u>	<u>100.0</u>

The horses were used for agricultural purposes, and fed on hay and oats.

Von Bibra states that the prevailing opinion that, by excessive work, the hippuric acid is replaced by benzoic acid, is incorrect. Benzoic acid was generally absent; and, when present, could only be recognized by the aid of the microscope.

The following is an analysis, by Boussingault, of the urine of a horse fed on trefoil and vetches. Specific gravity 1037.3; reaction alkaline; 1,000 parts contained—

Water and indeterminate matters	910.76
Urea	31.00
Hippurate of potash	4.74
Lactate of potash	11.28
Lactate of soda	8.81
Bicarbonate of potash	15.50
Carbonate of lime	10.82
Carbonate of magnesia	4.16
Sulphate of potash	1.18
Chloride of sodium	0.74
Silica	1.01
Phosphates	absent.

Boussingault, although he distilled upwards of twenty-six gallons at a single experiment, was unable to obtain a trace of the red oil which several chemists have noticed as a constituent of the urine of the herbivora, and on which the colour and odour are said to depend.

The urine of horned cattle was first examined by Rouelle, who stated that it contains urea in larger quantity than that of man, and also hippurate of soda. Exposed to the air, it deposits small crystals of carbonate of lime and magnesia.

The following is an analysis of the urine of cattle, by Sprengel. One thousand parts contained—

Water	926.24
Solid constituents	73.76
Urea	40.00
Albumen	0.10
Mucus	1.90
Benzoic acid	0.90
Lactic acid	5.16
Carbonic acid	2.50
Potash	6.64
Soda	5.54
Silica	0.36
Alumina	0.04
Oxide of manganese	0.01
Lime	0.65
Magnesia	0.36
Chlorine	2.72
Sulphuric acid	4.05
Phosphorus	0.70

Von Bibra analyzed the urine of oxen employed for agricultural purposes. The urine was of a dark yellow colour, having a peculiar odour. The specific gravity varied from 1032 to 1040.

The following analyses were made with the urine of the same animal, at different times :—

	(1)	(2)
Water	912.01	923.11
Solid constituents	87.99	76.89
Urea	19.76	10.21
Hippuric acid	5.55	12.00
Mucus	0.07	0.06
Alcohol-extract	14.21	10.20
Water-extract	22.48	16.43
Soluble salts	24.42	25.77
Insoluble salts	1.50	2.22

The saline residue contained—

Carbonate of lime	1.07
Carbonate of magnesia	6.93
Carbonate of potash	77.28
Sulphate of potash	13.30
Chloride of sodium	0.30
Silica	0.35
Traces of iron, and loss	0.77
	100.00

The food of the oxen consisted of fresh clover and a little hay. The salts of the saline residue were found to vary very much in quantity in different analyses. As a general rule, the urine of oxen may be said to contain more alkaline and less earthy carbonates than the urine of horses.

Analysis of the urine of a cow fed on after-math and potatoes, by Bous-singault. When treated with an acid it effervesced, and numerous crystals of hippuric acid were deposited. Specific gravity 1040; 1,000 parts contained :—

Water and indeterminate matters	921.32
Urea	18.48
Hippurate of potash	16.51
Lactate of potash	17.16
Bicarbonate of potash	16.12
Carbonate of magnesia, ...	4.74
Carbonate of lime	0.55
Sulphate of potash	3.60
Chloride of sodium	1.52
Silica	traces
Phosphoric acid	absent

The urine of the goat was analyzed by Von Bibra. It was obtained from an animal confined in a stable, and poorly fed upon sour hay, &c. Urine

alkaline, clear, with a pungent peculiar odour. Specific gravity from 1008 to 1009.

In two instances, 1,000 parts contained:—

	(1)	(2)
Water	980.07	983.99
Solid residue	19.93	16.01
Urea	3.78	0.76
Hippuric acid	1.25	0.88
Alcohol-extract	4.54	4.66
Water-extract	1.00	0.56
Mucus	0.06	0.05
Soluble salts	8.50	8.70
Insoluble salts	0.80	0.40

The ash consisted of:—

Carbonate of magnesia, with a little carbonate of lime	7.3
Sulphate of soda	25.0
Chloride of sodium	14.7
Carbonate of soda with a little carbonate of potash	53.0
	100.0

In the urine of the camel, Chevreul discovered a large quantity of urea, but no uric acid; also chloride of sodium, hippurate of soda, carbonate of soda, sulphate of potash, carbonate of ammonia, a little sulphate of soda, and a trace of peroxide of iron. The phosphates, as well as the urates, were found to be absent.

c. Urine of the Omnivora.—Man is the only omnivorous animal whose urine has been carefully examined. We will not unnecessarily repeat analyses which have been performed by distinguished chemists, and are well known. We will only give a few analyses of Lehmann, which may be compared with those of the urine of other animals, and at the same time illustrate several important points, as the influence of diet upon the excretions of the kidney.

Analyses of the urine of a healthy, well-fed, young man, living upon a mixed animal and vegetable diet:—

	(1)	(2)	(3)
Water	937.682	934.002	932.019
Solid residue	62.318	65.998	67.981
Urea	31.450	32.914	32.909
Uric acid	1.021	1.073	1.098
Lactic acid	1.496	1.551	1.513
Water-extract	0.621	0.591	0.632
Spirit and alcohol-extract	10.059	9.871	10.872
Lactates	1.897	1.066	1.732
Chlorides of sodium and ammonium	3.646	3.602	3.712
Alkaline sulphates	7.314	7.289	7.321
Phosphates of soda	3.765	3.666	3.989
Phosphates of lime and magnesia	1.132	1.187	1.108
Mucus	0.112	0.101	0.110

15.857

15.744

16.130

Under a strictly animal diet, the urine became pale, of a straw-color, limpid, and resembled in appearance and chemical constitution the urine of the carnivora. The following are two analyses of the same man, subsisting upon animal food, made on the 28th and 30th of July, the ninth and eleventh days of the experiment:—

	July 28.	July 30.
Water	909.32	933.27
Solid residue	96.68	66.73
Urea	53.79	41.65
Uric acid	1.41	1.18
Free lactic acid	2.28	1.64
Lactates	1.67	1.02
Extractive matter soluble in water	0.82	0.61
Extractive matter soluble in alcohol	4.50	3.24
Mucus	0.09	0.11
Chlorides of sodium and ammonium	5.37	3.46
Sulphates	11.51	7.08
Phosphate of soda	5.52	4.04
Earthy phosphates	3.72	2.70

Lehmann next investigated the effects of a strictly vegetable diet. The urine had a faint odour, and a decidedly acid reaction. That, passed in the morning, was of a dark-brown colour, and rapidly deposited a mucus sediment; after which, bright-red crystals of uric acid gradually separated.

The following are three analyses which were then instituted, on the ninth, tenth, and twelfth days of the experiment:—

	August 20.	August 21.	August 23.
Water	929.10	941.91	934.92
Solid residue	70.90	58.09	65.08
Urea	23.31	22.42	25.69
Uric acid	1.17	1.01	0.89
Lactic acid	1.55	1.01	1.35
Lactates	2.39	1.89	2.06
Extractive matter soluble in water	3.80	3.07	3.71
Extractive matter soluble in alcohol	17.84	13.78	15.77
Mucus	.12	.10	.10
Chlorides of sodium and ammonium	3.80	3.07	3.71
Sulphates	7.16	7.14	7.23
Phosphates of soda	3.54	3.68	3.74
Earthy phosphates	1.22	1.09	1.11

Analyses of the urine during a strictly non-nitrogenous diet of starch, sugar, gum, and almond oil. The urine, after this diet had been continued for twenty-four hours, became of a brownish-red colour, and a slightly acid reaction, which became alkaline in twenty-four to thirty-six hours.

The following analyses were made on the second and third days from the commencement of this course of diet, in the month of June:—

	(1)	(2)
Water	953.98	965.11
Solid constituents	46.02	34.89
Urea	18.92	11.08
Uric acid	.89	.54
Lactic acid and lactates	4.89	5.11
Extractive matter soluble in water	2.80	2.71
Extractive matter soluble in alcohol	8.32	8.78
Mucus	.11	.11
Chlorides of sodium and ammonium	2.74	1.14
Sulphates	3.25	2.98
Phosphate of soda	3.01	2.48
Earthy phosphates	1.00	.91

The following table gives the mean daily amount of the various solid constituents of the urine, excreted by the kidneys, during these different systems of diet:—

	Mixed diet. <i>Grammes.</i>	Animal diet.	Vegetable diet.	Non-nitrog. diet.
Solid constituents	67.28	87.44	59.24	41.68
Urea	32.50	53.20	22.48	15.41
Uric acid	1.18	1.48	1.02	.73
Lactic acid and lactates	2.72	2.17	2.68	5.82
Extractive matters	10.49	5.20	16.50	11.85

The results of these experiments demonstrate the great influence which diet exerts upon the chemical and physical characters, and the relative amounts of the different constituents of the urine. The amount of solid matters excreted during the twenty-four hours, under a purely animal diet, was more than double that excreted during the same length of time under a non-nitrogenous diet. The amount of urea excreted under the former was more than treble that under the latter. The extractive matters were greatest under a vegetable, and least under an animal diet, while uric acid increased under the latter, and diminished under the former. If the vegetable diet had been persevered in long enough, in all probability, uric acid would have been replaced by hippuric acid, as happened in the case of a girl who fed entirely upon apples.

It is a well-known fact, that, in the urine of herbivorous animals deprived of all vegetable food, hippuric acid is replaced by uric acid. The important principles established by these experiments of Lehmann are confirmed by the differences which exist between the urine of the carnivora and herbivora. In animals living upon animal food, the solid constituents of the urine are in much larger proportion than in that of animals subsisting upon a vegetable diet. The urine of the lion, tiger, and panther contains, in the 1,000 parts, 154 of solid matter. The urine of the bat contains even a larger amount of solids. The urine of hard-working oxen and horses contains not more than from 70 to 100 parts of solid matter, whilst that of the pig contains not more than 20 parts in the same amount.

Lehmann likewise showed by his experiments that, during severe bodily exercise, urea and the lactates, sulphates, and phosphates are increased, whilst uric acid and the extractive matters are diminished.

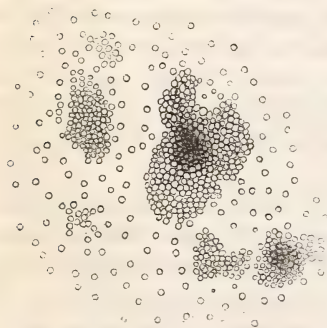
The following are the results obtained from a frequent examination of his urine during a pedestrian tour:—

	In 24 hours. Grammes.	In 1,000 parts.
Water	900.006	916.707
Solid constituents	82.594	83.293
Urea	45.314	45.697
Uric acid	.642	.647
Lactic acid	3.140	3.166
Extractive matters	8.455	8.526
Alkaline phosphates	4.598	4.636
Alkaline sulphates	15.047	15.174
Earthy phosphates	1.105	1.114

2. *Urine of Birds.*—In this class, the urine is characterized by being semi-solid, and consisting, in great measure, of the urate of ammonia, in the form of minute globules, which, when dry, resemble chalk. The following are several analyses which I made last summer.

a. *Carnivorous Birds.*—Qualitative Analysis of the Urine of the Osprey, or Fish-hawk (*Pandion Halicetus*). The urine of this bird consists of two distinct parts; a white, chalk-like precipitate, and a light yellow, ropy, supernatant fluid, resembling much in its appearance albumen. That it does not contain albumen is proved by the fact that it is coagulated neither by heat nor nitric acid. Under a magnifying power of 210 diameters, the solid white portion consists of innumerable small globules of the urate of ammonia.

Fig. 18.



Appearance presented by the solid white portion of the urine of birds and reptiles, under a magnifying power of 210 diameters. To the naked eye, this resembles chalk; under the microscope, it consists of innumerable minute granules of the urate of ammonia.

Their appearance is represented in Fig. 18. When heated in water, this chalky mass was dissolved, and deposited again upon cooling. When heated to boiling, the ammonia was driven off as a gas, whilst the uric acid was deposited in the form of rhomboidal, lozenge-shaped, and hexagonal crystals. The presence of uric acid was further determined by treating the urine with nitric acid, applying heat, and then testing with aqua ammonia; the production of the characteristic purplish scarlet colour was the result. When treated with acetic or hydrochloric acids, numerous hexagonal, lozenge-shaped, and columnar crystals of uric acid, often conglomerated together in large numbers, were precipitated. When the fluid portion was

treated with aqua ammonia, multitudes of stellate crystals of the triple phosphate were deposited. When the fluid portion of the urine was simply spread upon a glass slide, and allowed slowly to evaporate, there appeared, under the microscope, crystals of the mixed phosphates and fixed alkaline salts, together with crosslets and cubical crystals of the chloride of sodium. We were unable, by a close examination of the urine with the microscope, after it had been treated with nitric acid, to detect any traces of the nitrate of urea.

The following are the principal constituents :—

Urate of ammonia, in large amount.	Phosphate of soda.
Phosphate of lime.	Chloride of sodium.
Phosphate of ammonia.	Purpurine.
Phosphate of magnesia.	No urea.

Qualitative Analysis of the Urine of the Small White Crane (*Ardea Candidissima*).—The amount of urine secreted by this bird in twenty-four hours was three-fourths of a fluidounce. Like that of the *Pandion Haliaetus*, it consisted of a fluid portion, and a white, chalk-like precipitate. Specific gravity of fluid portion 1060. The white precipitate was about one-fourth of the whole amount of the urine. The urine gave a decidedly acid reaction, turning thick litmus blue paper promptly red. The solid portion, like that of the urine of all other birds, consisted of innumerable small globules of the urate of ammonia. When this was rubbed with the hydrated oxide of potassa, powerful fumes of ammonia were evolved, owing to its displacement by a stronger base, the potassa. When the urate of potash, thus formed, was treated with acetic or hydrochloric acids, a copious silvery-white deposit gradually precipitated, which, under the microscope, was found to consist of innumerable crystals of uric acid. The presence of uric acid, in combination with ammonia, was determined by many other characteristic tests. The solid white portion of the urine does not consist wholly of the urate of ammonia. When heated upon platinum foil, it becomes black, and gradually burns away, giving off a disagreeable smell similar to that of burnt feathers, and finally leaves a white residue. Under the continued and more direct action of the blowpipe, a large portion of this fused into semi-transparent, vitrified beads. Portions of the ignited mass were not thus fused, but continued as a white powder, which gave an alkaline reaction with turmeric yellow, and dissolved in acids with effervescence. By various chemical tests, the residue was found to consist of oxalate of lime and phosphates of ammonia, lime, and magnesia. Peniform and stellate crystals of triple phosphate were precipitated from the fluid portion by aqua ammonia. When simply spread upon a glass slide, and allowed slowly to evaporate, prismatic crystals of triple phosphate, cubical crystals, and crosslets of chloride of sodium, and crystals of the other fixed alkaline and earthy salts, were seen under the microscope.

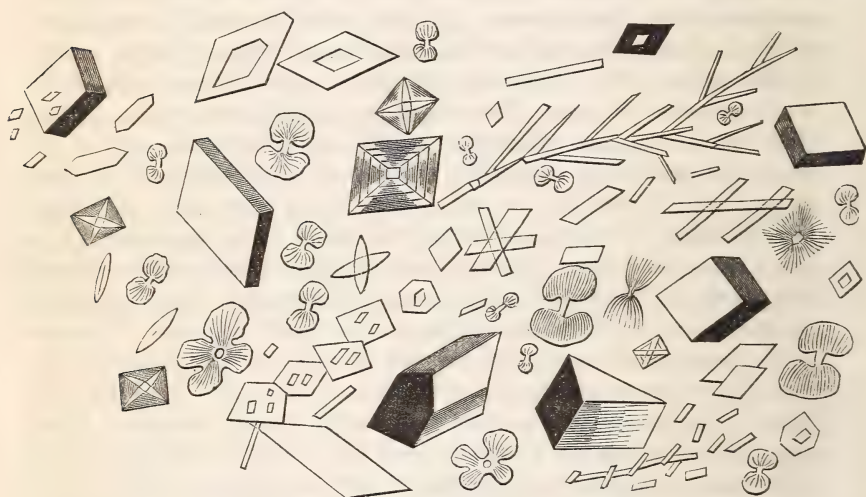
The following are the principal constituents of the urine of this bird :—

Urate of ammonia.
 Phosphate of lime.
 Phosphate of ammonia.
 Phosphate of magnesia.

Phosphate of soda.
 Chloride of sodium.
 Oxalate of lime.

Qualitative Analysis of the Urine of a young, half-grown Green Heron (*Ardea Virescens*).—The urine of this bird consisted of two portions, and resembled, in all respects, that of other birds. When the transparent liquid portion was spread upon a glass slide, and allowed to evaporate slowly, a magnifying power of 210 diameters revealed numerous dumb-bell and octohedral crystals of the oxalate of lime, lozenge-shaped crystals of uric acid, prismatic crystals of triple phosphate, cubes of the chloride of sodium, and other crystalline forms of the fixed alkaline and earthy salts. The most important and striking of these crystals are represented in Fig. 19.

Fig. 19.



Dumb-bells and octohedra of oxalate of lime, lozenge-shaped crystals of uric acid, cubes and octohedra of chloride of sodium, and prismatic crystals of triple phosphate, deposited in the clear fluid portion of the urine of the green heron (*Ardea Virescens*). Magnified 210 diameters.

The solid, white portion of the urine, like that of the *Ardea Candidissima*, was found, under the blowpipe, to contain other ingredients besides the urate of ammonia.

The following are the principal constituents of the urine of the *Ardea Virescens*:—

Urate of ammonia.
 Uric acid.
 Urea.
 Oxalate of lime.
 Chloride of sodium.
 Phosphate of soda.

Phosphate of lime.
 Phosphate of magnesia.
 Phosphate of ammonia.
 Fibrinous casts.
 Epithelial scales.

Epithelial scales and fibrinous casts were found in considerable numbers in the urine of this young bird. In this examination, we were surprised by the abundance of the crystals of the oxalate of lime.

The presence of oxalic acid in the excretions of the kidneys is always associated with some imperfection of digestion, and often of respiration. Young birds of this class are exceedingly voracious; they will gorge themselves with large and unnecessary quantities of food, and at the same time remain quiet and inactive. Here we have causes amply sufficient to account for the appearance of the oxalate of lime in the urine.

Urine of the Qua-bird, or American Night-heron (*Ardea Discors*).—The urine of this bird had the same appearance, and gave the same chemical reactions, and appeared to be constituted in all respects like that of other cranes.

In examining the urine of birds of this class, we are struck not only with the great amount of the urate of ammonia, but also with the large proportion of the earthy and fixed alkaline salts. Their presence in such abundance may readily be explained by considering the food of the birds, which consists of crabs, fiddlers, sea-conchs, and fish of all kinds, swallowed without any mastication. Although they eject the shells and bones from the stomach through the mouth, after the flesh has been removed by the gastric juice, still it is reasonable to suppose that a considerable amount of the fixed alkaline and earthy salts are dissolved from the shells, by the action of the gastric juice, simultaneously with the digestion of the flesh. The relatively large amount of the chloride of sodium must be derived from the salt water, a portion of which must be swallowed with every crustacean and fish.

The excretion of the kidneys of sea-birds is very abundant, and is the source of that valuable manure, guano. A faint idea of the immense quantities of this substance, deposited by myriads of sea-birds during many centuries, may be formed when we reflect that more than forty thousand tons have been imported to Great Britain and Ireland from the coasts of Peru, Bolivia, Chili, and Africa.

The following is an analysis, by Fourcroy and Vauquelin, of a sample of guano presented to them by Baron Von Humboldt:—

100 parts contained—

Urate of ammonia	9.0
Oxalate of ammonia	10.6
Oxalate of lime	7.0
Phosphate of ammonia	6.0
Phosphate of ammonia and magnesia	2.6
Sulphate of potash	5.5
Sulphate of soda	3.3
Sal ammoniac	4.2
Phosphate of lime	14.3
Clay and sand	4.7
Water and organic matters	32.3

The following is an analysis of Bolivian guano, by Andrew Ure:—

The soluble matter = 24 parts in 100, and consisted of—

1. Urea	5.00
2. Sulphate of potash	7.90
3. Chloride of sodium	5.00
4. Biphosphate of ammonia	5.50
5. Oxalate of ammonia	0.60

The insoluble matter = 69.5 parts, and consisted of—

1. Silica	2.25
2. Subphosphate of lime	9.00
3. Phosphate of magnesia and ammonia	1.25
4. Urate of ammonia	15.27
5. Undefined azotized organic matter, affording, with the 14 parts of uric acid, by ignition with hydrate of soda, 17.05 parts of ammonia	41.73

By comparing these analyses with those of the urine of birds, in its fresh state, we see that the urate of ammonia has almost entirely disappeared, whilst the fixed alkaline and earthy salts are relatively in much greater proportions. This is readily explained by the fact that uric acid is readily decomposed into ammonia and carbonic acid gas.

b. Urine of Granivorous and Frugivorous Birds.—In this class, the renal excretion is much less in quantity, and that of the large intestines much greater than that of carnivorous birds. In both classes, however, the urine appears to be similar in physical and chemical properties. The urine of several granivorous and frugivorous birds, I found, by microscopical and chemical examination, to consist chiefly of the urate of ammonia. Vauquelin and Fourcroy found that the urine of the ostrich contained uric acid to the amount of one-sixtieth of its weight. There were present, also, sulphates of potash and lime, chloride of ammonia, and a peculiar oily animal matter. According to J. Davy, the urine of the parrot is very similar to that of serpents.

3. Urine of Reptiles.—The urine of this class resembles that of birds, not only in appearance, but also in chemical constitution, being only a little more solid when first discharged. Here we have the urine of two wholly dissimilar classes alike in all respects. Birds have a high temperature, in many cases 10° higher than that of the mammalia. Their blood circulates with rapidity, and receives an abundant supply of oxygen. Their habits are those of activity.

Reptiles have no definite temperature; it corresponds with that of surrounding bodies. Respiration is imperfect, their lungs being, in most cases, simple sacs, around the walls of which ramify the bloodvessels. Their circulation is sluggish and imperfect, and their habits indolent and inactive.

Would any one suppose, *à priori*, that the secretions of these two classes would be similar? If the explanation given by Liebig be true, that the production of urate of ammonia in serpents is due to their imperfect respiration, and consequent incomplete oxygenation of the blood and tissues, why should

we have the same state of things existing in birds, who have oxygen supplied in large quantities, not only by their lungs, but also throughout the tissues, by means of their porous bones? It is useless to theorize upon this subject in the present state of our knowledge; we must first acquaint ourselves with the chemical constitution of the blood and tissues of cold-blooded animals, before we will be able to attempt an explanation of these apparently contradictory phenomena.

The urine of a land-tortoise (*Testudo Tabulata*), which had been starved for some months, was analyzed by Marchand. It resembled pus in appearance, and had a faintly acid reaction. He collected 1,337 grains, which gave the following analysis:—

		Or in 100 parts.
Water	1271.	950.64
Solid constituents	66.	49.36
Urea	8.5	6.40
Uric acid	23.0	17.25
Hippuric acid	none.	
Salts and indeterminate organic matter	34.5	25.70

The following is a qualitative analysis of the urine of the coachwhip snake (*Psammophis Flagelliformis*), which I made during the month of July:—

When the urine was allowed to stand for a short time, it formed a hard, white mass, which might have been readily mistaken for so much chalk. Under a magnifying power of 210 diameters, it was seen to consist of a conglomeration of innumerable globules of the urate of ammonia, resembling in all respects the solid portion of the urine of birds. Epithelial cells and fibrinous casts of the urinary tubes were also found in considerable numbers.

The following are the results of the microscopical and chemical examination of the urine of this snake:—

Urate of ammonia, in great abundance.
 Phosphate of lime.
 Phosphate of ammonia.
 Phosphate of magnesia.
 Phosphate of soda.
 Chloride of sodium.
 Epithelial cells.
 Fibrinous casts of urinary tubes.

We were unable to detect the presence of urea. I have examined the urine of other snakes, as the pine-snake (*Pituophis Melanoleucus*) and the corn-snake (*Côluber Guttatus*), and found them to resemble that of the coachwhip in most respects, consisting mainly of the urate of ammonia. We have as yet been unable to detect urea.

In the kidneys of the glossy-black pine-snake (*Pituophis Melanoleucus*), several pyriform calculi were found imbedded in the substance of the kidney, extending from the anterior surface to the ureter. These calculi were found to

consist, in large measure, of the oxalate of lime. Other substances were also present, as urate of ammonia, and the phosphates of lime, ammonia, and magnesia.

The urine of the boa constrictor has been found, by Prout, to yield more than 90. $\frac{9}{10}$ of uric acid. The urine of the rattlesnake has been examined by Simon. The following is his analysis. 100 parts of the dried residue contained :—

Free uric acid, with a little fat and extractive matter	56.4
Urate of ammonia	31.1
Urate of soda, with some chloride of sodium	9.8
Urate of lime	1.4
Phosphate of lime	1.3

This analysis of Simon is calculated to mislead. The urine before analysis was thoroughly dry, all the water having been driven off, and with it much of the ammonia combined with uric acid. In no instance have I detected free uric acid, under the microscope, in the fresh urine of serpents; it exists always in combination with ammonia. The urine of the alligator and lizards consists principally of urate of ammonia. The fixed alkaline and earthy salts exist in considerable quantities in the urine of serpents, especially the salts of lime. Their presence in such abundance may be explained by a consideration of the food and habits of serpents. They are carnivorous, and swallow their prey, often of considerable size, without any mastication. In this manner, a large amount of the earthy salts are taken into the animal economy in the form of scales, bones, claws, teeth, &c. The bones frequently remain in the intestinal canal for a considerable length of time, and they must be acted upon more or less by the gastric juice. In the rectum of a large rattlesnake (*Crótalus Adamanteus*), six feet in length, whose intestinal canal was completely devoid of food, I discovered several balls of hair about two inches in length. When these were opened, they were found to consist of a mass of broken fragments of the bones of an animal, apparently of the size of a rabbit, so carefully wrapped up in the hair that not a single angular point protruded which might injure the intestinal canal. The bones were of a yellow colour, and appeared corroded in many places. The manner in which these bones were in the intestinal canal of the living snake, carefully wrapped up, is unknown.

The urine of the bullfrog (*Rana Taurina*), has been examined by J. Davy. It is a fluid having a specific gravity of 1003, and contains in solution urea, chloride of sodium, and a little phosphate of lime.

The urine of the *Bufo Fuscus* had a specific gravity of 1008, and contained chloride of sodium and phosphate of lime, together with a larger proportion of urea than the urine of the *Rana Taurina*.

4. *Urine of Fish*.—We found it very difficult to obtain specimens of this for examination. The following is a qualitative analysis of the urine of the bassfish (*Corvina Ocellata*):—

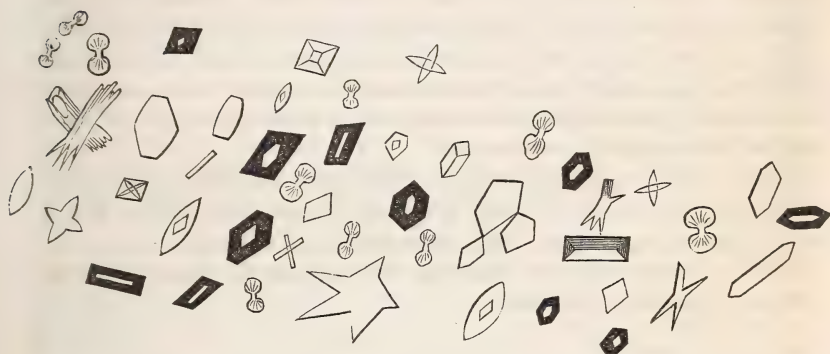
Uric acid.
Oxalate of lime.
Phosphate of lime.
Phosphate of ammonia.

Phosphate of magnesia.
Phosphate of soda.
Chloride of sodium.

When the urine was spread upon a glass slide, and allowed slowly to evaporate under a magnifying power of 210 diameters, there appeared lozenge-shaped and hexagonal crystals of uric acid, dumb-bell, and octohedral crystals of oxalate of lime, and prismatic crystals of triple phosphate.

The appearance presented by the urine thus treated is represented in Fig. 20.

Fig. 20.



Crystals of uric acid, chloride of sodium, phosphate of soda, oxalate of lime, and triple phosphate, which made their appearance under a magnifying power of 210 diameters, in the urine of the bassfish (*Corvina Ocellata*), which was spread upon a glass slide, and allowed slowly to evaporate.

2. *Urine of the Invertebrata.*

Not much attention, as yet, has been paid to this subject. The following are the results of the investigations of Siebold and others.

The urine of the *Insecta* consists of a fluid containing numerous granular masses, which is poured from the kidneys into the digestive canal, and accumulates in the colon, or in its cœcal appendage, and is excreted with the feces separately or as a turbid liquid, of a colour varying with the species.

The *Lepidoptera*, when bursting from their pupæ, emit a considerable quantity of urine of a variable colour.

In the larva and pupa of *Mymeleon* it is of a rose colour, and gradually accumulates in large quantities in the digestive tube, which the perfect insect immediately discharges on leaving the pupa envelop, as a solid, elongate, ovoid body.

Red crystals, of a quadra-pyramidal form, are perceptible in the urine of many *Insecta*.

The urine of the *Arachnoidæ* is poured by the ureters into the digestive canal, and accumulates in the cloaca, and consists of a turbid, dirty-white liquid, holding in suspension innumerable dark molecules. When treated with nitric acid, heat, and ammonia, it gives the characteristic reactions of uric acid.

In the Crustacea, the urinary secretion has been observed only in the Myriapoda. This, like the urine of insects, contains uric acid.

In the Cephalopoda, the organs, for a long time known as the spongy bodies, have been demonstrated to be kidneys. The urine is secreted from their external surface, and is of a dirty-yellow colour, and escapes immediately into the peritoneal cavities, and thence is discharged externally through their orifices.

Not unfrequently reddish crystals, formed in the urine, completely invest the kidneys, giving to them their peculiar colour.

A chemical analysis of Harless shows that they contain uric acid.

The cells of the kidneys of the Cephalophora contain a transparent liquid, in which float obscure nuclei, of a brown or violet colour. These nuclei are round or embossed, having a dense crystalline structure, and contain uric acid.

The gland of Boganus in the Acephala has been proved to be a kidney; the cells of which generally contain blue-black nuclei, which are sometimes visible to the naked eye as inorganic concretions, and, as they contain uric acid, they may well be compared to renal calculi.

These concretions in the *Petunculus Pilosus* were analyzed by Herrn Von Babo, who found them to consist principally of phosphate of lime, with a trace of magnesian phosphate and a small quantity of uric acid.

Neither kidneys nor their excretions have been, as yet, discovered in the Vermes, Zoophyta, or Protozoa.

ART. II.—*Account of the Asiatic Cholera, as it prevailed in Columbia, Lancaster County, Pa., in the Autumn of 1854.* By T. HEBER JACKSON, M. D. (With a plan of the town.)

It is only by carefully observing and recording the conditions under which epidemic cholera prevails, that we can hope to discover its cause, and the laws by which it is governed. With the hope of contributing some materials for this purpose, we have drawn up the following report:—

It is no easy matter to follow distinctly the progress of an epidemic when it prevails extensively in a large and populous city; but in a small town, its origin and progress can be more readily traced, and we shall endeavour to do this in regard to the disease as it occurred in the town of Columbia, Lancaster County, Pa., during the month of September, 1854.

It will be useful to consider, briefly at least, the situation and aspect of Columbia, and all such conditions as might reasonably be supposed to have exerted an influence upon the development and propagation of the epidemic in that town. To this end, the reporter avails himself of a communication from Dr. A. CLARKSON SMITH, of Columbia.

“Columbia is situated on the north bank of the Susquehanna, forty miles above the Chesapeake Bay, and is the terminus of two canals and three rail-

roads. The town lies on a gentle slope inclining to the northwest. Just above the town, high, bluff hills jut out into the channel of the river; while below, another ridge, running parallel with the one above, separates it from the town of Washington. North and northeast there is a valley of the most fertile land in the country. Our population is made up of representatives of almost all nations. The German and Irish, however, compose the principal portion of the foreign population. Of the latter, there are many. The railroads, canals, coal-yards, and commission warehouses give employment to so many persons of this kind, that, in their vicinity, they generally compose the bulk of the population. As a general rule, these persons drink freely of alcoholic stimulants."

The river flows with a gentle current, and for a considerable distance above and below the town is quite shallow. The surface is studded with innumerable islets covered with a rich vegetation, which adds much to the beauty of the scene, though not, perhaps, to the salubrity of the locality. Above Washington, and a short distance below the town, a dam has been constructed for the purpose of backing the water into the canal. This structure is provided with an outlet or sluice. Above the dam—in that section of the town bordering upon the river, and west of north—is the "basin," which empties its contents, by means of the outlet-lock, into the river. Between the dam and the outlet-lock are the "reservoirs" from whence the hydrants are supplied. The condition of the river, the basin, and hydrant-water will engage our attention when we come to investigate the possible causes of the appearance of cholera in Columbia. "For several weeks previous to the appearance of cholera, remittent fever had been unusually prevalent. It began generally with bilious diarrhœa, or when this looseness of the bowels was not found, there was an extraordinary susceptibility to the action of cathartic medicine. The great prevalence of remittent and intermittent fever had left many persons debilitated, and in a condition the most susceptible to the action of any epidemic influence which might chance to come upon them. As a *general rule*, persons thus debilitated were *first attacked* by cholera." Some weeks previous to the appearance of cholera as an epidemic in Columbia, the inmates of a house on Front Street, not far from the place of its reappearance, were attacked by the disease in its most fatal form, and several fell victims to its violence. This building was destroyed by order of the town authorities, and no new cases occurred until Wednesday, the 6th of September. "At this time," writes Dr. Smith, "the emigrant train, west, left at our depot two German emigrants, sick with cholera. One of them died during the night; the other, a boy, seemed to recover from it, but afterwards died with what, I have no doubt, was ship fever; the vessel in which they crossed the Atlantic having had that disease, with cholera, on board. On Friday evening two cases occurred among our own citizens, both of whom were unhealthy men; one of these having suffered with diarrhœa for two weeks previously. At midnight, it made its appearance in almost every section of the town, and at daylight there were *thirty cases*, all of which proved fatal during Saturday. Of those who fell under my care, I recognized many whom I had noticed in the room with the German emigrants during their illness. Most, if not all of these persons had diarrhœa the day previous, but as there was nothing to alarm them, paid no attention to it until too late. Most of them were sinking into collapse when first seen by the physicians."

The class of the community chiefly affected by the disease was composed of German labourers. But on this fatal night *all* portions of the town, *all* classes of persons were compelled impartially to contribute victims to the

merciless pestilence. By referring to the accompanying plan of the town, this will clearly appear. A panic now seized upon the citizens, and many of those whose means enabled them to leave, fled from the devoted town. By Monday, it is calculated that more than half of a population, numbering some 5,000, had left, and numerous persons left daily, until the week was far advanced.¹ It is necessary to mention this, lest the erroneous conclusion might be drawn, that the working class was much more obnoxious to the disease than they really were. It is fair to presume, from the history of the epidemic during Friday night and Saturday, that, had all the citizens remained, no distinction of *class* would have availed as a protection, but all would have suffered alike, in proportion to their numbers. Upon those whose health was vitiated by intemperance, and the many privations attendant upon poverty, the arrows of pestilence might be expected to fall with fatal effect; but it did not appear, on Friday night and the succeeding day, that these conditions served to invite or determine the direction of their flight.

It is proper, now, to investigate the cause of the appearance of cholera in Columbia. This town is peculiarly liable to those morbid influences which are supposed to generate malarious diseases. Accordingly, we find the physicians of the place report the almost constant prevalence amongst them of intermittent and remittent fever, and, did the statement need confirmation, it is to be found in the appearance of the inhabitants. An unusual continuance of dry and very warm weather had lessened the volume of the river to a degree unknown for years; had exposed and promoted the decomposition of the vegetable matter on its banks and islets, and diminished the rapidity of its current. And, in addition, it is stated in a Columbia paper that "the canal had been made the receptacle for animals dying in the cars while being transported to the eastern markets, thrown in during the night, and washed to the shores to putrefy in the blazing sun." The sluice being closed, by direction of the Tide-Water Canal Company, the escape of the double products of animal and vegetable decomposition was most effectually prevented. Upon the sluggish waters of the river, therefore, and upon its banks and islets were to be found, in even greater than usual abundance, the noxious properties, which, co-operating with a uniformly high temperature, are so generally supposed to generate malarious diseases. And there were very many sick with bilious fever, intermittent and remittent; and there were likewise many cases of bilious dysentery and diarrhœa. This might have been anticipated. The history of this year, so far, differed in no essential respect from that of former years. For the same causes, real or apparent only, had always been present at this season of the year, in a state of greater or less intensity, and like diseases, as effects, attended. In degree, as in kind, effect answered to cause; for the condition of the river and its precincts being such as to afford more than the usual amount of miasmata—"for several weeks previous to the appearance of cholera, remittent fever had been *unusually* prevalent." Cases of the diseases mentioned, continued to occur up to

¹ It will not be amiss to mention that many generous men having removed their families, returned that they might aid the sick.

Friday, when the cholera broke out; they *then* disappeared, and reappeared only when the cholera epidemic was abating.

In former years, the diseases peculiar to the several seasons were impressed with the leading characteristics of the malarious fevers; this was true in regard to pneumonia, functional derangements, and organic lesions of the stomach and bowels, and also entero-mesenteric fever. But now, cholera has entirely superseded the malarious fevers, and is, by no means, impressed with their characteristics. The two diseases are essentially different, and will hold no intercommunication. Are they, then, effects of the same cause? The hygrometric and thermometric condition of the atmosphere continued the same; the state of the river, of its putrid burden and noisome shores, was unaltered—at least there was no apparent change; and yet the usual and concomitant fevers have suddenly disappeared, and, in their stead, is this strange pestilence! Some were satisfied to assign miasmata as a sufficient cause. Strength, though not actual confirmation, will be lent to this hypothesis by a knowledge of the following curious phenomenon: On Friday, about noon, a southeast wind began to blow—of course, blowing over the river directly into the town. On the afternoon of this day, the first case of cholera occurred. This wind prevailed steadily until Sunday morning, when it changed to the northwest, blowing in a direction directly from the town to the river. A very sensible diminution in the number of new cases was observed at once, and when the wind veered to the original quarter (southeast), as sensible an increase was perceived. This curious coincidence (?) was noticed *at the time* by several. Indeed, it was a matter of general remark, and the “direction of the wind” was being constantly questioned with marks of deep interest and solicitude. But remarkable as this may appear, before it is determined that emanations from the river, wafted into the town by this southeast wind, were productive of the cholera, it will be worth while to remember that during a long series of years Columbia had been exposed to precisely the same influences, the same combination of circumstances, and yet remained happily free from cholera. It is not denied that the condition of the river air, probably impure, may have afforded a suitable nidus for the disease, and that the wind, by wafting the exhalations—the blastema, so to speak—into the town, may have thus exerted a powerful agency in the advancement of the epidemic. Moreover, if the river and its shores are to be accused of having generated the cholera poison, why and how did the people of Wrightsville, on the opposite bank from Columbia, escape? especially when, as on Sunday, the strong northerly wind was blowing? And yet escape they did, without a single case. In addition to this, it is to be observed that Washington, on the *same* side of the river with Columbia, and distant only one or two miles, also escaped. When the cholera had disappeared from Columbia, a few cases were reported in Washington.

It may be said that Washington was below, and separated from the area of the peculiar miasmata, by the dam. But few would care to repose upon such a protection. We have already described the position of the basin and canal;

by some it was contended that their impure waters mingling with the river water, and being then pumped up into the reservoirs, and used by the inhabitants for drinking, and for culinary purposes, were alone concerned in generating the epidemic. But it was well known to all the physicians that very many who confined themselves, from habit, to the use of pump-water, were attacked with fatal effect. This is surely a sufficient refutation. Perhaps the most popular explanation was that which attributed the appearance of the malady to contagion. There is much to be said both for and against this hypothesis. In favor of the affirmative it may be urged that the appearance of the epidemic was subsequent to the arrival of the two emigrants, one of whom died of the disease; that many of those who were in attendance upon these aliens were amongst the first to be attacked; and, lastly, that many cases can be instanced, in which, owing to peculiar circumstances, the disease *could* have been contracted in no other conceivable way than by contagion.

The first proposition, that of the sequence of the local epidemic upon the arrival of the imported cases, is true enough. Unless taken, however, in conjunction with, and supported by other and more weighty facts, it is really worth nothing; for it is obviously unfair to conclude that, because the cholera appeared *after*, it must have depended for its existence upon the arrival of the emigrants. But this argument for contagion acquires importance when taken in connection with the next fact, viz: that many of those attacked had communicated directly with the sick emigrants. "Of those who fell under my care," says Dr. Smith, "I recognized many whom I had noticed in the room with the German emigrants." But "at *midnight* the cholera made its appearance in *almost every section* of the town, and by *daylight* there were *thirty* cases, all of which proved fatal during the same day." Now, amongst these *thirty cases*, what more likely than that some, or even "many of those seen in the room with the German emigrants," should have been found? Doubtless, there were *many more* drawn by curiosity, or more laudable motives, about the emigrants who were *not* attacked. But, above all, it is worthy of notice, that cases of Asiatic cholera had occurred in Columbia previous to the arrival of the emigrants; and if contagious *now*, the disease did not appear to be so at that time.

After the arrival of the reporter in Columbia, on Sunday the 10th, he neither heard of, nor was himself conversant with any case that could be fairly traced to contagion. Of all the physicians (except the much to be lamented Dr. Cochran, who was seized on the first, and died on the second day of the epidemic), officers of the sanitary committee, and nurses in attendance upon cholera patients in private practice, or at the Town Hall, but *one*, a nurse, was attacked! And yet all these were most zealous and constant in attendance upon the sick. The emigrant who died of cholera, died on the night of the day on which he reached Columbia; no one of the citizens was attacked before Friday evening. Is this, then, to be considered the duration of the incubative stage of cholera, if contracted by contagion? Perhaps the period of incubation varies—for, between midnight and daylight,

there were thirty cases. But did the disease spread as those known to depend upon contagion are propagated, viz: from one to another, at appreciable intervals of time? No: "at midnight it made its appearance in almost every section of the town, and by morning there were thirty cases!"

Surely, this malignant disease which thus, in a few hours, invaded the whole town, could not have been produced by contagion, which requires either actual contact, or at least the reception into the body of some emanation from an individual affected with a similar disease.

But all of the thirty cases had not been in contact with the emigrants, and it is probable that not more than two or three had been so actively engaged about the persons of the sick, as were the numerous physicians and nurses, and yet but one of the latter died! Moreover, it is necessary that these poisonous emanations from the person of one labouring under a contagious disease, should not only be inhaled, or otherwise effect an entrance into the body, but that they should likewise undergo some change, in which the body participates before it can become a new focus of contagion. This change requires time, which varies much according to the susceptibility of the individual. But, on Friday night, thirty were seized almost simultaneously.

Contagious diseases do not seize upon great numbers at once, but progress from case to case. Therefore, it does not appear that the disease was simply contagious, nor that the cause of its extensive spread was of an epidemico-contagious character. Doubtless, there was some modification of the atmosphere favourable to the extension of the disease *in the town*; but this appeared to be connected with some *local* condition, for those living in the country and near to the town, escaped altogether. In company with Dr. Smith, the reporter visited many individuals in the country adjacent to, and within a mile or two of Columbia; these were affected with various ailments, and one, a gentleman advanced in years, and liable to derangement of the bowels, appeared to have cholera; but he readily recovered under the use of simple measures. The reporter believes this to have been the only case of the kind in the country. Communication with the country, during the first three or four days of the epidemic, was indeed very limited.

Still, physicians from the town visited their patients in the country, living near to Columbia, and yet the disease, if portable, was not conveyed to any one.

Hence, it would appear that the cause of the appearance and spread of malignant cholera in Columbia, was manifestly connected with the air and locality; that it was eminently endemico-epidemic.

Before quitting this subject, it will be proper to give some individual cases which would appear to favour the doctrine of the contagiousness of Asiatic cholera: "A gentleman from Bainbridge visited this place during the prevalence of cholera, returned home, a distance of seventeen miles, took the disease and died. His family fled, and he was left in charge of a friend who also took it, as did another who assisted in his burial. Neither of these persons had been to Columbia, nor at any place where they could have contracted it save from his person." Isolated cases of this kind by no means prove

that cholera is contagious, though they serve to render it probable. They should not, however, be regarded apart, but in connection with the host of facts rising up on every side to prove the opposite, and amongst which so vast a multitude, a few eccentric phenomena might well be supposed the result of accident. Hundreds left Columbia during the height of the epidemic some of these died of the disease away from home; but, so far as the reporter could learn, in only one or two cases did there appear to be a communication of the disease to others.

To attempt the description of a disease which has been so frequently and faithfully portrayed by able writers, that one conversant with their writings cannot fail to recognize it, would be superfluous. It will be sufficient to mention some modifications under which the disease appeared in Columbia. Premonitory symptoms were present in nearly all the cases, for periods of time varying from several days to a few hours. The advent of the disease found many labouring under bilious diarrhœa; many also, weakened by recent attacks of remittent fever; "and, as a *general rule*, persons thus debilitated were *first attacked* by cholera." The characteristic discharges from stomach and bowels were present in all the cases; in some, profuse; in most, however, remarkably moderate, and yielding with unusual facility to the agents employed to arrest them. But the welfare of the patient oftentimes did not appear to be promoted by their suspension, nor could a favourable prognosis be founded upon the circumstance of their being small in quantity. While thus mild in the particulars specified, the case frequently presented all the other worst symptoms of the disease, viz: a rapidly failing pulse; hurried respiration; anxious expression of countenance; altered tone of voice; increasing coldness, and diminishing resiliency of skin; great restlessness, and violent cramps. The nervous symptoms were the most prominent. Hence, the poison may be supposed to have acted directly upon the nervous system, without its usual irritant action on the mucous membrane of the alimentary canal; or else, that the effusions really took place into the bowels, but were retained. Opportunity to determine the last by actual inspection did not offer; but the physical signs failed to indicate the presence of a large amount of fluid in the bowels. The altered voice, when present, was regarded as most unfavourable. The cramps were chiefly confined to the muscles of the lower extremities, but in some extended to the dorsal, abdominal, and in a few to the brachial muscles. These spasms induced extreme suffering. When reaction succeeded to the stage of collapse, it was usually attended with the greatest danger, from the liability to cerebral congestion; this was more especially the case with children, of whom many were attacked.

In the treatment of this disease—many agents, of the most diverse characters—many methods, some with, some without a reason, or a good one—were tried. But, from the results, could be gleaned no argument in favour of the empirical practice. Indeed, the success of rational treatment was most gratifying—inasmuch as many were saved, who, for the lack of it, could not have recovered. In the stage of collapse, even, means were sometimes found to

restore to the blood its lost elements, and cause the heart again to propel its life-giving contents to the exhausted capillaries. Such general measures as experience and observation had recommended, were adopted by the Sanitary Committee; these it is unnecessary to enumerate. By the delegates from the College of Physicians of Philadelphia, the resident and visiting physicians, a series of instructions were drawn up, and, by the Sanitary Committee, printed for general distribution. A notable improvement in the health of the town followed their publication, from which, perhaps, a faithful compliance with the instructions may be inferred. (?)

It would be alike tedious and unprofitable to mention all the means and drugs employed in the treatment of the disease.

In view of the great discrepancies that exist as to the pathology and nature of the disease, that treatment was the safest, and no less successful, which was based upon indications drawn from the symptoms. If the physician was so fortunate as to see a case in its incipency, he seldom experienced difficulty in arresting its progress. The diarrhœa, unless clearly to be traced to the presence of irritating ingesta, was met at once, and successfully with astringents, the particular article of the class used being determined by the preferences of the physician.

Opium, or some one of its preparations, was almost always used in conjunction with a mineral or vegetable astringent. Large opiate enemata were used with the greatest advantage when everything else had failed. It may be observed now that throughout the treatment of the disease, perfect rest and quietude in bed were deemed essential to success.

In large or small doses, some preparation of mercury was very generally employed, and that throughout the attack. In the first, second, and third stages, to restore the secretions, especially that of the liver; in the fourth stage, that of reaction, as a sedative, and for its general alterative action.

Unfortunately, a large number of patients were, when first seen by the physician, already in a collapsed state, and the end, not far distant, was but too frequently fatal. Nevertheless, some even of the worst of these recovered under judicious management. The great object in this stage was to prevent the further loss of the serum of the blood, to allay nervous irritability, and support the system until it could react. To accomplish the first indication, the most powerful astringents were used, preferably by injection; while, to supply the waste already sustained, ice-water in small quantities at a time was freely given, a salt of soda in small doses being added from time to time. Very large doses of opium were given in this stage, without inducing any impression whatever; when carried still further, a comatose condition was induced, which would cease in a short time, if the narcotic was discontinued.

Though the application of friction and warmth to the surface was markedly disagreeable to some, many yet seemed to derive relief from their use. These applications oftentimes seemed to lessen nervous excitement, to have a quieting, soothing effect upon the patient; and, certainly, were the most useful of

all the agents employed for the relief of the cramps. These spasms were most rebellious in spite of all that could be done to control them. Bandaging failed to afford more than very temporary relief. Beef-tea, animal broths, salted to the taste of the patient, and likewise various stimulants were given to support the strength and equalize the circulation, so far as the condition of the blood would permit.

Perhaps sufficient caution is not used in the exhibition of stimulants. In some cases—those in which the blood, robbed of its serum, has become of the consistence of a syrup—it is vain to think of deriving advantage from stimulants. If the stomach will tolerate the intrusion of dram after dram of alcohol, and quickly recurring doses of ammonia, what good will their presence in the stomach effect? Can they impart normal fluidity to the blood, or enlarge the caliber of the arterioles and venules. Even if they could greatly increase the force and frequency of the heart's action, which, fortunately, they cannot, would it not be dangerous to employ them? For a fluid of the consistence of that now found in the heart and large bloodvessels, cannot be forced through the capillary system, without rupture of these delicate vessels and consequent extravasation. Moreover, the blood in this condition is not fitted to fulfil the functions for which it was designed; therefore, care should first be taken to repair it, and it will then circulate readily enough. But, in those cases in which the discharges have not been very large, internal and external stimulants may be used with advantage. But here something is wanted which will produce a sudden and powerful impression upon the whole system; and, to this end, the reporter believes nothing will be found to surpass the hot salt-bath in efficacy. In conjunction with Dr. Smith, the reporter made frequent trial of this agent, and the result was highly pleasing to both. It was not until the epidemic had continued some days, that this agent was had recourse to, otherwise it would have been employed much more frequently.

“In the stage of collapse,” writes Dr. Smith, “the hot salt-bath was more efficacious than anything I am aware of. Seven apparently desperate cases recovered after being immersed in it. One of these had been in collapse full thirty hours.” The water should be raised to a temperature the highest compatible with safety. Prior to the immersion, the patient should take some stimulus, whatever has proved most grateful to his stomach. On removal from the bath it would be well to repeat the excitant, while, at the same time, friction with coarse towels should be practised, and the patient enveloped in blankets. Heated bricks, wrapped in some coarse material, which has been steeped in water, should be disposed along the person of the patient beneath the bed-clothes. For adults, it is necessary that the temperature of the bath should exceed 98° Fahr. Children, having a delicate cuticle, and being much more impressible, do not need so high a temperature. The patient, subjected to this treatment, more especially if a child, should be closely watched, lest reaction run too high; or, having lasted a short time, should begin to be followed by depression. This did not happen in any of the cases seen by the

reporter, but it is well enough to be on our guard against the possibility of its occurrence.

It is well known that patients in the third stage of cholera, though with a cold surface, suffer extremely from hot, or even warm applications; hence the use of the hot bath is not unattended with pain. This, though to be regretted, should not deter us from employing it when it is indicated. In children, when the disease could not be cut short, it was a comparatively easy matter to conduct them through the first three stages of cholera; but the fourth was fraught with the greatest danger. "The difficulty in managing children in this disease was not in checking the vomiting and purging, nor even in rescuing them from collapse (a simple astringent mixture, and placing them in the hot bath, being mostly sufficient to accomplish these purposes), but in preventing an attack of congestion of the brain, which often proved fatal." By the early employment of topical depletives, of derivatives, and the alterative action of mercury and ipecacuanha, this morbid condition was sometimes overcome. The reporter believes general bloodletting was practised in only one case, and then at so late a stage of the attack that but little hope could be reposed in any remedial measure whatsoever. When the cholera made its appearance in Washington, Dr. Smith made use of venesection, and the result is given below in a quotation from his letter to the reporter:—

"In the second or serous stage, I know of no remedy equal to the lancet; and I am only sorry I did not use it when the epidemic first made its appearance, so as thoroughly to have tested its efficacy. It was not until after the disease ceased to prevail as an epidemic that I determined to try its effects, and in five cases in which bleeding was practised all rapidly recovered. I was called to see a young married lady, of full habit, who had had diarrhoea for forty-eight hours, and the morning I visited her had the characteristic serous discharges from the stomach and bowels, attended with most distressing cramps in the extremities. Altogether, it was as severe a case of the second stage of cholera as I saw during the epidemic; and feeling assured she would die under the ordinary treatment, I determined to try the effect of bleeding. The blood was drawn in a full stream from a large orifice, and when she had lost $f\frac{3}{4}$ xvj, she complained of feeling faint. The vein was closed, and gr. $\frac{1}{4}$ morphiae sulph. given. After this, she had not another cramp; the purging and vomiting ceased, and with the above-mentioned combination of soda and water, she rapidly recovered. Subsequently, I bled four others with like results."

Should this report appear meagre and too general in its character, the fact that no notes were taken by any of the physicians will sufficiently account for it, as well as for deficiencies and errors, should any appear.

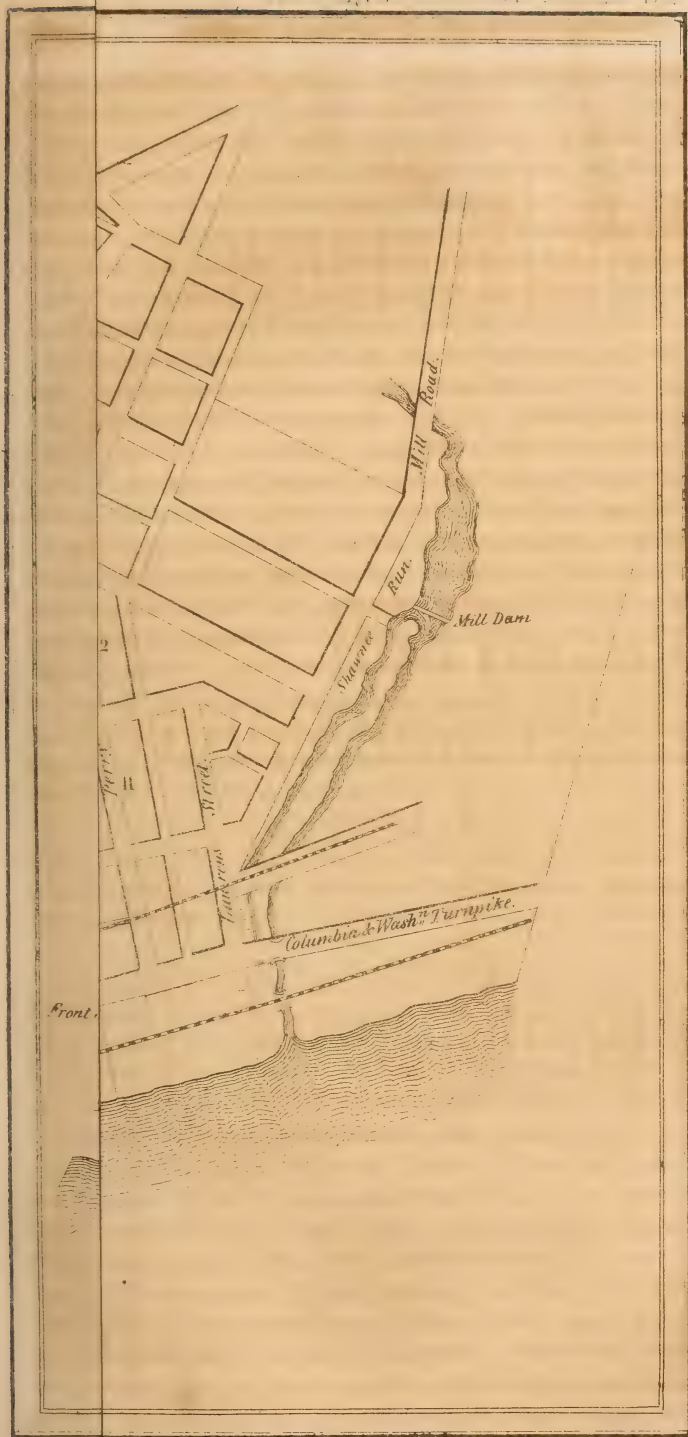
The reporter feels it is but just he should state that he spared himself no trouble in the endeavour to obtain accurate information.

On the accompanying plan of the town, has been indicated by numerals, the position of the residences of fourteen of those who were seized on the evening and night of the 8th of September. The numerals indicate also the order of succession in which the cases occurred, or rather were seen by the physicians. They took place in very rapid succession, almost simultaneously, indeed. By referring to the table, a synopsis of each case will be found.

No.	NAME.	RESIDENCE.	TIME OF ATTACK.	TIME OF DEMISE.
1	Francis Bradley,	Front Street	Friday afternoon	Died in a few hours.
2	E. A. Howard,	Front Street	Friday evening	Saturday.
3	Mrs. Wm. Hippey,	Cherry, between 4th and 5th Streets	Friday night, visited No. 1, on Friday afternoon	Saturday at 8 A. M.
4	Dr. R. E. Cochran,	Walnut Street	Saturday morning early, had diarrhoea two days previously.	
5	Margaret L. Hagan,	Walnut Street	Friday night	Saturday, at 12 M.
6	James Keely,	Harkin's Tavern	Friday night	Saturday, at 8 A. M.
7	Mrs. S. Felix,	Fourth Street	Friday night	Saturday, at 10 A. M.
8	Mrs. S. Atkins,	Union Street	Friday night	Saturday, at 1 P. M.
9	Mrs. Richards,	Third Street	Friday night	Saturday, at 12 M.
10	Robert Spratts,	Fourth Street	Friday night	Saturday.
11	Peter Renler,	Perry Street between 2d and 3d	Friday night	Saturday morning.
12	John Gheen,	Perry Street between 3d and 4th	Friday night	Saturday night.
13	Lorenzo Krab,	Third Street between Union and Perry	Saturday	Sunday.
14	J. J. Strickler,	Herr's Hotel	Saturday morning	Saturday.
15	H. H. Lichty,	Locust Street	Friday night	Saturday.
16	Samuel Hinkle,	Union Street	Saturday morning	Saturday.
17	Mrs. S. Lysle,	Lauren's Street	Saturday morning	Saturday.
18	John Gilbert,	Perry Street	Saturday morning	Saturday.
19	Miss Ann Harnly,	Locust Street	Saturday morning	Saturday.
20	Mrs. C. David,	Union Street	Saturday	Saturday.
21	John Boyd,	Locust Street	Saturday	Saturday.
22	Chas. Benner,	Union Street	Saturday	Saturday.
23	— Weaver,	Minnich's Tavern	Saturday	Saturday.
24	Chas. Jackson (col'd),	.	Saturday	Saturday.
25	Webster Fox (col'd),	.	Saturday	Saturday.
26	Mathias Neidinger,	.	Saturday	Saturday.
27	German, boy,	Union Street	Saturday	Saturday.
		Name and residence unknown	Saturday	Saturday.

The reporter has not marked on the plan the position of the residences of the last thirteen cases given in the table; he could only ascertain the street in which each lived.

The total number of deaths from cholera, as reported, was 127.



PLAN
OF THE TOWN
OF
COLUMBIA, PA
Illustrative of the Cholera Epidemic
of September 1854.



ART. III.—*Extracts from the Records of the Boston Society for Medical Improvement.* By WM. W. MORLAND, M. D., Secretary.

November 13, 1854. *Unusual Appearances in the Larynx in two Cases of Typhoid Fever.*—Dr. J. B. S. JACKSON showed the specimens, taken from hospital patients. 1. Erosion of the epiglottis, the whole top of which has been entirely removed. The cartilage stands up, quite denuded, and of a dull-yellowish bronze colour, nearly or quite a line above the termination of the mucous membrane, which last appears perfectly healthy. Contrast between the apparently dead cartilage and that below the line of erosion was striking. The larynx was otherwise healthy; but in the pharynx, and near to the glottis, was an irregular, thin, quite defined ulcer, through the mucous membrane, and about two-thirds of an inch in extent. The patient, a man, æt. 23, had been sick about four weeks; local symptoms mainly pulmonary, and nothing was known of any affection of the throat. Congestion of the lungs was found, with the usual affection of Peyer's glands.

2. Something like a gangrene of the mucous membrane upon the inside of the larynx. The appearance is confined to the left side, and is nearly limited in extent by the thyroid cartilage. The affected tissue retains its polish upon the surface, and its consistence and thickness are not perceptibly changed; but there is a tawny opaque hue, which is not merely strongly marked, but perfectly defined, and showing even some indications of a line of separation. There is no effusion into the cellular tissue beneath this patch, and little or no redness of the mucous surface about it. From the vocal cord to the top of the larynx there are the appearances of inflammation; but still, they are confined to the left side, the right being quite healthy. The parts are much swollen, flabby to the feel, and upon the rima glottidis is an ill-defined, superficial, tawny discoloration, as if threatening to slough. About this discoloration are some small, vividly red spots; but otherwise there is no redness of the mucous membrane, nor any other unusual appearance. Upon incision, there is very little effusion of serum into the swollen parts, and no pus.

The patient was a man, 32 years of age, and had been ill five weeks, but without any especial local symptoms. The voice was rough for the last two weeks, during which time he was in the hospital; but disease of the larynx had never been suspected. In the intestines were the usual appearances of Peyer's glands.

Death from Hæmoptysis.—Dr. CHARLES E. WARE reported the case. The subject was a young man, 35 years of age, who had his first attack of hæmorrhage in the autumn of 1852. Dr. W. was called to him in July, 1853. He had at that time an attack of hæmoptysis; quantity about half a pint. He had no cough, and had had none. He had not recently lost flesh nor strength, but rather gained. He had distinct crepitus, dulness on percussion, and resonance of the voice under right clavicle. He kept about his business, and the hæmoptysis was not renewed at the time. Dr. Ware was called to him again, November 6, 1854. He had enjoyed nearly his usual health, which was not very reliable, until the day previous, when, walking in the street in Lowell, he was seized with quite a copious hæmoptysis. He got into a carriage, drove to the cars, and came to Boston. The hæmorrhage had ceased, and he arrived in his usual health. A physical examination of the chest was postponed until

the excitement attending the accident should be past. He remained quiet until the 10th, when, feeling well enough, he thought, to take a walk, he went out into the street. He had only walked about a quarter of a mile, when he was attacked by hemorrhage, and died immediately, apparently from suffocation; for the loss of blood was not more than half a pint. No examination was obtained.

Dr. PARKMAN mentioned a case observed by him at the Massachusetts General Hospital, in 1836. The individual had only once or twice noticed that his sputa were bloody; he was much alarmed, and, while Dr. P. was conversing with him, endeavouring to divert his fears, he was suddenly seized with violent hæmoptysis, and died almost instantly.

Dr. COTTING said he was called, two years since, to see an omnibus-driver, who was ill with phthisis, but well enough to go out. He was at a stable, looking on at the work, was seized with hæmoptysis, and was immediately taken into his own house, not more than a gunshot's distance, where he was visited in a very short time by Dr. C.; who, however, found him dead.

Dr. JACKSON referred to certain similar instances; but said that, in all he had seen or known of, phthisis was far advanced.

[The Secretary remembers an instance in which the disease was advanced, but the patient was in the habit of being up daily, and had, not long before the fatal bleeding, been out. Summoned in the night to this person, whose residence was only across the street, he was found dead, although not more than six or eight minutes could have elapsed. When the messenger left, the patient was bleeding copiously, but had spoken only a short time previously.]

November 27, 1854. Membrane, of Tubular Form, passed per Anum.—Reported by Dr. HOMANS, Sen. The patient was an infant, about six months old in August last, when it was attacked with dysentery, and seen by Dr. H. in consultation. The dejections varied in frequency from ten to thirty-five in the course of twenty-four hours, attended with tenesmus and corresponding constitutional symptoms. They consisted at first of fecal matter with mucus, then of mucus tinged with blood, and finally blood alone was to be seen. On the ninth day the symptoms began to abate, the dejections had become less frequent, and some fecal matter had appeared in them.

On the sixteenth day, after much straining, a mass supposed to be composed of lymph was suddenly protruded from the anus, to the extent of about six inches, according to the report of the attending physician. Efforts were made to remove it by the fingers, but, great resistance being offered, it was cut off by scissors near the anus, a few drops of blood only following the incision. Two or three days after this, a second portion was expelled, by the natural efforts of the child, about one inch in length. Prolapsus ani followed daily for six days, and has since occasionally recurred. The little patient gradually and steadily improved in every respect, and now appears as well as other children of the same age. The mass, after its removal was found to be a hollow tube, which was cut open and then placed in alcohol by the attending physician. It is now about four inches in extent, and of considerable consistence. The surface corresponding to the inside of the original tube is smooth, the other rough, with what appear to be vessels running in different directions. It has been examined by Dr. JEFFRIES WYMAN, who reports it to consist of mucous membrane, vessels, and areolar tissue. He also found what he supposed to be circular muscular fibres under the microscope, and has passed bristles into an artery and a vein.

Singular Movement of the Pupils, corresponding with that of the Eyeballs.—Dr. BETHUNE reported the case. John G., aged 13. Health good, but is “nervous,” especially on excitement. The eyes oscillate considerably, as is frequently seen in congenital cataract. This has only been noticed for six years past. The sight is generally good, but not quick, and he is rather near-sighted; and occasionally he has a “blur” before the eyes. On examination, the eyes are generally well-formed and full, but the sclerotica runs into the cornea at the margin. The pupils play constantly as the eyes move, dilating and contracting—the right, however, being more active than the left, which is not noticed in the oscillations.

Ellen G., aged 16. Sister of the above. The same malformation at the edge of cornea as in the brother, but the eyes are small and badly developed. She is very near-sighted, and can see neither to read nor sew. She has the same movement of the globes as observed in her brother, but much less in degree, and only of late noticed by the family; also the same corresponding movement of pupils. Her health is good. These are two of eleven children; none of the others have any defect in the eyes, and the parents are not related by blood.

Overdose of Opium in a Child; Olive Oil probably Preventive of Absorption.—Dr. GRAY related the following case, communicated to him by the physician in attendance. E. G., aged 8 years, a resident of Brooklyn, took a dessertspoonful of laudanum, the same being mistaken for tincture of rhubarb. The error was instantly discovered, and the child made to swallow, in addition to a quantity of mustard and salt, four ounces of olive oil. Ipecacuanha was shortly afterwards administered. Dr. S. arrived in about twenty minutes after the opiate was taken; found the patient sitting up, not much alarmed, and apparently free from narcotism. Sulphate of zinc and ipecacuanha, of each a scruple, were directed, causing copious vomiting in the course of ten minutes. The patient then manifested a tendency to sleep, but wakefulness was maintained by conversation. This tendency continued for the space of five minutes only, after which she was perfectly well in all respects. She had repeatedly taken three drops of laudanum, with good results, when suffering from pain similar to that complained of in this instance. The opiate was swallowed when the stomach was empty. Was its absorption prevented by the oil?

December 11, 1854. Spontaneous Dislocation of the Crystalline Lens in both Eyes, without loss of Transparency; Frequent Prolapsus of the Lens into the Right Anterior Chamber.—Dr. WILLIAMS presented to the Society a patient, about thirty years of age, whose eyes exhibited the above extraordinary phenomena. She is of feeble constitution. At an early age she had measles, scarlatina, and variola, in immediate succession. Her sight has always been imperfect, and she has been able to see small objects—as in reading or sewing—only when held very near her eyes. On the 8th of June last, she saw a circle—light in the centre, dark around its edges—which interfered with vision in her right eye; but no other change was observed till about a week before she was seen by Dr. W. At this time, in stooping to lift a tub, she was conscious that something occurred in her right eye, and soon experienced circumorbital pain and nausea. She afterward noticed a peculiar appearance in the anterior chamber. During the night these symptoms vanished; but vision was not as good, the following day, as it had previously been. The same phenomena were repeated on subsequent days, after she had stooped forward.

When seen by Dr. W., on the 14th November, in consultation with Dr. Wm. H. Page, the appearances were as follows: The right anterior chamber exhibited the crystalline lens in a perfectly transparent state, resembling, at first sight, a drop of oil between the cornea and the iris. Its weight caused the formation of a sort of pouch at the lower part of the iris, so that the lower edge of the lens was a line or more below the cornea, and its upper edge extended to rather above the middle of the pupil. The edge of the lens was sharply defined, as much so as that of a perfect cataract-glass of one inch focus. No attachment could be seen, nor did the aspect of the pupil indicate that any attachments passed through it. There was no injection of the eye. She had a constant sensation of nausea and discomfort, but less circumorbital pain than after the first prolapsus. Vision was almost abolished; but, on trial of cataract glasses, it was at once improved, so that with those of two inches focus, she was able to read.

As the sight of the other eye was very imperfect, this was also examined. It presented a very marked floating motion of the iris, similar to what is frequently observed after operations for the removal of the lens. Displacement of the crystalline was, therefore, presumed to have occurred in this eye also, and a trial with cataract glasses proved the correctness of this supposition, as she could at once see perfectly. The lens was not visible in this eye; but there was an appearance at the lower part of the iris as if the lens were lying in this situation, and were impelled against the iris as the globe moved. Probably a dilatation of the pupil might have allowed the lens to be seen, but it was not thought advisable to expose the patient to a risk of a similar prolapsus to that existing in the other eye. Visiting her a few days after, when the right lens had fallen back into the posterior chamber, Dr. W. found it equally impossible to perceive any portion of the lens in this, as in the left eye. She required a glass of slightly greater power for the right eye; and, by the aid of these auxiliaries, enjoyed perfect vision.

On the 9th of December, the lens, which for several days had been in the posterior chamber, fell through the pupil, as before, and continued in the anterior chamber till the date of her presentation to the Society. If her head is held forward in sewing, there is no displacement; but it is only in some unlucky moment of stooping, while engaged in active employment, that the lens is projected through the pupil.

The case is of interest, not only from its rarity and the singular beauty of the appearances presented, but as suggesting several questions for solution. It is asserted by M. Magne, in the *Gazette des Hôpitaux*, 10th October, 1854, that all the published observations of dislocation of the crystalline, establish the fact that it can only occur after a direct or indirect blow, or as a consequence of convulsions. But our patient asserts that she has not been exposed to any of these causes. A second point to determine, is whether the lens has or has not escaped from its capsule. If no longer protected by this envelop, we should expect it to become opaque or absorbed. But, though the patient has been four weeks under Dr. W.'s observation, not the slightest loss of transparency or size has been undergone. If the capsule is entire, it must have become detached from its normal adhesions, otherwise the pupil could not contract to a size less than half the diameter of the lens during its presence in the anterior chamber. This could not be the case if suspensory attachments extended from the margin of the lens, through the pupil, to the ciliary circle. The fact of the spontaneous occurrence of the displacement in both eyes, would allow of the supposition that some change has taken place in the vitreous body, depriving the lens of the support usually afforded.

Dr. BETHUNE remarked that, in these cases of dislocation of the lens, he does not remember to have seen opacity of the capsule. This is certainly very remarkable. It is natural to suppose that the capsule would be affected, especially if the dislocation be the effect of a blow.

Chronic Dyspepsia; Cure by Cathartic Treatment.—Dr. BOWDITCH reported two cases in which cathartics had proved of great advantage. The first was that of a seamstress, 64 years of age. She had been liable to what she called a diarrhœa, for some months, having been, before, alternately costive and loose. Under this state of things, she had grown weaker, and a week before Dr. B. was called, she had been confined to her room with colic.

On the day of the visit, she had a chill, followed by thirst and fever, dysuria and confinement to the bed. There was an evident soreness, and a hardness in the left hypochondrium. The age of the patient, the long illness connected with the intestines, the tumour in the abdomen, led Dr. B. to suspect some organic disease. It was found, however, on inquiry as to the nature of the dejections, that the *diarrhœa* was, in reality, composed in part, at least, of small, hard, scybala. Dr. B. determined, therefore, to prescribe a gentle but efficient laxative and tonic course. Tinct. rhei and tinct. gentian. comp., each ʒss, twice daily. Eight grs. of blue pill were administered previously to the commencement of this treatment. It is sufficient to say that the cathartic operated gently but effectually. Scybalous masses were daily discharged. Under this, the patient rapidly improved, and after nine visits the tumour had subsided, and with its disappearance the health was soon re-established, so that, at the end of about two or three weeks, she felt better than for months previously.

The result of this case shows the importance of examining *particularly* into the nature of excretions, inasmuch as, although the patient had had a chronic diarrhœa, the scybalous nature of the dejections induced Dr. B. to believe in the retention of small irritating masses in the *lacunæ* which are often noticed in the large intestine.

The second case was similar to the former:—

An Irish servant girl, aged 27, had been in the Massachusetts General Hospital, in 1851, for catamenial difficulties, of which she was relieved. In 1852, she began to complain of dyspepsia, pain in the left hypochondrium, constipation, &c. At her entrance again into the hospital, in June, 1854, the pain in the side was so severe that in walking she had to press upon the part in order to relieve it. From her entrance till the cathartic course was thoroughly tried, in November, she had similar complaints. The usual remedies for dyspepsia were employed, and occasionally cathartics were given, when costiveness was actually reported. She had dysmenorrhœa, dysuria, &c. Once, there seemed so much uterine trouble, that examinations were made, and the os tinæ was found elongated, but nothing especially abnormal was observed, except an irritation of the vagina. The irritability of the bowels was often complained of, and chalk-mixture and laudanum prescribed. Dr. Bowditch saw her four months after her entrance. Remembering the preceding case, and learning that similar hard masses were discharged, *even when the dejections were said to be loose*, he determined upon a cathartic and tonic course, until no scybala should appear: R.—Sulph. ferri gr. ss; aloes gr. ii, three times daily, ordered on Nov. 5, and continued, with the exception of one day, when the patient had a febrile paroxysm, until 21st. During every day but one or two of that period, the patient had from three to six dejections. Some of them, and generally most of them, were of a scybalous character.

Under this course the patient improved rapidly. She lost her pain in the abdomen, the bowels felt easy, the appetite and strength increased, and she was discharged "much relieved," *i. e.* relieved of many of her abdominal troubles, but still obliged to use laxative medicines, or constipation ensued.

Apparatus for Generating Vapour in Croup.—This was shown by Dr. LYMAN BARTLETT, of New Bedford, Associate Member of the Society. It consists of a common saucepan, and of a tube connected therewith, for conveying the steam, when generated, under curtains surrounding the patient's crib, so that he may be immersed in vapour; a crib can be so curtained by duly covering a clothes-horse, disposed around it. Dr. B. spoke in terms of high praise of this treatment for croup; he had been very successful with it. Other means were, of course, conjointly employed. In from four to five years he had lost only three patients by croup. The vapour is often needed for four or five days; mercurials and small doses of morphia are also employed by Dr. B. There had been no *expectoration* of false membrane; Dr. B. believed it to be often absorbed; occasionally, in his cases, it was thrown off by emesis.

Dr. PUTNAM had treated a case of membranous croup, successfully, by vapour, produced by a common nurse-lamp.

Dr. COTTING had used this latter arrangement; he highly values the vapour-treatment in croup.

Dr. BIGELOW, Sen., referred to the method adopted by the late Dr. FISHER, for vapourizing the air-passages of a patient. Dr. F. was in the habit of tying a bunch of mullen-leaves over the nose of a pair of fire-bellows, then, dipping the mullen in boiling water, he caused the action of the bellows to be continually maintained, in the close vicinity of the patient's mouth and nostrils.

Dr. Bigelow said he had seen some recoveries under this treatment, but also a number of failures.

Dr. ABBOT suggested the employment of the flexible caoutchouc tubes for conveying the vapour.

Dr. TOWNSEND, Sen., mentioned the plentiful generation of steam by immersion of hot bricks in a tub partially filled with water; he had found it of great service, and has lately had a case of membranous croup in which this treatment was followed by recovery. Dr. T. added that steam *may* be so copiously produced by this method that the paper will be removed from the walls of the room.

Calculi from the External Face of both Tibiæ.—Dr. HODGES showed these, which he had found in a cluster upon both the tibiæ of an adult negro woman, a dissecting-room subject; they were situated low down, towards the ankle-joint, and varied in size from that of a small mustard-seed to that of a split pea. Some of them were soft, and crumbled on removal; others were hard as bone. They were imbedded in the cellular and adipose tissue, and had no visible connection with the *veins*. The patient died, probably, from tubercular disease.

Analysis by Dr. JOHN BACON, Jr.—Phosphate and carbonate of lime, with some earthy matter. No concentric laminæ on section, such as are always found in phlebitis. Other concretions had the same composition, and therefore it is difficult to class them with any one special kind of calculi.

Tendinous Fibres between the Phalanges of the Hand.—Dr. HODGES exhibited a hand dissected by himself, in which the above fibres were distinctly shown; they are strong and tendinous, found just beneath the skin, and imbedded

in the adipose tissue; they always exist in this situation and (what has probably prevented them from being often dissected), form, as it were, the *skeleton* of the rudimentary *web* existing between the fingers. They extend about half an inch in width; the fibres thickly interlacing with each other and losing themselves on the palmar aspect of each finger half way down the length of the first phalanx; the fibres coming from one side of the finger crossing those from the other at its base, to be distributed on the opposite half of the phalanx. They have no connection whatever with the palmar fascia, and exist about equally well-marked in all hands.

Dr. H. remarked that neither Cruveilhier, Sappey, Sharpey, Quain, nor Wilson, or other authors to whom he has had access, figure or describe these fibres.

Encephaloid Disease about the Scapula; Malposition of the Uterus.—Dr. J. B. S. JACKSON showed the specimens; fragments of the bone, which, to a considerable extent, was quite destroyed; an encephaloid mass of moderate size, from about the bone, and one from the axilla, which last is about half as large as the fist. The cartilage over the glenoid cavity is almost intact; as it is so often observed to be in the case of other bones when affected with cancer. The head of the humerus appears healthy externally; but, on section, shows a cancerous deposit within; as does also the sternum. The lymphatic glands in the upper part of the abdomen were also diseased; with traces of the same in the breasts, and upon the surface of the lungs; there being in these last, the central depression in one or two of the deposits that is so often seen in cancerous disease of the liver.

The patient was a very healthy-looking girl, æt. 25. Swelling was first noticed nearly five months before death, and steadily increased. Pain at first dull, but, for the last three months, quite severe; with much discharge for several weeks before death, and a general decline of the health.

The uterus, in this case, lay horizontally and almost transversely across the pelvis; the os uteri being directed towards the left hip, and the fundus lying directly against the right ovary; the ligament of this last being quite short, whilst that of the left ovary is as much too long. The cervix is not bent upon itself, and the organs are seen to be otherwise perfectly normal.

Upon her entry into the hospital this patient reported the catamenia as regular; and, upon inquiry of the nurse who had charge of her for three months before her death, it is ascertained that the function was in every way perfectly performed. Dr. J. has recently met with a case of very complete retroversion, and in which the fundus lay about opposite to the right edge of the third sacral vertebra; state of the catamenia unknown. He seems to think the importance of these malpositions very much over-estimated.

January 8, 1855. Union of both Leg-bones by Callus after Fracture; Fracture across Base of Skull, with extensive Laceration of the Cerebral and Cerebellar Substance resulting from a Fall.—Dr. CABOT showed the leg-bones entirely united together by callus; the specimen was sawn longitudinally, and the bone shown, continuous throughout. The cause of death being interesting, Dr. C. described the case. T. C., 35 years old, a healthy-looking Irish labourer, was brought into the Massachusetts General Hospital, December 29, 1854, P. M., reported to have fallen into the hold of a vessel. At first, he was quite delirious, throwing himself wildly about, and it was necessary to restrain his motions by strapping him to his bed. He finally became quiet, fell asleep, and died about 5 o'clock A. M. of December 30th, having

lived sixteen hours. There had been free hemorrhage from the right ear, and from the nose; both upper eyelids largely ecchymosed.

Post-mortem Examination.—Upon removing the brain, a fracture was found extending along the right side of the base of the skull, running from before backwards, from about the lambdoidal suture, the whole length of the base, and across, apparently through the ethmoid bone, and appearing on the left pterygoid process of the sphenoid bone. There was also a fracture from before backwards through the centre of each orbital process of the frontal bones. The right lobe of the cerebellum was completely “mashed,” as it were; the right anterior lobe of the cerebrum was extensively lacerated, and the left slightly so; there were several other slight lacerations about the base of the right hemisphere. Right ventricle largely torn, with a considerable clot of blood. Copious effusion of blood, coagulated, was found over the base of the brain.

The prolongation of life, after such very great injury, was probably owing to the free escape of blood from the nose and ear.

Cancerous Disease of the Œsophagus.—Dr. CHARLES E. WARE presented the specimen, and gave the following account of the case: The patient was a woman, 42 years of age. For five or six years she had been subject to an embarrassment about the larynx, which disturbed her, particularly in her sleep, producing a peculiar sonorous sound, different from that which ordinarily attends sleep. During nearly the same period, she had had occasional difficulty in swallowing, which would sometimes force her to rise from table and clear her throat. When Dr. Ware was first called to her, the 3d of March, 1854, she said that, at that time, she had no difficulty in swallowing. She only complained of uneasiness about the larynx, some cough, and her sleep disturbed by the noise she made in respiration. She was always a pale, colourless woman, but she then looked more blanched than was natural to her. In the course of two or three weeks, it was evident that there was some trouble in swallowing, and that it was increasing, although it was not until the middle of April that it was sufficient to prevent her from taking her usual amount and kind of food. Her cough continued to increase in severity, and was particularly urgent on lying down at night. It was accompanied by a copious frothy expectoration and great irritation about the neighbourhood of the larynx. Her catamenia, which had been irregular for about two years, appeared scantily for the last time in April. By the 6th of May, she could swallow nothing dry or solid. Then a staff, with an ivory bulb three-eighths of an inch diameter, was passed with some difficulty and pain. The obstruction was found to be at the upper part of the Œsophagus. In the course of three weeks, passing an instrument every three or four days, the passage was so much dilated that an instrument a third larger could be passed, and deglutition so much improved that she left town on a journey. She was absent four or five weeks, and, on her return, her Œsophagus was more contracted than at any time. She was only able to swallow liquid food, and this annoyed her exceedingly by pouching at the side of the stricture. She had a great deal of pain about the back of the neck and through the ears, but not much pain or tenderness on external pressure about the immediate neighbourhood of the disease. The instrument was passed again, but with more pain than at first, and without benefit. It was continued until the middle of August, when it was abandoned on account of the suffering it occasioned. She continued to lose strength and flesh very slowly, and to be forced to more and more diluted nourishment. During the last few months she suf-

ferred very little from pain or cough. Both seemed to subside as the disease advanced. Towards the close, she suffered most from the accumulation of viscid mucus above the stricture, and which she raised with difficulty. Sometimes the mucus contained blood and pus, and was fetid. But this was not its most common character. She died January 5, 1855. As there is always during life an uncertainty in the diagnosis of these cases, the treatment must be based upon the possibility of their being simple stricture. And this was the course pursued with the present case. Instruments were used as long as they could be borne, and any benefit could be obtained from them. On the return to the instrument in July, it was found necessary to use one with the staff projecting beyond the bulb to act as a director. Otherwise, it was almost impossible to pass the instrument into the stricture. It would lodge by the side of it. It was also attempted to keep the stricture on the stretch for awhile by passing an instrument with a bulb of a dumb-bell shape, allowing the instrument to rest with a swell of the bulb at each end of the stricture. This, on account of the pain it occasioned, was not long persisted in. During the last few months, the treatment was simply alleviative. After all solid food was abandoned, the meat biscuit, made into soup, was found to be the most nutritious and satisfactory diet, and it continued to be acceptable the longest. During the last few days of her life, she was sustained entirely by nutritious injections.

Dr. J. B. S. JACKSON communicated the following account of the appearances observed at the *post-mortem* examination:—

On dissection, there was found a well-marked encephaloid disease of the pharynx and œsophagus, extending from the top of the arytenoid cartilage to the lower extremity of the thyroid gland. The disease was remarkably defined; the posterior and lateral portions being affected, whilst the anterior, with the glottis and the larynx, was perfectly healthy. The diseased structure was from one-fourth to half an inch in thickness, white, soft, and upon the inner surface, uneven; the passage through it being sufficiently free. Just above the upper limits of the disease, and upon the left side, was the opening of a cavity that burrowed down between it and the larynx, and that would have held, perhaps, two drachms; it was offensive and somewhat dark, though not fairly gangrenous, but it contained no proper pus, nor any appearance of food. The other organs were healthy.

“Gaseous Cysts” upon the Mesentery and Intestine.—Dr. H. J. BIGELOW exhibited the intestines and mesentery of a hog, which he had received from a butcher, and which resembled one which Dr. B. had shown to the society a year or two before. An almost precisely similar specimen existed in the Hunterian Museum, and is engraved in the works of Mr. Hunter. Much prominence has been given to this specimen by Mr. Paget in his work on Tumours, being the only one which he refers to under the head of “Gaseous Cysts;” the specimen, moreover, had been examined by Mr. Cavendish and by Dr. Jenner, who sent it to Mr. Hunter. The present specimen shows innumerable air-bubbles beneath the peritoneum, especially at the junction of the mesentery and intestine, and throughout a large portion of its length. They vary in size from a millet-seed to a large green pea, are thickly studded, some of them agglomerated, others solitary, some imbedded in the fat, and others exceedingly delicate, transparent, and hanging off from the peritoneal surface by a slender pedicle of some length.

Dr. B. said that, in the absence of any satisfactory explanation of the mode of production of these air-cells, he would allude to a precisely similar

appearance in the human subject that he had observed and brought before the Society some years ago. The patient was a boy, about ten years of age, and died under the care of Dr. Phelps, of Attleboro', from whom Dr. B. had received the specimen. In that case, the air-vesicles were numerous, and identical with those above described; but the stomach presented an enormous dilatation consequent upon an old contraction at the pylorus. Precisely at the pylorus there also existed an ulcer of considerable extent, which might readily have engaged beneath its mucous surface a part of the air which was forced from the stomach into the duodenum, thus producing a sub-peritoneal emphysema. In the present case, however, there was no ulcer in the stomach or duodenum, which were opened and thoroughly examined; and about which last there were as many vesicles as about the intestine below. In the case of the hog first examined, the stomach was not seen by Dr. B. It is stated by Mr. Hunter that the hog is subject to ulceration of the alimentary canal during the summer months; and it is said by the butchers that the appearance of air-cells, as above described, is not of rare occurrence. In the course of the discussion upon this case, Dr. Jackson alluded to one that he met with some years ago, of chronic ulcer of the duodenum, and which caused death by perforation; besides the peritonitis, there was found an extensive emphysema of the sub-peritoneal cellular tissue, very much as in the foregoing cases.

Evidences of Arrest of Tuberculous Disease in the Lungs; Cicatrization, Cretaceous Masses, &c. &c.—Dr. ELLIS showed the specimen, and gave the history of the case. The patient was a printer, 52 years old, who, since the age of 18, had been subject to attacks of hæmoptysis, a year sometimes elapsing between them, but the interval was usually much shorter. The quantity of blood lost was perhaps only sufficient to tinge the sputa, but frequently amounted to Oss. There was also a "wheezing asthmatic cough," with dyspnoea and palpitation of the heart on exertion. He, notwithstanding, attended to his business until three months before his death, weighing, by estimate, about 125 pounds. After being exempt from the hæmoptysis one or two years, it again returned a month previous to his death, and increased until Oss of blood would be lost at a single hemorrhage; and this would perhaps be repeated during the day. After persisting in this manner several days, it ceased a week before his death; but, apparently exhausted by the repeated losses of blood, he sank, and died on the 19th of December. The above particulars were all that could be obtained, nothing being known about the physical signs.

Autopsy, about twelve hours after death.—*Externally.* Cadaveric rigidity well marked. The chest appeared longer than usual. *Pleuræ.* Surfaces of both pleuræ universally adherent, the uniting tissue over the greater portion being very thin and delicate, but at the apices very thick and strong. The false membrane at the latter points was from one to two lines in thickness, of a dull-white colour, very firm, dense, and contained much fat, which gave it the appearance of adipose tissue. The trachea and bronchi were considerably dilated and thickened, and contained some muco-purulent fluid. *Lungs.* The upper part of each lung, to the depth of an inch and a half from the summit, corresponding to the thick false membrane mentioned, was converted into a mass of dense fibrous tissue, in the midst of which were seen the dilated bronchi, in which the mucous membrane was raised in the form of transverse valves or ridges; a few small cretaceous masses; and a number of cavities, several of which contained caseous or purulent matter, but the greater part of them were quite smooth on the inner surface, particularly the largest, which measured six-eighths of an inch in diameter, and was lined with a red

membrane, very closely resembling the mucous. A bronchus was here seen, which terminated abruptly, its extremity being rounded off, forming a cul de sac. The rest of the pulmonary tissue below the diseased apices was healthy, with the exception that it contained quite a number of isolated tubercles scattered about in its substance, and also a cretaceous mass, two or three lines in diameter, around which the lung, for some distance, had a puckered appearance, as if drawn in by the contraction of a deposit much larger than that which existed at the time of death. Heart rather large, but otherwise normal. Other organs not remarkable. Brain not examined.

January 22, 1855. Screw ejected, after Six Weeks' Retention in the Neighbourhood of a Child's Larynx.—Dr. C. E. WARE reported this case, the history of which he received from the patient himself, now a vigorous man of about 30 years. The patient, when three years old, picked up the screw from the floor, and, putting it in his mouth, by accident swallowed it. The screw was such a one as is used in fastening brass plates on bedsteads.

For six weeks, at intervals of a few days, there would be an access of very severe dyspnoea, with cough of the character of croupy cough, and discharge of frothy mucus, mingled with blood, and, as is stated, purulent matter; after which, for about a week, ease would be obtained. Emaciation and weakness became extreme; it was supposed by the attending physician that the patient was in consumption. The child not talking plainly, and saying it had "swallowed a skewer," much obscurity attended the diagnosis of the case. During one of the fits of dyspnoea and strangling, the patient's mother, much alarmed, held him bending forward, while in the act of coughing, and, as the patient writes, "out flew the screw, covered with rust;" and, immediately after it, "half a pint of blood and matter."

The patient did not fully recover for nearly a year.

Dr. CALVIN ELLIS exhibited the following specimens, and gave accounts of the cases:—

CASE I. Induration of the Brain in a Child.—The child from which the specimen was taken was healthy at the time of its birth, and continued so for two weeks, when contractions of the hands and feet were noticed. At the same time, it became very irritable, and restless at night, and soon began to lose flesh, although continuing to nurse. It finally became unconscious, and remained so, constantly moaning and crying, until death, which took place at the age of nine months and three days, a sister having previously died, at the same age, with the same symptoms. During the last three weeks there was some cough. The above account is necessarily imperfect, as the attending physician was not seen.

Autopsy, twenty-six hours after death.—*Externally.* Great emaciation; no cadaveric rigidity. *Head.* Sutures closed; a small membranous point alone remained of the anterior fontanelle. Dura mater universally adherent to calvaria, but separated without any great difficulty, except at the sutures. Its necessary removal with the bone allowed the escape of 3vj or 3viij of yellow serum, which had collected in the cavity of the arachnoid, filling a space about an inch in depth between the cranium and the surface of the brain. The membranes covering the convexity of the hemispheres normal. No unusual amount of serum under the arachnoid, nor in the lateral ventricles. To the feel, the brain was very much firmer than usual, but the surface was not involved, since the cortical substance could be scraped away, exposing the condensed portion below, which retained the form of the convolutions. The

brain was removed for a more accurate examination than could be made at the house, but it unfortunately softened so much in a short time that the exact limits of the disease could not be ascertained. It may, however, be stated, in general terms, that the white substance was very extensively, though not uniformly, affected, the induration being as well marked in the cerebellum and optic nerves as in the cerebrum. The consistence of all of these parts was somewhat greater than that of the pons varolii in the normal state. Their colour was everywhere the same, viz: bluish white, with perhaps some bloody points, and the cut surface smooth and shining. Spinal cord normal. *Lungs.* Some limited pneumonia in each lung, but particularly in right lower lobe. Liver light-coloured, of large size, and very fatty. Other organs not remarkable.

CASE II. Glandular Proliferous Cyst, with Hypertrophy of the Breast.—The breast was removed by Dr. HAYWARD, Sr., from a woman 37 years of age. The disease had existed a year, and, towards the close, became quite painful. No retraction of the nipple.

On the external surface was a small opening, looking like that of an ordinary abscess. Breast of small size, but imbedded in, and firmly attached to, various parts of the gland, were a number of hard nodules, the largest of them half an inch in diameter, which, on incision, yielded a milky juice, and presented an appearance not unlike that of common scirrhus, with the exception that they were somewhat lobulated. At one point, where fluctuation was detected, a cyst was opened, more than an inch in diameter, with a perfectly smooth lining membrane, attached to which, by broad pedicles, and nearly filling the cavity, were cauliflower-like growths, somewhat lobulated or foliated in their structure, and much less firm than the nodules previously mentioned. Another cyst, filled with a sanious fluid, was opened during the operation.

On microscopical examination, by Dr. H. J. BIGELOW, there were found numerous large free nuclei, with large nucleoli, many large irregular cells, and also a few compound cells. With a low power, a glandular structure was seen, composed of a number of uniform, rounded masses, the interior of which was occupied by the nuclei and cells described.

This hypertrophy of the breast is an exuberance of the glandular structure, with a little deviation from its normal character, and not the hypertrophy of the fibrous tissue of the ultimate lobules which is a distinct variety.

CASE III. Disease of the Liver.—The patient was a gentleman, 53 years of age, who formerly resided for many years in Pernambuco, but during the latter part of his life had lived in the vicinity of Boston. Always well until two years ago, since which time he had been troubled with ascites and anasarca. No pulmonary nor cardiac symptoms. Never had any pain in right hypochondrium, nor yellowness of skin, according to the report of one who had seen him during life, but there was yellowness of the face after death. Urine examined a number of times, but no albumen ever detected.

Autopsy, twenty hours after death.—*Externally.* No cadaveric rigidity. Emaciation well marked. Abdomen flat, but fluctuation very distinct. No œdema of lower extremities noticed, this having disappeared, as was stated, before death. Head not examined. *Pleuræ.* The left contained 3ii of serum; the right, Oj. No adhesions. Lungs congested posteriorly; lower lobe of right considerably compressed; otherwise nothing remarkable. Heart of small size; left ventricle firmly contracted. *Stomach and Intestines.* Small intestine contracted; otherwise nothing remarkable. Spleen of usual size. Surface wrinkled, with a whitish macerated look. Substance firm; of a chocolate-brown colour. Kidneys quite firm; normal. Bladder normal. Peritoneal

cavity contained about a gallon of greenish-yellow serum. Diaphragm very flaccid, as if from over-distension. In this connection, it may be well to state that the size of the abdomen had diminished very much before death. Liver of a dark-brown colour, six inches long, five broad. Surface smooth, with the exception of a portion, about two inches in diameter, immediately above the gall-bladder, where it presented the appearance of an old cicatrix, in the midst of which were seen a number of yellow nodules, the colour of which was owing to the presence of dense yellow fibrous masses, of irregular shape, imbedded in, and firmly attached to, the substance below, the latter being, for some distance, in the same cicatrized state as the surface. Several similar yellow masses, a few lines in diameter, were seen in other parts, through the capsule, but no cicatrization had taken place around them. The substance of the organ was very firm, and on the cut surface had a granular appearance, which was still more strongly marked when the organ was torn. The fibrous tissue did not appear unusually abundant, except around some of the branches of the vena portæ; neither were the edges of the liver sharp, nor the external surface nodulated, as in ordinary cases of cirrhosis. Under the microscope, nothing was noticed in the yellow masses but amorphous matter, small oil-globules, and a few small scales of cholesterine. Bloodvessels, where they entered the liver, normal, as were also the bile-ducts. *Gall-stones.* In the gall-bladder, which contained a small quantity of dark green bile, were quite a number of blackish gall-stones, the smallest two or three lines, the largest an inch and a half in diameter, all rounded, but marked with facets.

Fibrous Polypus of the Uterus, ten inches in circumference, five inches in length, with a Pedicle half an inch in diameter.—Dr. PUTNAM presented the specimen, and reported the case of a patient he had visited, in a neighbouring city, with Dr. CHANNING. She was 41 years of age; had one child æt. 21. Began to flow nine years since, chiefly at the menstrual period; but in the interval also, whenever she made any exertion. Had latterly been much reduced by the loss of blood, and was confined to her chamber. One quite unusual symptom was a painful distension, caused by the accumulation and gradual wedging of the coagula between the tumour and the walls of the vagina. This, at times, became so intolerable that she was compelled to have them removed.

Dr. C. passed a ligature by means of Gooch's canula, and the tumour came off on the fifth day.

Intermittent Fever; Homœopathic Treatment; persistence and aggravation of the Symptoms; Treatment by Quinine and other usual means; immediate arrestation of the Chills; rapid recovery.—Dr. MORLAND read the following account. It is well known that homœopaths adduce the curative action of cinchona in intermittent fever, as "one of the strongest possible proofs" of the truth of their doctrines; in fact, "it was in attempting to ascertain how cinchona cured ague or intermittent fever, that Hahnemann made his alleged discovery." While experimenting upon himself with the bark, "an intermittent fever ensued," and he came to the conclusion that the drug must cure intermittent solely by its power, as he asserted, of producing like symptoms in a well person; and hence his deduction "*Similia similibus curantur.*"

Professor SIMPSON (*Homœopathy: Its Tenets and Tendencies*; Edinburgh, 1853) has well shown the fallacy of the premises upon which Hahnemann founded his system of practice; and he has abundantly proved it not to be true that intermittent symptoms are, by any means, invariably produced, as above asserted; and not only so, but he shows that such an occurrence is a

very rare and quite an exceptional thing. We can readily suppose that, if the very imagination which suggested in Hahnemann's mind the idea which serves as the motto for his system be so utterly fallacious, the attempts to *cure* intermittent fever by infinitesimal doses of quinine, would signally fail; the endeavour itself thus becomes a severe test of the practice; and even homœopaths confess their want of success. (*Vide* SIMPSON, *op. cit.* p. 248, Edin. Ed.) How absurd, in view of such facts, do the confident assertions of the majority of homœopathic practitioners become, such as the dogma that "if any remedy be homœopathically selected, it *will* cure," in whatever dilution it be given?

By their own showing, cinchona and its extracts are "homœopathically selected," if administered in intermittent fever; why should they ever fail in their hands, particularly when the medicine is properly and sufficiently *diluted*, to adopt their, to us, ridiculous phraseology.

In the serious matter of compromising a patient's existence, by a do-nothing course, in a case demanding the most decided remedial interference, it is unfortunate that the public cannot be enlightened as to the responsibility incurred, and the neglect practised, not only by the practitioner who persists in leaving unassisted Nature to struggle with an opponent sure of gaining the mastery, but also by themselves, in allowing their friends thus to be left to their fate.

With a view of illustrating, in some degree, these positions, the following account of a case has been prepared.

Mrs. ———, a young lady of delicate constitution, and for several years a resident in a tropical climate, had, during the latter part of 1853, while at the South, an attack of intermittent fever of the tertian type, and of but slight intensity; it readily yielded to quinine. She came to the North, in January, 1854, and by a fatiguing journey; being far advanced in pregnancy with her third child; fatigue and apprehension caused by accidents during the journey, nearly produced miscarriage soon after her arrival, and she suffered from weakness and cough, until the period of her confinement, February 26, 1854, having completed her full term. The access of labour being somewhat sudden, a midwife was first in attendance; there was retained placenta, and profuse and exhausting flowing subsequently, which, supervening upon her previously weak condition, reduced her to an alarming state; she was perfectly bloodless in appearance; greatly emaciated; her pulse rapid and feeble. She, however, rallied; and, in from three to four weeks, went down stairs to dinner. At this time, a most unfortunate epoch for the advent of new trouble, chills, followed by fever, came on, and a regular *tertian* was declared. She was attended by a homœopathic practitioner, who administered various infinitesimal doses; and, finally, but not until the *tertian* had become *quotidian*, gave quinine in so diluted a form (stating it to be the "first dilution") that, when subsequently asked by the physicians who finally managed the case, how much he gave of the salt, he was unable to say. Under this course the patient continued to grow weaker; the chills recurred, with violence, every twenty-four hours, and generally at an early hour of the morning; after each attack the patient evidently had less and less power of resistance. On the arrival of her husband, who had been absent during this last illness, he immediately dismissed the attending practitioner, remarking that, even to the eyes of persons unskilled in medicine, it was sufficiently evident that Nature unassisted, could not, in *this* case at least, do the work of cure—however possible such a result might be in a robust person.

On Tuesday, April 4, 1854, I was desired to take charge of the patient; but, in view of her then almost hopeless situation, declined to do so unless

with a previous consultation; which being consented to, Dr. BIGELOW, Sen., saw her with me, at about noon of the above day. There had been a severe chill about ten or twelve hours previously. Dr. Bigelow expressed strong doubts as to her recovery, taking into consideration the previous history and her present very weak and alarming condition. It was, however, resolved to give *quinine* in as large quantity as the system seemed likely to bear, combined with nourishment sedulously and judiciously given (the latter point had been, in good measure, attended to previously); the administration of the remedy was immediately commenced—two grains every two hours; at a second visit, same day, P. M. I directed brisk friction with warm laudanum, just before the *expected* chill, with fifteen drops of the same internally; *dry* warmth to be afterwards employed (in place of wrapping the patient in a blanket soaked in warm water, as was done by the former attendants), and the said friction to be kept up as long as there seemed even a tendency to chill.

April 5, 10 o'clock A. M. *There had been no chill*; patient expressed a sense of comfort at her escape from it. Pulse, about 110, feeble; skin natural. *Perge*. The same course of treatment was pursued, with the same, and with constantly better results, as regarded the patient's progress towards recovery. Much of the success attained is doubtless to be ascribed to the untiring exertion of the patient's husband in giving the quinine regularly, securing the prompt administration of food, and making the frictions with his own hands; these latter, either with warm spirit, laudanum, or with the dry hand, were resorted to on the least feeling as if of threatened chill. There was, however, no recurrence of the chills which, before the change of treatment, had been so regular of access. The quinine was continued for two weeks; for the first week in the dose above stated; during the second it was gradually diminished, and finally suspended. Citrate of quinine and iron was given pretty freely during convalescence, which was rapid, when the extreme prostration is considered. The pulse, three or four days after commencing the quinine, sank from over 100 per minute, to 96, 90, 80; it gained strength and regularity daily; the appetite became strong; the digestion was good; colour returned to the previously white lips. In the second week of treatment the patient could walk across the floor, with assistance (ten days before this she could scarcely lift her hand), in a few days she was out, and in a very short time, she whose chance of life was pronounced "*not worth the toss of a dollar*," went from Boston to Washington, D. C., bore the journey well, grew stronger and gained flesh rapidly, has since gone to Europe, and by late accounts is quite well.

I firmly believe that what is said by Dr. BARTLETT (*Treatise on Fevers*, 1847) of the "*congestive form*" of periodical fever, would have proved true of this case of simple intermittent, had the same course been continued under which it became so grave. Dr. B. says: "*The paroxysms must be arrested or the patient will die; the only agent in our possession, by which this can be done, is the bark (cinchona) and its preparations; and no time is to be lost in their use.*" (*Op. cit.* p. 391.) The question will arise, how any well-educated practitioner (in this instance the homœopathist in attendance was such) could, in conscience, allow the disease to progress, when, to his knowledge, he had the means of arresting the paroxysms at hand. One more quotation seems so apposite that it may be admitted: "All that respects the disease, and all that respects the remedy, is so marked, so sudden, and so forcible, that physicians neither doubt nor reason about the matter. They see what happens, and, resting upon the evidence of what they see, they know that the disease is cured by (mercury)" quinine. (Latham *On the Heart*, vol. i. pp. 266-7.) Dr. Latham is speaking of mercury—by substituting *quinine* the sentence is quite in place.

It is often remarked that one isolated case proves nothing; granted—yet an aggregate of such cases will surely prove something; and amidst the boasted “cures” of the homœopathsists, it seems but simple justice that a counter-report should occasionally be made. To most legitimate practitioners, however, such cases, singly, must carry their own evidence.

At the next subsequent meeting (February 12th, 1855) Dr. GOULD related the following case, of which he had been reminded by the above :

A gentleman from the state of New York, on a visit to Boston, had an attack of tertian, and placed himself under homœopathic treatment. The paroxysms grew more severe, and became quotidian. Consultations were held, but no abatement was experienced; and, after two weeks, being very much exhausted, it was concluded, by both patient and doctor, to abandon that method of treatment. On surrendering his patient, the physician remarked, that he presumed that the disease would be soon arrested, as it was well known that quinine would control fever and ague. Being asked, why then did he not employ it, he replied that it was not in accordance with their doctrine, and therefore he preferred not to try it.

On visiting the patient, he was found to be deeply jaundiced, and his liver protruding from under the ribs; bowels constipated. Blue pill was given, and hot fomentations were applied to the hepatic region. The bowels were freely evacuated, discharging large quantities of bile. One paroxysm, only, occurred subsequently, and the recovery was very rapid and complete. No quinine was given.

[The statement of the homœopathist, in this instance, that the use of quinine in intermittent fever is “not in accordance with their doctrine,” only serves to expose his ignorance of his master’s teachings, and indeed of the *basis* of the “doctrine;” as noticed in the remarks prefatory to the first case.—*Secretary.*]

Dropsy, supposed Ovarian; its disappearance after copious Diuresis.—Dr. WM. E. TOWNSEND related the case. The patient had had spinal irritation, and had kept her bed for fifteen years, from this cause; there being actual inability to maintain the erect position. The cysts, said Dr. T., could be distinctly felt; the patient having become very thin. Last summer she had disease of the heart, and also dysentery, which latter became chronic. Death occurred on Saturday night last (20th inst.). The body weighed only forty pounds. The dropsy was in all probability ovarian, having lasted for a period of five years from its commencement to its termination, and completely disappearing after very abundant diuresis. No *post-mortem* examination was allowed.

ART. IV.—*Cases of Fistulæ, communicating with the Gall-bladder.*

By SAMUEL KNEELAND, Jr., M. D., Boston, Mass.

CASES of fistula communicating with the gall-bladder are alluded to in the books and journals, but not with sufficient details to render the symptoms and pathology intelligible. Cases are occasionally found in the books like the following from *Heberden's Commentaries*:—

"CASE I.—A woman, 50 years of age, was for ten days severely afflicted with pain in the stomach, hiccough, purging, and fainting. A month after, a swelling arose near the navel, which was opened, and discharged a great quantity of yellow fluid for the space of four years; at length the pain increased, with sickness and shivering, and after a few days there was discharged a gall-stone, three inches long and as much in circumference, weighing 245 grains. During the following two weeks, a thin liquor was freely discharged; the sore soon after healed up, and the woman recovered."

CASE II.—This is taken from vol. 16 of the *Surgical Records of the Massachusetts General Hospital*, June, 1837:—

H. E., female, æt. 55; married. Lived three years in Lowell; health in youth pretty good; is the mother of eight children, all living; ceased menstruation in 1830, at the age of 48. A short time previous to that, began to feel an uneasiness in right side; a kind of aching and smarting, extending sometimes to the epigastrium; with a throbbing under the cartilages of the right ribs, where a small hard tumour could be felt, as if deep within and a little upward under the ribs. Much oppressed by food, but no vomiting; frequently troubled by cold chills; skin never yellow. Pain and swelling in side worse in winter; bowels often much distended, distension coming on suddenly, obliging her to loosen all her clothes; could not wear clothes tight on account of soreness of side. About three years ago, exerted herself much, when a soreness was felt in side, extending down to right iliac region, where a hard body could be felt. At night, unable to lie on right side until the swelling and pain had subsided. Bowels usually costive, requiring medicine; when no dejection for two or three days, had "cramp" about right short ribs, at times very severe. In October, 1834, had "typhus fever," which kept her sick two months; towards the end of disease, had bloody discharges from bowels. In January, 1835, the swelling much increased, frequently extending over half of abdomen; at one time very great, with severe pain passing to left side; in the course of the following night the pain passed off, leaving a soreness in right iliac region, preventing a free motion of leg. The next month, after another attack of pain, became suddenly yellow; the next day, after nausea all night, a discharge of blood took place from the bowels. During the following summer and autumn, was subject to frequent pains in side, but during the intervals felt herself getting better. About sixteen months before entrance, an opening appeared about a quarter of an inch to the right of the umbilicus, discharging a watery fluid; the tumour gradually got more towards the umbilicus, and was more distinctly felt under the parietes. During the last summer, pus was discharged at times; sometimes a fluid like "clear gall," staining whatever it touched. About three months before entrance, a substance like a gall-stone was discharged. This stone is in the cabinet of the Boston Society for Medical Improvement, and is numbered 566; it was about equal to an inch in diameter, and much worn at one extremity. Since then, the opening has never closed; the discharge is small; twice since the discharge of the stone, a yellow, gall-like fluid has flowed from opening. During the last winter, the bowels have been very costive, requiring medicine often; strength much diminished; appetite good, with no trouble whatever from food since the discharge of blood in February, 1835; sleeps well; complains of pain between shoulders and at epigastrium.

Condition on entrance.—Aspect good; skin not yellow, cool, and soft; respiration natural; no cough; pulse 72; tongue clean. Complains of pain between scapulæ, and of a prickly sensation just to the right of umbilicus; had a similar sensation before the last gall-stone passed. Around umbilicus,

and at right side, are the remains of the cicatrix, with a fistulous opening; a probe passes into the fistula, to the right and a little upwards, about one and a half inches, in a direction nearly parallel with the integuments, striking a body evidently hard; a slight tumor perceived externally in this spot, a little movable; flatness of percussion in the right hypochondriac region, extending down nearly to the ileum; the edge of the liver is felt just above the crest of the ileum, quite perceptibly. Appetite good; not troubled by food; one dejection yesterday; urine well; slight discharge from fistula.

She remained in the hospital sixteen days, during which poultices were applied twice, and the opening dilated with a sponge tent. The discharge became more profuse, rather thin, and there was considerable pain and smarting about the fistula, with some tumefaction; otherwise well. The stone was gradually advancing to opening, when she asked for her discharge.

In this case, the liver was considerably prolonged downwards, perhaps increased by the use of corsets; but, whatever the cause, the same will account for the low situation of the fistula.

CASE III.—This case I first saw in 1847. The patient, a man aged 77, had enjoyed almost uninterrupted good health till within a year, and had led the laborious life of a New England farmer; he had been at times intemperate and irregular in his habits, from which his health, however, had not apparently suffered; at that time he was remarkably vigorous for one of his age. In 1846, he was attacked, without any known cause, with bilious vomiting, accompanied by colic pains of considerable severity; in a few days appeared in the right hypochondriac region what he thought was a boil, which swelled, and opened itself, when a gush of bile and afterwards a serous fluid flowed out, with a gall-stone of the size of a beech-nut; in a week came out another stone of the size of a pigeon's egg; and, in the course of a few weeks, two others of smaller size. The passage of the stones was accompanied with pain and a pricking sensation. The only treatment employed was a sponge-tent to dilate the opening, and poultices occasionally. The discharge varied in its character from a thin, serous fluid to apparently pure bile. He has never had any trouble in the digestive, urinary, or respiratory organs; has never had jaundice, to his recollection, nor clay-coloured stools; the bowels generally have been regular; few men have been more free from indisposition than he has been through life.

In 1847, when I first saw him, there was a fistulous opening, with irregular and somewhat irritated edges, situated about two and a half inches above, and four inches to the right of the umbilicus. The opening would admit the end of the little finger, and with a probe could be felt a hard substance, about an inch and a half from the skin; by a slight dilatation, could be seen a whitish calculus, about half an inch in diameter, and quite smooth; the discharge was slight, and of a serous character. The stone came out in the course of three months. The fistulous opening then healed, with an irregularly depressed cicatrix.

The patient, having sprained his ankle severely, came to Boston to consult an eminent surgeon, about four years after this. The surgeon disbelieved the old man's statement of the case as above given, at which he was much vexed. In 1852, I saw him again, when he told of the surgeon's incredulity, and of his affirming that such a thing was never heard of; at that time the fistula remained closed, but a sense of pain and pricking in the part told him another calculus was on its passage. I lost sight of him after this, but have been told

that the calculus passed in the same way as the others. All the stones that were passed have been presented to the Boston Society for Medical Improvement.

A chemical and microscopical examination of these calculi was made by Dr. B. S. Shaw, with the following result: Calculi seven in number—three measuring a quarter of an inch in diameter, three measuring three-quarters of an inch in diameter, and one measuring five-eighths of an inch in diameter.

The largest was externally of a dirty-white colour, with spots of green and yellow where the outer coat was worn or broken off. The others were green or greenish brown externally, and of the colour of ordinary biliary calculi. They were all covered with smooth convex, concave, or plain faces, having evidently once been articulated with each other.

On section, they presented a central nucleus, or a cavity in which a nucleus of soft and moist nature had dried and shrunk. This nucleus, in the larger calculi, measured one-fourth of an inch in diameter, and the cavities measured the same. It was composed of crystals of cholesterine, and inspissated biliary matter. The substance of the calculi was yellow, with spots of green, and coated with a layer of dark green, to which their colour externally was due. The dirty-white coat on the largest calculus was found to be composed almost wholly of cholesterine.

The substance of the calculi was composed principally of cholesterine, and the different shades of colour were owing to greater or less quantities of biliary colouring matter. The powder resulting from the section of the calculi, and containing a portion of each layer and colour, gave, on analysis:—

Cholesterine	84 parts.
Biliary and colouring matters	10 “
Inorganic matter	6 “
	100 parts.

The specific gravity, also, of these calculi was carefully taken, and found to be 0.812. This is of considerable interest, as the specific gravity is rarely, if ever, noted.

CASE IV.—In the *London Medical Times and Gazette* (for January 7, 1854) is a notice of a case, reported to the Pathological Society of London, of a man who passed, at various times, thirty-one gall-stones from a fistulous opening at the umbilicus; five years after the last one passed, he died of some other disease, when the gall-bladder was found almost obliterated.

Biliary calculi may be situated in the substance of the liver, in the biliary ducts, or, as most commonly, in the gall-bladder. Craveilhier speaks of unilocular and multilocular cysts formed by dilatations of the smaller biliary ducts, and containing calculi. Abscess of the liver may open externally, giving rise to a fistulous opening, very difficult to close; calculi are sometimes passed from such fistulæ, of which Van der Wiel (*Obs. rares de Méd.* tom. i. p. 190, &c.) gives many curious examples. It may be questioned whether the first and second cases did not originate in a hydatid or other cyst, with which the gall-bladder became afterwards adherent, hence allowing the passage of the calculi through its opening. In the first case, the surgeon, and in the second, nature opened the tumour, which was followed by a quantity of yellow, watery

fluid, for four years in the former, and for thirteen months in the latter, before the passage of the calculus, which was immediately preceded by symptoms indicating a change in the character and seat of the lesion.

In the cases above cited, the gall-bladder was the seat of the calculi; the situation of the fistula near the umbilicus, in the first two and the last cases, may be owing either to hypertrophy of the liver, enlargement of the gall-bladder, or to displacement downwards by dress; in the third case, the fistula was in the right hypochondriac region, where the gall-bladder would be situated in a normal liver. The appearance of the fistula, in so many instances, at the umbilicus, far from the normal situation of the gall-bladder, would seem to indicate a connection between this course and the direction of the obliterated umbilical vein and its branches, forming the round cord of the liver—which would serve as a guide to any collection of serous, biliary, or purulent fluid tending to open externally. The secretion from the gall-bladder was considerably changed, becoming at times more like a troubled serous or mucous fluid than bile; in the last case, the coats were thickened and the viscus almost obliterated, as is generally the case when the gall-bladder is irritated by calculi. The ages of the individuals were 50, 55, and 77; and, in the third case, intemperate habits might be chargeable with the disposition to biliary trouble.

The symptoms preceding the opening of the fistula are not very constant; obscure pains and sense of uneasiness in region of liver; bilious vomitings and purgings; a swelling, more or less well defined; pains, resembling colic, caused probably by pressure on, or irritation of, branches of the hepatic plexus, quite different in intensity and duration, and also in seat, from the pains attending the passage of calculi through the biliary ducts; and immediately before the opening of the fistula, shivering, throbbing, and a sense of pricking on the passage of the calculus. There is not of necessity any jaundice, as the biliary ducts may be free, nor any trouble in the digestive, respiratory, cerebral, or urinary organs, and no ascites or indication of biliary obstructions.

The gall-bladder, irritated and inflamed by the contact of the calculi, contracts adhesions to the contiguous parts; ulceration establishes itself, and the tumour opens or is opened externally, or internally into the stomach, duodenum, or transverse colon. No. 565 in the above-mentioned Society's catalogue is a calculus which passed from the gall-bladder directly into the colon, and was discharged per anum; it measured $3\frac{3}{4}$ inches in its greatest, and 3 inches in its least circumference. Another case is alluded to, in which a calculus, nearly two inches in length, passed directly from the gall-bladder into the duodenum. In the first case, which was a female, 75 years of age, "a small, puckered cavity, about the size of an olive, being all that remained of the gall-bladder;" death took place, from scirrhus disease in the region of the gall-bladder, about three months after the passage of the calculus.

The *prognosis* in these cases does not seem unfavourable; in two of the cases the fistula healed up; in one the patient lived five years afterwards, with

the gall-bladder nearly obliterated, and died of some other disease; and in the other there was a lapse of at least seventeen months, without any aggravation of disease, as far as the fistula and calculus were concerned. Indeed, the circulation of the bile and the digestion seem to go on perfectly well, as Cruveilhier remarks, without the aid of the gall-bladder. The invertebrated animals, many fishes, birds, and mammals have no gall-bladder, and it is occasionally absent in the human subject. No. 552 of the same Society's catalogue shows a congenital deficiency of the gall-bladder, in a man 26 years of age, who died in the Massachusetts General Hospital, of pneumonia. No. 553 is a case of atrophy of the gall-bladder, from obliteration of the cystic duct, in a woman 80 years old, whose death did not seem to be caused by bilious trouble, to which she had been subject for three or four years.

The indications for *treatment* are very simple, consisting in the administration of narcotics to calm pain, and poultices and sponge-tents to soften and dilate the parts. When the tumour is much inflamed, and pain severe, surgeons advise puncture; incision was sometimes practised by the French, as if for an abscess, this mode being considered less dangerous than to risk the tumour opening into the abdominal cavity. If the tumour be interfered with at all, it would seem best to open it by means of caustic potash, by which means adhesive inflammation would be set up, and the peritoneal cavity be intact. Considering the difficulty of the diagnosis, and the fact that, as far as is known, the cases do well without interference, it seems the wisest course to allow the tumour to open itself. The resulting fistulæ do not seem to impair the constitution; and when the calculi, which keep them open, come away, they heal of their own accord.

In the *Mémoires de l'Académie Royale de Chirurgie*, tom. i. part 1, 1743, page 255, is the celebrated memoir, by the elder Petit, on "Tumours formed by the Retention of Bile in the Gall-bladder, often mistaken for Abscess of the Liver." Some cases are there given, bearing on the subject of this paper, which are here reproduced, that the symptoms and treatment of this rare affection may be more fully indicated in a single place. The cases are numbered in continuation of those already given.

CASE V.—He was consulted in the case of a lady who had a tumour in the region of the liver, which was regarded as an abscess. The tumour was opened, contrary to his advice. Seven or eight months after, the lady consulted him for a fistula, from which flowed nearly pure bile. The fistula was dilated, and a calculus removed.

CASE VI. shows that the presence of a calculus is not necessary for the production of a fistulous opening. A lady, 66 years old, subject to hepatic colics for many years, with retention of bile, had a tumour in the right hypochondrium; this became inflamed, suppurated, and opened externally, to the great relief of the patient. A fistula was the result, the secretions for a long time being a limpid fluid, and finally bile; the fistula closed and opened several times. The patient died, worn out by chronic disease, impaired nutrition, and fever. The gall-bladder was firmly adherent to the abdominal parietes.

CASE VII.—A man, 45 years of age, somewhat emaciated, with a frequent dry cough and disordered digestion, was attacked with bilious disease; there was at first a moderately hard swelling, without pain, extending from the right hypochondrium to the middle of the epigastrium; urine very abundant, troubled, and red; stools clay-coloured. He underwent the usual treatment of aperients, bitters, plasters, &c., but finally lost his appetite and died. On examination, the gall-bladder was immensely enlarged, and adherent to the peritoneum and abdominal parietes; it was filled with a limpid, but viscid and bitter, liquid; more than sixty calculi were taken from it. This case shows the probable condition of things before the ulceration of the gall-bladder and the escape of the calculi through the fistulous opening.

CASE VIII.—A woman, aged 66, for many years subject to hepatic colics, perceived on the right side of the abdomen a tumour, extending from the edge of the false ribs to the anterior superior spinous process of the ilium. When the tumour appeared, she was free from any biliary symptoms; so that she neglected it until violent colics came on, with fever. Her symptoms were supposed to be independent of the tumour, which appeared to be confined to the integuments; after being bled, she used tonics, with relief; but, for about three years, she was frequently troubled with the same symptoms, being either very constipated or with a bilious diarrhoea, with irregular attacks of fever. The tumour in the mean time grew painful; poultices were applied, suppuration took place, the tumour opened itself in about two months, and a slight discharge, during several months, almost dissipated the swelling. The fistulous opening then began to close and open alternately, with a slight swelling and pain at the edges. A limpid fluid escaped, and finally a sudden gush of bile, amounting to a quart in twelve days; the flow of bile gradually ceased, and the fistula closed up. In this way the fistula alternately opened and closed, furnishing at one time a serous and purulent fluid, and at another pure bile; the pain and fever were always relieved by the discharge of bile. Death took place from phthisis. On passing a sound in the fistula, it penetrated about five inches obliquely upward, towards the gall-bladder; the sound entered a kind of ligament, which attached the gall-bladder to the abdominal parietes, an inch and a half below the cartilages of the false ribs; this ligament was penetrated by a fistulous canal leading from the gall-bladder to a sac secreting pus, situated between the two oblique muscles, and which emptied itself by the external fistula. The gall-bladder had several elongated cul-de-sacs, in which calculi were lodged.

CASE IX.—A woman, 39 years old, after suffering great and almost constant colic pains in the epigastric region (most acute under the ensiform cartilage) for two months, during which she was treated by antiphlogistics and narcotics, perceived a tumour in the region of the gall-bladder. This gradually increased in size, following the linea alba, on the right side, to within an inch of the umbilicus; it opened spontaneously, about six months after its appearance. About four ounces of a purulent matter flowed out, in which floated five or six calculi, of the size of a pea, of spongy texture, light, easily crushed, and inflammable; the suppuration was very abundant for two months. In the course of six months, seven or eight stones like the above were passed; the more free the discharge, the less were the pain and weakness. A probe passed about four inches before reaching the gall-bladder, and caused the discharge of bile accompanied by another calculus. The sinus, being very tortuous, was incised, when bile was seen issuing from a hole in the middle of the rectus muscle, about three inches above the fistulous opening; through this hole the

sound passed into the gall-bladder without obstacle; the bile and other discharges passed freely through this opening for two years, during which the hole in the rectus muscle formed adhesions with the skin, causing a secondary fistula. The patient enjoyed good health, and was quite free from pain and weakness. In case of obstruction by a calculus, it would be very easy to dilate the opening in the rectus by prepared sponge or other means, and calculi could be extracted as from the urinary bladder.

CASE X.—A woman, aged 74, had a colic, followed by universal jaundice; bleedings, deobstruents, and aperients relieved the symptoms, but a tumour appeared in the right hypochondrium, which suppurated, leaving a fistulous opening; this closed from time to time, causing intense suffering. For a year the tumour was considerable, rather nearer the vertebral column than the middle of the hypochondrium; the fistula opened near, and a little below, the umbilicus. On one occasion of its closure, a probe was forced in, causing a discharge, followed by great relief; in the course of the fistula, a calculus was felt; an incision enabled the surgeon to seize and extract a very adherent calculus, four inches long and three inches in circumference, smooth at one end, but having at the other several small cavities, in which were lodged fleshy masses, the cause of the adherence. Another opening was also found, leading to the opposite side, quite into the left hypochondrium, where there was no appearance of tumour, which nevertheless contained a calculus, which was removed also by incision. The patient was completely cured at the end of two months.

In volume ix. of the same *Mémoires*, for 1757, p. 107, are two cases, similar to the above, which will complete the list to be quoted here. Many more might be adduced, but without rendering the subject clearer.

CASE XI.—A lady (age not given) had a tumour of the gall-bladder, the integument being red and inflamed; it was opened as an external abscess, and clear pus was evacuated. Some time after this, bile was discharged through the opening. It was evidently an ordinary abscess, in which the gall-bladder was implicated; the adhesions naturally formed were sufficient to prevent the bile from entering the abdominal cavity. By incision, a calculus was extracted from the gall-bladder, of the size of the largest acorn. A fistula remained for many years. The patient died at an advanced age, enjoying good health to the last.

CASE XII.—A man (age unknown) had a fluctuating tumour above the umbilicus; it was opened, and a very fetid pus discharged; on probing the wound, a few days after, a sinuous canal, two inches long, was found, at the bottom of which a hard body was felt, apparently movable; by a slight dilatation, a calculus of the size of the thumb was removed by forceps; two smaller ones afterwards escaped; a fistulous opening remained, from which there was a continual oozing of biliary matter.

The two latter cases show a difficulty in the diagnosis, or rather the coexistence of abscess of the liver with a tumour of the gall-bladder. The abscesses were properly opened by the surgeon; the escape of the biliary matter and calculi being the consequence of the action of the pus on the gall-bladder.

Remarks.—The principal question resulting from the study of these cases is, how to distinguish these tumours of the gall-bladder from an abscess of the liver. The differential diagnosis is not always easy, and mistakes have been

made by surgeons, instances of which Petit enumerates; the gall-bladder has been opened, instead of a supposed abscess, and patients have died from abscess of the liver, from fear of opening a supposed tumour of the gall-bladder. The difficulty is the greater that the preliminary symptoms in both cases may be the same, or the absence of the ordinary phenomena may mislead the surgeon as to the gravity of the disease; both may commence by a painful swelling in the region of the liver, accompanied by jaundice, clay-coloured stools, high-coloured urine, great thirst, bitterness in the mouth, intense itching, uneasy sleep, fever, vomiting, and other symptoms of retention or obstruction to the passage of the bile. If suppuration takes place, the pain and fever diminish, irregular shiverings come on, and a tumour appears, soft and fluctuating; these would be the usual symptoms of abscess; but an accumulation of bile in the gall-bladder may present the same swelling, fluctuation, shiverings, and diminution of pain and fever after the acute stage has passed. The pain, however, of the abscess is generally pulsative or throbbing, which is not the case with tumours of the gall-bladder; in the former, the constitutional trouble is great; in the latter, trifling. The tumour of the abscess is diffused, and the integument over it oedematous; that of the gall-bladder, distinctly circumscribed, rarely accompanied by oedema of the integuments. The abscess may occupy any part of the hypochondriac and epigastric region; the tumour of the gall-bladder is below the false ribs, and beneath the rectus muscle. The fluctuation in abscess is not apparent till long after the appearance of the swelling; is not always easy to ascertain; is first noticed at the centre, and gradually extends to the circumference, the latter having a hard and swollen edge. The fluctuation in tumour of the gall-bladder appears from the moment of the swelling, and suddenly; is easily recognized, as well at the circumference as at the centre; and is generally free from surrounding hardness.

These differential points, laid down by the elder Petit, 120 years ago, would seem sufficient to distinguish between abscess of the liver (a dangerous disease) and tumour of the gall-bladder (a comparatively trifling affection), the former requiring active interference on the part of the surgeon, while the latter may generally be left with safety to take its natural course.

November, 1854.

ART. V.—*On the Use of Oil of Valerian in the Typhoid Disease.* By J. LEASURE, M. D., of New Castle, Laurence County, Pa.

ABOUT the middle of September last, we were visited by a rather mild epidemic dysentery, which did not spread with any alarming rapidity, nor was it attended by any unusual symptoms, except that there was a much greater discharge of pure blood than we had been accustomed to meet for some years in our epidemic dysenteries.

About the same time we had a few cases of our usual autumnal remittent fever, and also a number of cases of masked intermittent fever or "dumb ague," as it is familiarly called in many portions of the Western States.

Towards the latter end of the month, all these forms of disease seemed almost insensibly to pass into the abdominal typhoid disease, familiarly known in some localities as typhoid fever, and which is frequently confounded with a low form of the regular remittent fever of this and some of the adjoining malarious districts, and which is also called typhoid fever.

Though called by the same name popularly and sometimes professionally, it is hardly necessary to specify the distinctive differences between the two diseases; suffice it to premise that the disease which we propose to treat of in the following remarks, is that form of fever in which the principal lesion is in the mucous follicles of the alimentary canal and generally accompanied by a diarrhoea, always recognizable after having been once seen, and unfortunately only too familiar to many of our professional brethren, in districts where it is an annual but unwelcome visitor.

CASE.—On the night of the 6th of October last, I attended Mrs. W., aged 26, in labour with her third child. Presentation natural, of the vertex, and labour over in an hour and a half. Had not felt quite well for a week previous, having had some dull pain in the forehead and lumbar region, but did not go to bed or take medicine. On visiting her the second morning after her confinement, on looking at her tongue, I found it presenting the same appearance as the tongues of some of my patients labouring under the typhoid disease. Still, she had no fever, nor any unusual tenderness over the abdomen, and I concluded to wait for further signs of mischief before commencing active medication, for what might only be a coincidence in appearance between the tongues of Mrs. W. and my typhoid patients. There were four or five cases of the fever next door, and I left orders that if there should be any diarrhoea, or other untoward symptoms, before my return, I should be immediately sent for.

I was not sent for, but, on my visit next morning, I found her labouring under an attack, having all the characteristics, of a very violent bloody dysentery, and which had commenced some hours previously. The pulse was 120; skin rather cool and moist, with an anxious, haggard expression of countenance indicative of deep abdominal distress. There was no unusual tenderness on pressure over the abdomen except a little soreness in the right iliac fossa; lochia continued natural. This state of affairs gradually changed until the case presented all the characteristics of typhoid fever as manifested in my other patients labouring under the legitimate disease. Still, the lochia continued normal in *quantity*, but very dark and fluid, like dirty water containing the washings of bloody meat. The discharges from the bowels were the dirty-brown dejections nearly resembling old beef-pickle, so common in my other cases, and the mental and physical signs altogether were of the most discouraging character. I supposed, of course, that the case was hopeless, as neither I, nor any of my professional friends whom I consulted, had ever known a recovery from an attack of the real abdominal typhoid disease in a lying-in woman. I attempted to moderate or control the excessive diarrhoea by a free use of opiates and such astringents as answered the purpose in the other cases. I gave oil of turpen-

tine till satisfied that it was of no avail. I gave nitrate of silver in the vain hope that it would accomplish something, but it seemed to have no observable effect even when given in doses of a grain and a half every hour for many consecutive hours. During all this time, up to the ninth day after confinement, there was milk in her breasts sufficient for her child, which I permitted to nurse from the opinion that it *might* have a salutary effect on the mother, and because I entertained no fear for the child, as no cases of the disease had as yet attacked young children. (The child was not injured and lives yet.)

At this period (the ninth day after confinement), milk ceased to form in the breasts, and the patient gradually grew weaker until the 25th, the nineteenth day after labour, when I found her to all appearance moribund.

The diarrhoea had continued at the rate of from five to eight discharges in every twenty-four hours, sometimes involuntary; and, about 2 o'clock on the morning of the 25th, she began to vomit. When I visited her at 8 o'clock in the morning, I found her vomiting a dark-brown substance resembling muddy coffee, which was ejected out of the mouth as out of a spout, and where it ran over her linen and that of the bed, it stained them a dirty-greenish colour mixed with brown.

Her extremities were entirely cold, pulseless, and bedewed with a clammy sweat; her eyes were sunken into the bottom of their sockets, and her lips and nails blue as I have witnessed in the collapsed stage of Asiatic cholera. Indeed, the whole appearance of the patient resembled a case of malignant cholera in the last stage. Believing the case hopeless I did nothing until 2 o'clock, P. M., when I visited her again accompanied by my friend, Dr. J. H. M. Peebles, who coincided with me in a most unfavourable prognosis, but suggested the use of small potions of hot brandy and artificial warmth to the extremities (mustard had been freely applied without any effect). At 9 o'clock at night, I visited her again and found her in *statu quo*.

The hot brandy was thrown up as fast as swallowed, and the extremities were as cold as before, notwithstanding the artificial heat which had been constantly applied by hot bricks and bottles of hot water. I felt loth to go away without at least seeming to *want* to do something, and some passing thought, I scarcely know what, suggested the use of small doses of the oil of valerian frequently repeated. Possibly, I thought it might stop the vomiting; or, peradventure, if retained in the stomach, it might stimulate the nervous power which seemed so greatly deficient in this as well as my other typhoid patients. I ordered one drop of the oil of valerian every hour, and that the artificial heat be continued to the extremities.

26th. Morning. Patient still living; has not vomited since 2 o'clock this morning, and but twice since she took the oil of valerian. Some warmth has returned to the extremities, and there is pulse at the wrist, small, quick, and one hundred and thirty-two strokes to the minute; no diarrhoea; medicine to be continued as before. *Evening.* Extremities quite warm; pulse 120; profuse warm perspiration with a most disagreeable odour; no vomiting since last visit; one voluntary discharge from the bowels, of same appearance as before; nails and lips becoming more natural in colour, and patient says she feels a little better; medicine as before.

27th. Morning. Patient rested quietly; took her drop of oil every hour; vomited none; mind seems clear; had two dejections, the last a little more consistent; still sweats profusely and same disagreeable odour from it, and the sweat stains the linen of a dingy yellow colour; says she feels better, and is *hungry*. To have a little beef-tea; medicine as before. *Evening.* Has not vomited; has had one discharge from the bowels, more consistent

than any former one; has taken half a pint of beef-tea; asks for wine; pulse 100. To take beef-tea and oil as before, and half a tablespoonful of port wine whenever she asks for it.

28th. *Morning.* Rested quietly; slept some between doses; no vomiting; one operation on the bowels almost natural in colour, and no longer watery; pulse 100; still sweats, and same disagreeable odour; warmth natural; feels stronger. To take one drop of oil of valerian every two hours; beef-tea and wine as before. *Evening.* Says she has had a "good day;" no vomiting or nausea; no operation on the bowels; does not sweat so much; pulse 98, soft, and tolerably full. To take the oil every three hours during the night; beef-tea and wine *ad libitum*.

From this time she gradually recovered. The oil was continued at longer intervals for a few days longer; the diet was cautiously made more solid and stimulating, and, on the 2d of November, the milk began to return in her breasts, and in a week from that time she dismissed her nurse and took charge of her child, and may now be said to have perfectly recovered.

During the progress of this case, and before I had used the oil of valerian, I had a patient, a fine young man, who died from the typhoid disease under symptoms of pure prostration, or perhaps more properly, from a want of recuperative vigour of the nervous system, which refused to respond to all the stimulants I addressed to it. This, indeed, seemed to be the principal difficulty to contend with in every instance, and that, too, from a very early period in the sickness.

The marked change which followed the use of the oil of valerian in the case of Mrs. W. led me to try it in other low cases of the same disease, though without being very sanguine of the same success; for, after all, nature, and not the remedy, might have deserved all the credit of her recovery. The other cases were not accompanied by the same extreme state of affairs as to vomiting, prostration, collapse, &c., but the same result followed, viz: the most profuse sweating, with the same odour, and lasting from one to four days, and, in every instance, followed by a rapid amelioration of all the disagreeable and dangerous symptoms.

I now felt encouraged to give the oil a trial in the early stage of the disease, and in doses of one or two drops, varying in time from two to four hours; it early induced a copious diaphoresis, speedily followed by a mitigation of the disease. After I commenced a general use of the oil, I rarely gave any medicine for the diarrhoea, which mostly subsided as soon as the sweating began, and if the bowels were entirely closed for a week or ten days, I rarely interfered, for I found the patients steadily improve with a constipated state of the bowels, and I was only too well satisfied to "let well enough alone." In two or three instances I gave a teaspoonful of castor-oil, with eight or ten drops of oil of turpentine, where I thought an aperient indicated. I have, in all, treated twenty-two cases as above, some of them complicated by pneumonia with profuse bloody expectoration, and in which I did not find it necessary to make any addition to the treatment with the oil of valerian.

Other cases I have treated without the oil, but they were my mildest cases, and did not progress so satisfactorily as the more grave cases treated by the oil alone.

One patient treated with the oil, after seeming to be convalescent for a week, suddenly died, with all the symptoms of perforation of the intestine.

Pari passu with the progress of these cases, I had a couple of cases of very violent delirium tremens, in which I wished to avoid the use of opiates and alcoholic stimulants, and used the oil of valerian in the same small doses frequently repeated, as in the typhoid disease, and, unquestionably, the delirium subsided while the patients took the remedy, and returned with violence as soon as it was suspended a few hours, to subside again as promptly under its resumption.

In my typhoid cases there was little or no delirium where I gave the oil, while it was a very troublesome symptom in *all* the cases in which I did *not* use it, and one which I have always found very annoying in my former experience with the disease in question.

I do not wish it to be supposed that I am an advocate for the indiscriminate use of the oil of valerian in all cases of the typhoid disease, because it has seemed to be useful in a limited number of cases in a particular epidemic, but I do think that it may prove a valuable agent in the treatment of certain low forms of the typhoid and kindred conditions, in which an agent is required to rouse up the prostrated energy of the nervous system, thereby enabling *Nature*, our only real *vis medicatrix*, to rally her vital forces to the expulsion of disease and the reparation of its ravages.

ART. VI.—*On the Climate and Salubrity of Fort Moultrie and Sullivan's Island, Charleston Harbour, S. C., with Incidental Remarks on the Yellow Fever of the City of Charleston.* By JOHN B. PORTER, M. D., Surgeon U. S. A. (Continued from p. 101.)

WE will proceed to consider the epidemic fever of the third era, from 1817 to the present time, and the first in order occurred in

1817. In relation to this epidemic we have ample evidence. Even Dr. Hume says: "All we can now gather is, that the first city case was in Lynch's Lane; how it got there, no one can say." He ought to say how it "got there," if it was imported directly from the West Indies, as seems to be intimated subsequently.

Dr. Shecut is good authority; he was an eye-witness of the disease, and an active practitioner of medicine.

"In this year it commenced in the south-east section of the city, at or near Lynch's Lane, and progressed northwardly and partly north-westwardly, to

the market, and along the lower end of Church Street; of which places it is to be remarked, and particularly of the two former, that they are on made lands, having been formerly creeks, which intersected the city. By the middle of August, its extension and fatality were greatly increased, and in the latter end of the month, it infested most parts of the city, except the north-west.

"All the causes of yellow fever existed—heat and moisture in excess; animal and vegetable putrefaction, in its usual quantity; sinks and drains, as heretofore; and, to crown the whole, there was a great deficiency of electricity."

Quantity of rain :—

	June.	July.	August.	September.	October.	Total.
Inches	8.90	5.20	9.35	5.45	4.75	33.65

"The greatest number of deaths that have been recorded in the city, from yellow fever, from the year 1748 to the present, a period of sixty-nine years, are those of the present year. Total number of deaths this season, 272. Great as this number may appear, if we notice the number of deaths from yellow fever in the years 1699, 1703, &c., it will be found that according to the increase of population the proportion is considerably less now than at any period of the 17th and 18th centuries."

Dr. Rush saw a letter from Isaac Norris to one of his correspondents, dated Charleston, Nov. 18, O. S., 1699, which says that "150 persons had died in Charleston in a few days; that "the survivors fled into the country;" and that "the town was thinned to a very few people"—facts which show that yellow fever was much worse soon after the settlement of the town than in 1817. It is hoped that Dr. Rush's facts are not so "flimsy and imaginative" as Dr. Hume considers his arguments.

No proof, nothing but assertion, has ever been offered that the yellow fever of 1817 was imported. It has been said by the advocates of contagion, Drs. Strobel and Hume, that commerce revived at the close of the last war with Great Britain, and introduced yellow fever; we should say, on the contrary, that commerce revived and brought subjects for the disease. These advocates of contagion seem to speak of 1817 as the first commercial year after the war; but they ought to recollect that peace was concluded early in 1815, and that this and the following year—neither of them epidemic—were more important, commercially, than 1817. We are indebted to Drs. Dawson and De Saussure, *Census of Charleston*, for the following :—

EXPORTS. *Value.*

Fiscal year.	Domestic produce.	Foreign produce.	Total.
1 Oct. 1814, to 30 Sept. 1815	6,574,783	100,346	6,675,129
" 1815, " 1816	10,446,213	403,196	10,849,409
" 1816, " 1817	9,944,343	428,270	10,372,613
" 1817, " 1818	11,184,298	256,664	11,440,962
" 1818, " 1819	8,014,598	236,192	8,250,790
" 1819, " 1820	8,690,539	192,401	8,882,940

IMPORTS. *Value.*

Fiscal year.	Duties collected on imports in Charleston.	EXPORTS AND IMPORTS OF THE U. STATES.	
		Exports.	Imports.
1 Oct. 1814, to 30 Sept. 1815	1,400,887	52,557,753	113,041,274
“ 1815, “ 1816	1,474,474	81,920,452	147,103,000
“ 1816, “ 1817	1,145,678	87,671,569	99,250,000
“ 1817, “ 1818	1,308,104	93,281,133	121,750,000
“ 1818, “ 1819	813,829	70,142,521	87,125,000
“ 1819, “ 1820	613,698	69,691,669	74,450,000

From these tables it is apparent that in the years 1815 and 1816 there was plenty of commerce, and plenty of opportunities for the importation of fever; and there were numerous subjects among the strangers and commercial men; but the epidemic causes did not act with intensity, and there was no malignant fever.

It clearly appears, that of the commercial years 1815, 1816, 1817, 1818, the year 1817 was the least commercial of all. The amount of duties on imports is the criterion of the commercial intercourse, the very intercourse which, according to the contagionists, should import yellow fever; and we find from the tables that less revenue was collected from imports in the port of Charleston, in 1817, than in either of the other great commercial years succeeding the war. The years 1814, 1815, 1816, and 1818, were among the most healthy of all years since the settlement of the town.

In Dr. Strobel's book, the following passage occurs (p. 200):—

“The yellow fever had not prevailed in Charleston from the year 1807 up to this time (1817), a period of ten years. Upon the revival of commerce, however, at the close of the war of 1812, a sudden irruption of yellow fever took place, at a time when the city was filled with strangers.”

This passage is calculated to mislead. In the first place, there was yellow fever in Charleston in both 1809 and 1811, notwithstanding the embargo. Then, one might be led to suppose that the war lasted from 1812 to 1817, whereas the treaty of peace had been ratified by the United States and Great Britain several months more than two years before the epidemic broke out; that this was the first year of commercial intercourse, when it was the third; and that there had been no strangers in the city until this year, when they had been necessarily present for more than two years previous to the breaking out of the epidemic.

1818. Epidemic fever did not prevail, though one of the decidedly commercial years. Dr. Shecut says of this year:—

“In 1818 many of the causes of yellow fever existed; the heat and dryness in excess; animal and vegetable putrefaction in the same proportion; and sinks and drains remained as before; but there was no epidemic yellow fever, and only a very few sporadic and doubtful cases of it.”

1819. This was an epidemic year, though not to the extent of 1817. Dr. Shecut says of this year:—

"The present season, to the 25th of August, has been hot and wet, and remarkable for the excessive quantity of thunder and lightning."

Again: "The prevailing diseases for the season were intermittent, remittent, and catarrhal fevers; and by the 10th of August, a few sporadic cases of yellow fever were detected in the city, the atmosphere of which gave every evidence of a disposition to generate an epidemic yellow fever therein; and the immense number of strangers in Charleston, some of whom, both in point of situation and mode of living were proper subjects of disease, became soon its victims."

"All the causes of yellow fever, with the exception of a deficient electricity, existed this season as heretofore; the heat, though not excessive, was great; and the quantity of rain such as to constitute a wet season."

It is evident that the "plague had begun" at the time Dr. Shecut was writing, but he gives no intimation that it was imported; on the contrary, we are distinctly told that by the 10th of August a few sporadic cases (what the doctor doubtless hoped were sporadic) of yellow fever were detected in the city; and he gives the reasons for apprehending a malignant epidemic—the state of the atmosphere, the heat, the wet season, the immense number of strangers and their imprudent manner of living, and the filth, for he says that every cause "except deficient electricity existed as heretofore." No mention is made of importation from any part of the world, and if the disease had been imported, Dr. Shecut would have told us so. But it was not imported; it was generated in the city, notwithstanding "that Charleston is beyond the norther limits of *self-generating* epidemic yellow fever."

Dr. Shecut believed that the electrical condition of the atmosphere has much agency in producing yellow fever, as in 1817; while in 1818 an *electrical equilibrium* was kept up during the whole summer by excessive thunder and lightning, and the city was healthy; 1819 was "remarkable for the excessive quantity of thunder and lightning," and Dr. Shecut hoped when his work went to press, which was before the end of the epidemic season, that the city would escape from the ravages of malignant fever. But in this he was disappointed; epidemic fever prevailed, and there were 172 deaths. Dr. Simons says that this fever was less general than that of 1817, "although the disease was equally violent in its type."

We see that Dr. Shecut ventured to predict, though not very positively, that the yellow fever would not become epidemic in 1819; and that he was disappointed. In the spring of 1853, Dr. Hume predicted, publicly in the newspapers, from meteorological observations, that yellow fever would prevail in Charleston during the summer, in consequence of which many persons left the city. But the summer of 1853 was notoriously one of the most healthy in a number of years. The condition of the streets was horrible. During the wet season of 1817, not a single street in Charleston was paved, and it was no uncommon thing for loaded drays to be *set* or *stalled* in East Bay street, so deep was the mud. A great era commenced in 1819, when East Bay and Elliott Streets were paved. Let the good work proceed—not a single street, lane, or alley ought to remain unpaved.

1820. The fever prevailed slightly, but did not become epidemic.

1824. The next epidemic fever after 1818 occurred in 1824, when, says Dr. H. Y. Simons,

"It again raged in violence equal to any period in the annals of this country. The first cases were exhibited in a narrow, dirty alley, occupied chiefly by debauched females and sailors; from thence it extended among the shipping, then generally attacking strangers, and latterly children. Individuals who had lived here many years, but had not spent a yellow fever year here, died of it. Several cases were known of persons being seized and dying, who had been here in 1819 and even 1817. Children, some as old as ten years of age, and one as old as fourteen, natives and inhabitants of Charleston, died with this disease; many negroes from the country were seized, but the disease was much milder and few deaths occurred among them.

"Charleston Neck, which adjoins the city, and which is all original land, was quite healthy; strangers residing there generally escaped; and in almost every instance where it did occur, the cause could be traced to exposure in the city."

Dr. Simons concludes:—

"1. The yellow fever is endemic of Charleston, and has its origin in causes there.

"2. All persons who have not spent a yellow fever year there are liable to the disease, and it is questionable if they are wholly exempt until they have had the disease.

"3. Children from the age of one year to twelve are susceptible of this disease, although not in an equal degree with strangers.

"4. Persons living in warm latitudes are liable to this disease, but in a less degree than those of cold latitudes.

"5. Persons born and raised in Charleston, but who have lived for some time in cold latitudes, are liable to the disease.

"6. The disease is most prevalent and general in places most thickly populated, and where the earth has been made, although no place in the city is exempt.

"7. Charleston Neck, where the soil is original, is greatly exempt, although it would be impossible to say wholly so.

"8. From cases which have occurred, it appears very questionable if the disease cannot be twice taken; of this, however, it is impossible to be accurate.

"Cleanliness is all important, and could our streets be all paved, and the marsh and low land be filled up with some other materials than the scavenger's offal now used, we might hope for better things."

Mortality, 236: adult males, 160; adult females, 32; children under 12 years, males, 17; females, 27.

"It must be kept in mind that some died in the suburbs of the city, and very many on Sullivan's Island, which, if added to the list of 1824, would increase it considerably, and, I presume, those of preceding years."

"The spring of 1824 was uncommonly hot, many persons were sun-struck. Cholera infantum was very prevalent, and many children died of it. The atmosphere was close and dense throughout the summer. The bilious fever among the inhabitants when it did occur, was very violent in its type. At the termination of the yellow fever, catarrhal fevers, sore-throat, &c., supervened." *Carolina Jour. Med.* for January, 1825, pp. 1-20.

From Dr. Simons we learn, 1. That the fever of 1824 was not imported, but originated in the city. 2. That it extended from the point of origin to the shipping, and then among strangers generally. 3. That it was malignant, attacking children and negroes. 4. The season was very hot. 5. The sanitary police was not good. 6. Yellow fever is endemic of Charleston, and

has its origin in causes there. 7. The disease is most prevalent and general in places most thickly populated, and where the soil has been made. 8. Charleston Neck, where the soil is original, is generally exempt. 9. 1824 was an epidemic year.

There is not a particle of evidence that this fever was imported, and there is no doubt of its domestic origin. We have, then, three severe epidemics in succession, in 1817, 1819 and 1824, all of local origin, for not a shadow of proof has ever been offered that any of them were imported, against the positive testimony of Drs. Shecut and Simons.

1827. Yellow fever prevailed to such an extent that it may almost be considered epidemic. Dr. Simons says there were 67 deaths.

In August of this year, the disease was imported from Charleston to Fort Johnson, as stated in Dr. Selby's letter. Three soldiers of that post remained a night in the city; on the 9th day after, they were attacked with yellow fever; they were sick in company quarters;" ten or twelve men slept in the same room with them who were alike strangers to the climate; and yet there were no other cases of the disease in the garrison."

Here is an instance of genuine importation, and under the most favourable circumstances, the disease did not spread. The disease, which will be noticed in another place, also prevailed at Fort Moultrie. But how came the yellow fever in Charleston? No one has attempted to prove importation from the West Indies, nor can I find any account of the diseases of this year in Dr. Strobel's book, nor in Dr. Hume's paper.

1828. Dr. Simons states that there were 26 deaths in the city. The dengue prevailed as an epidemic.

1830. There were 29 deaths from yellow fever. Dr. Simons.

1831. This is not put down as a year in which yellow fever occurred in Charleston, but Dr. Selby gives some account of the fever at Castle Pinckney. "One of the Irish labourers died in the city of supposed yellow fever. Several other cases of fever occurred afterwards among the labourers," which Dr. Selby thinks were cases of bilious remittent fever.

1832. The information for this year is derived from Dr. Selby's letter to Dr. Strobel.

"The disease appeared in the city of Charleston, and from two or three of the first cases being traced to the Irish labourers at the Castle, it was supposed to have originated there. A deputation of medical gentlemen from Charleston visited the post, who thought the disease was caused by a 'quantity of shells which had been used for the purpose of filling up the parade ground;' and others thought the disease originated from the 'opening of two privies in the north wall, which had been bricked up for a number of years—their contents in an undecomposed state taken out in the month of August, and exposed to the action of the sun on the bank near by, together with the exposure of the lower story of the quarters to the action of the atmosphere, it being necessary to rip up the floor which had sunk down to the surface of the ground, and had remained in that state for some years.'"

Whether the disease arose from the shells on the parade, or from the

privies and the ripping up of the old floors, or both, it is evident that the disease was of local origin.

1834. Dr. Simons says there were 46 deaths in Charleston from yellow fever. Severe yellow fever prevailed at Castle Pinckney, and there were several deaths, among others Capt. H. W. Griswold, 1st artillery, who died October 23d. We have 46 deaths in Charleston and several at Castle Pinckney, showing an epidemic tendency, or a real epidemic. How came there so much yellow fever in Charleston and at the Castle during this year? We have no account of importation from the West Indies. It has never been pretended that the fever of this year was imported, and the cases and deaths must come under Dr. Hume's clause—"Charleston does occasionally produce sporadic cases of fever of domestic origin, but they do not extend, nor infect localities." Whether they extend or not, or infect localities or not, there was quite a sprinkling of yellow fever, of domestic origin, in and around Charleston. To the patient and his friends it makes but little difference whether such cases are sporadic, or really epidemic, whether of domestic origin, or of West Indian importation. To the patient, if fatal, the result is the same. There can be no doubt of the domestic origin of the fever of this year.

It will be noticed that Castle Pinckney was used as a Lazaretto previous to 1832. It was again recommended to the city council by their committee, December 20, 1853, for this purpose, and was on the point of being turned over by the Government, when some one had the good sense to put a stop to the project. A worse place for sick people could not well be selected. It is surrounded by marshes; in winter it is bleak and disagreeable; in summer, damp, unwholesome and unhealthy.

1835. Dr. Simons says there were 26 deaths.

From 1807 to 1835, a period of 28 years, we find 10 years in which yellow fever prevailed, more or less, sometimes severely, and at other times lightly; but in not one of these years have the contagionists attempted to point out a single instance of importation.

Dr. Strobel says: "In 1807, the embargo was laid, and instantly the yellow fever ceases, and we hear no more of it for 10 years. The embargo and non-intercourse laws had the effect of closing our ports from the year 1807 until 1812, when the war was declared, and lasted until 1815. Upon the declaration of peace, our commerce again revived, and in 1817, we had a return of yellow fever, and from that time to the present, it has occasionally occurred." p. 221.

It is not essential to our present purpose to have it determined whether the embargo and non-intercourse laws applied only to British commerce, or whether there was an embargo on all vessels coming from the Spanish, French and Danish West Indies; and there certainly was none on vessels from New Orleans; but we will take Dr. Strobel's passage as it stands, and inquire how yellow fever came in Charleston in 1809, for there was no foreign commerce, and there could be no importation; nor could there have been any in 1811, when yellow fever prevailed; nor in 1812, which Dr. Hume says was one of the years of epidemic visitation. Intercourse with

New Orleans was uninterrupted during the embargo; and the truth is, that the Port of Charleston has not been shut against commerce and the importation of disease in any year, from 1792 to the present day, except 1812, '13, and '14.

1838. We come to the first epidemic yellow fever to which the contagionists undertake to give an origin by importation, except by general and sweeping assertions. Dr. Strobel knew that there were severe epidemics in 1817, 1819, and 1824, as well as previously; but he makes no attempt to account for their appearance, except in a general way and by strong assertions. Dr. Hume merely mentions them, and passes them by in the same way.

The epidemic of 1838 is thought by many to have been the most severe ever experienced in Charleston, the number of deaths amounting to 353. In relation to this fever, Dr. Dawson and Dr. Saussure say:—

"It is well known that in the spring of 1838 occurred the dreadful fire, which laid nearly a fifth part of the city in ruins, and exposed to the action of the sun and air nearly 150 acres, saturated with the accumulated off-scourings of nearly six hundred families, leaving cesspools, wells, and cellars bare, and ready to give off their fatal emanations as soon as acted upon by the rays of the summer sun. It was not to be wondered at, therefore, that an epidemic arose which exceeded in severity all that had been known heretofore in Charleston."—*Census of Charleston*, p. 202.

Dr. Strobel gives a short account of this fever, which does not differ materially from that of Dr. Hume, and we will give the history, with the remarks of Dr. Simons, from the *Chas. Med. Jour.* for May, 1854, p. 345.

"In relation to the fever of 1838, Dr. Hume says: that the Lord Glenelg arrived at Boyce's wharf on the 4th of July, with yellow fever on board; that the Medora lay at Magwood's wharf, one hundred and twenty yards from the Lord Glenelg; and that the first case of yellow fever was the wife of the steward of the Medora, who frequently visited her husband in that ship; but it is important to add, that her case did not occur until one month after the arrival of the Lord Glenelg."

"In the first place, we deny that the Lord Glenelg had any yellow fever on board, and denied it at the time. And next, we ask, is it reasonable or feasible, or in accordance with experience and observation, that the disease should have been taken by the wife of the steward of the Medora, one month after the arrival of the Lord Glenelg, she not visiting this vessel, the infection remaining dormant in her system, and attacking no one else, not even those who were loading and unloading the said-to-be infected vessel, but reserving all its virulence for this poor woman, whose nearest approach to it was at the distance of one hundred and twenty yards, and that only occasionally."

"But in his anxiety to trace the disease to a foreign source, Dr. Hume forgets other more potent causes; for he says nothing of the extensive conflagration which occurred in Charleston, in the spring of that year, destroying nearly one-third of the city, nor of the effects of the consequent exposure of a large mass of heterogeneous matter to the influence of the sun and rain, nor of the number of strangers who came to the city for the purpose of rebuilding the dwellings which had been consumed by the fire, crowded together in boarding-houses deficient in number, and which strangers, already strongly predisposed to the disease, were the principal victims."

Dr. Joseph Johnson says:—

"On this principle, of a large surface exposed to putrefaction, may be explained the fact, that, after each extensive fire, have succeeded years of yellow

fever, desolating in proportion to the surface laid bare by the ravages of those fires respectively. (Dr. Johnson goes into an examination of the consequences of the great fires of 1796 and 1810, already cited, and then proceeds.) So also with other years, particularly the last and most destructive of fires in April, 1838, by which 25 acres of land, in the centre of the city, was laid open to the lodgement of water and exhalation of deleterious vapours from animal and vegetable substances in a state of putrefaction. The fire deprived eight or ten thousands of the inhabitants of their habitations. Many left the city, never to return. Strangers coming in as labourers, and residents, were attacked with yellow fever, and the desolation was awful. Charleston has not yet recovered from these unfortunate visitations."

"A similar disease followed the destructive fires in Savannah, Augusta, and, we believe, Wilmington, N. C."—*Chas. Med. Jour.* for March, 1849, pp. 57–58.

Dr. Strobel states, that:—

"On reference to the books of the Marine Hospital, we find the first case reported for 1838, to be that of Ryan, who was a seaman on board the *Medora*. He came in on the 4th of August, a decided case, which terminated fatally. He had been sick three days before his admission." p. 124.

Ryan was therefore taken sick on or about the first day of August, the same day on which Mrs. Martin, the steward's wife, was taken, and being the first yellow fever patient admitted into the Marine Hospital this year; it looks, coupled with Dr. Simons' testimony, that there was no yellow fever on board the *Lord Glenelg*, as if the vicinity of the *Medora* was one of the points of origin. We have no idea that the steward's wife took the fever on board the *Medora* from occasionally, or even frequently, visiting her husband; but if she did, what has that to do with the *Lord Glenelg* and importation? To suppose that she contracted the fever by contagion from the *Lord Glenelg*, is, to say the least, to suppose an improbability. We have no doubt that Mrs. Martin was indebted to her own residence for the fever, for Dr. Strobel says: that she "took sick on East Bay, opposite to Hard Alley," a most filthy part of the city. We have no proof whatever, so far, that the fever of 1838 was imported.

1839. Dr. Strobel says:—

"On the 7th of June, 1839, the first cases of yellow fever which occurred in Charleston, were brought to the Marine Hospital. They came from on board the ship *Burmah*, Webster. That vessel left Havana on the 1st of June." Again: "All of a sudden, however, on the 7th day of June, three cases of yellow fever were introduced into the city by the ship *Burmah*, from Havana, and then we learn for the first time, that the disease had been prevalent in that port as well as in Matanzas, as early as May. In ten days after the arrival of the *Burmah*, 3 cases are presented by the ship *Leonore*, and 1 by the ship *Chatham*. These vessels were both from Boston, that port as well as their crews being perfectly healthy at the time of their departure. They are officially inspected, and pronounced perfectly clean and healthy, so much so, that they were released from the quarantine which had been imposed. The *Leonore* lay in the stream the whole time from her arrival, and one patient, Simondson, never landed in Charleston."

It will be noted that Dr. Strobel says the fever was imported into the city from the *Burmah*, on the 7th of June. He must, therefore, refer to Cobb and Shute, who were carried to the Marine Hospital "late in the evening," and who died there, for the *Burmah* (Dr. S. himself says) lay in the stream

until the 22d of June, when she hauled to the Commercial wharves, where she lay until the 4th of July; yet we do not find the fever extending in and around that hospital, but we are always referred to the shipping.

Dr. Strobel goes on to say :—

“On the 17th of June, Ryder, Thorne, and Schmidt were brought in from on board the *Leonore*. Ryder and Thorne came in on the 4th day of their disease, and both died with black vomit. Schmidt, who came in on the 2d day, was discharged cured. The ship *Leonore* arrived in harbour on the 7th day of June, after a passage of 11 days from Boston. She lay in the stream all the time, and Schmidt, one of the patients, never landed in the city of Charleston until brought ashore sick. The *Leonore* lay in the stream 200 yards from the shore, and opposite to Central wharf. The ships *Burmah*, *Medora*, and *Copia*, from Havana and Matanzas, also lay in the stream during the same period.

Dr. Strobel admits that no communication between the *Burmah* and *Leonore* could be traced, but supposes it, without any reason, in our humble opinion. He omits, however, a very important circumstance. He states distinctly, that the *Leonore* lay in the stream 200 yards from the shore, opposite Central wharf, which is nearly up to Market street; and that the *Burmah* also lay in the stream during the same period. He does not inform us whether the *Burmah* lay only half a cable's length from the *Leonore*, or half a mile, a material circumstance; but it so happens, that others tell us that the *Burmah* lay off Roper's wharf, by the South Battery, and consequently, the two vessels were nearly half a mile apart. And now, will any reasonable person affirm that the *Leonore* could be infected by the *Burmah*, at the distance of half a mile in the stream? But let us refer to Dr. Simons.

“On the 7th of June, 1839, three patients were admitted into the Marine Hospital from the ship *Burmah*, which had arrived from Havana, of which I was informed by the physician of that institution. * * The remainder of the seamen on board who were sick, were sent to the Lazaretto, and the ship was thoroughly cleaned and ventilated, she being in ballast. She was in the stream and did not come to the wharf for some weeks after, and had no communication, as far as could be ascertained, with other vessels. On the 17th and 19th, cases were admitted into the Hospital from the ships *Chatham*, *Leonore* and *Elizabeth Bruce*. The *Chatham* and *Elizabeth Bruce* were lying at Fittsimmons' wharf (foot of Market street), the *Leonore* was lying in the stream opposite these vessels, and had never been to the wharf; the *Burmah* was lying in the stream off Roper's wharf; the distance between each of the vessels was therefore considerable, and there were a good many vessels between, on board of which no sickness as yet occurred. The *Chatham* arrived here from Boston on the 5th of June, in ballast; the *Elizabeth Bruce* arrived in Charleston from New York, 7th of June, in ballast; and the *Leonore* sailed from Boston, and arrived on the 7th June, in ballast; all the crews were well. Subsequently, the disease occurred in different vessels in the harbour, which it would be unnecessary to detail.”

“The fever having occurred so early in the season and so soon after its appearance on board the *Burmah*, created a suspicion of contagion in the minds of some, but I could not, upon the minutest investigation, come to that conclusion; and a committee, consisting of Drs. Lopez, Moultrie, Geddings, Campbell, Winthrop and Horlbeck, the President, which was appointed by the Medical Society, after making a minute and thorough investigation, came to the conclusion, that the fever was not introduced by the *Burmah*, or by contagion, but was produced by the peculiar condition of our atmosphere. In other words, it was epidemic, and arose from causes among us. From that report I select the following:”

"By an examination instituted through reference to the captains and mates of the various vessels, whose information was given from the log-books, your committee have ascertained that they had, at the time of their sailing from the different ports, viz: Liverpool, Boston, and New York, for this port, perfectly healthy crews, with sound cargoes, incapable from their character of generating foul air. That there was no malignant disease at the time of their departure; that their crews had not, while in this port, any communication, either direct or indirect, with the *Burmah* or her crew; and finally, that *that ship*, in all these instances, lay in the stream, from a quarter to half a mile from them, except for a short period, of which mention will be made hereafter."

Again: "The presumption is thus fairly induced, that the cleansing and ventilation must have disinfected her (the *Burmah*) sufficiently of her foul atmosphere for purposes of safety, *else, why did none other of her crew remaining on board thence to the time of departure from our port, contract the disease?*"

"Again: 'Thus far your committee have satisfied themselves, that the transmission of the fever through the agency of the *Burmah*, is neither tenable as a fact, nor in accordance with the opinions of a great majority of the medical profession in this country.'

"And again: 'Your committee, therefore, are of opinion, that the yellow fever which has prevailed, and still continues this season, has its origin, not from contagion derivable from those cases imported in the ship on the 6th of June last, but from local and general causes.'

"I may here also remark," says Dr. Simons, "that in 1838, a fire occurred which destroyed one-third of the city of Charleston, and in this year and 1839, the exposure of an immense mass of materials on the surface of so much uncovered earth, and water in the cellars, were exposed to solar influence, sufficient of themselves to generate yellow fever."

"It must be here remarked, that the month of June was uncommonly hot; and the whole of this summer was remarkable for the great drought, as well as high temperature."

The remainder of Dr. Simons' report is devoted to a consideration of the importance of a full supply of pure and wholesome water, the planting of trees, of awnings to the vessels in the docks, the evils of small houses on filthy and partially filled lots, the disposal of the city offal in a more judicious manner than to use it for filling up low streets and marsh lots, the cleansing of the docks at a proper season, the preventing of exhalations from the drains, and clearing them out in the winter season, keeping the streets and yards clean, draining and filling up low lots, properly ventilating and keeping the cellars dry, prohibiting any more cellars, and burying the dead beyond the precincts of the city. Report on the yellow fever of 1839: also *Chas. Med. Jour.* for Novemb., 1851.

Dr. Simons' account renders it certain that the cases of yellow fever on board the *Chatham*, *Elizabeth Bruce* and *Leonore*, were not contracted from the *Burmah*, and the disease was doubtless of local origin, which is not surprising, considering all the circumstances of the city. There were also other cases of this fever in the harbour, with which the *Burmah* could have had nothing to do. Let us look at a portion of Dr. Lebbys' letter.

"Capt. R., of the U. S. Lighter *Valiant*, lying in the cove of Sullivan's Island," was taken with this fever on the 16th June. "This man had not been in the city, from all the information I could obtain, for twelve days, at least, previous to his attack." He had black vomit and died.

This man, then, had not been in the city since the 4th of June, which was before the *Burmah* arrived in the stream, and he was taken sick before she went to any wharf; for Dr. Strobel distinctly informs us, that the *Burmah* was not hauled to the wharves until the 22d June, but "continued to lay in the stream." From circumstances, there is no doubt of this man having taken the disease on board of his own vessel.

The next case admitted was on the 20th June, from the lighter *Sea Flower*, the captain lying at the wharf at Fort Johnson. "This vessel had been engaged for some time in transporting materials from this port to Fort Moultrie—the captain had not been to the city for many weeks." This case must have originated at Fort Johnson, Sullivan's Island, or on board the vessel—certainly not on the *Burmah*.

"The third case was admitted on the 22d June." This person, a northerner, in high health, temperate, took charge of the *Valiant* as soon as R— was taken sick. "This case was well-marked yellow fever from the commencement." He had been frequently to and from the city. On the next day a third case from the *Valiant* was brought in—a black man with bilious congestive fever;" and on that evening another black with the same fever, from the lighter *Fame* from Charleston." Immediately after the admission of the third case, Dr. Lebbly suspected the *Valiant*, and suggested the removal of the crew, and to have her "ventilated and well cleansed."

"Capt. Harvey was placed in command of her, who stated to me that he found a large quantity of decomposed cabbages and potatoes, and other vegetable matter in a putrid state in her lockers, and a quantity of dirt and filth between her ceiling. Several of the negroes engaged in cleaning her were taken sick with intermittent and congestive fevers. She was scuttled and allowed to remain full of water for some days. After this she was pumped out, and continued healthy the rest of the season."

She continued healthy the rest of the season, just as the *Burmah* remained healthy after a thorough cleansing.

We have three cases of yellow fever, two cases of congestive fever in blacks, and several cases of intermittent and congestive fever among the negro labourers about the *Valiant*, not one of whom can be suspected of communicating with the *Burmah*. These three cases of yellow fever are about as strong instances of the "self-generating" process as have ever been placed on record. The lighters did not communicate with the vessels at quarantine, for Dr. Strobel inquired if "they did not communicate with vessels from Havana or Matanzas, lying at the quarantine ground." Dr. Lebbly's reply is what follows:—

"I have taken considerable trouble to ascertain this fact, and I have no hesitation in asserting that, with one exception, there never was any communication with the quarantine vessels and the government lighters, nor with any one in the service of the United States. The exception alluded to, was the captain of the steamer *Sumpter*; on one occasion he sent his boat on board the *John C. Calhoun*, some days after she had been boarded by Dr. Simons. No ill effects resulted from it, for neither of the negroes that were in the boat were sick afterwards. The steamboat continued to perform her regular duties between the

city and the fort throughout the season, and though her engineer and mate were entire strangers, there were no cases of fever on board."

These extracts settle the question. There seems to be no doubt of the local or domestic origin of the fever on board of the Chatham, Elizabeth Bruce, Leonore, and the lighters in Charleston harbour; and it is certain that the disease was not contracted from the Burmah. Every one of Dr. Lebbey's three cases of yellow fever was taken before the Burmah hauled to the wharf on the 22d of June.

Neither Dr. Strobel nor Dr. Hume says one word in relation to the condition of Charleston in 1839. Drs. Dawson and De Saussure say:—

"During the succeeding summer (1839) there still remained sufficient putrescent material exposed in the burnt district, to give rise to a second but less extensive epidemic."

In 1839 yellow fever prevailed in Augusta, Ga., and Dr. F. M. Robertson, now of Charleston, as chairman of a committee "to inquire into the origin and causes which gave rise to the late epidemic," addressed Dr. Geddings in relation to the Charleston fever of the same year. The questions and replies are as follows:—

"1. Do you consider the fever commonly called *yellow fever*, or what is termed *stranger's fever* in your city, a contagious disease?"

Ans. In the whole course of my observation, in several epidemic visitations of yellow, or stranger's fever in this city, I have not witnessed a single fact favourable to the doctrine of contagion. On the contrary, facts, almost without number, have fallen under my observation, all producing the fullest conviction in my mind, that the disease is not contagious."

"2. Do you believe that a state of the atmosphere can be produced, capable of rendering the disease epidemic in a city, by the simple introduction of cases from other cities?"

Ans. In reply to this query, I must likewise state, that I have never witnessed a single fact calculated to favour the belief that the disease can be rendered epidemic in a city, by the introduction of cases from other cities. Such an effect I hold to be impossible in any other way, than by the agency of either common causes or contagion. Believing, therefore, that contagion does not exist, my conviction is that when the disease prevails as an epidemic, it owes this character entirely to common causes, operating through the medium of the atmosphere of the place. A ship arriving in port, having yellow fever on board, may impart that disease to persons exposed to its atmosphere; but the disease produced under such circumstances, will never spread through the community, or in other words, assume the form of an epidemic."

"3. Do you consider the disease as it prevailed in your city, during the past summer, to have exhibited, in any degree, a contagious nature?"

Ans. I have never, either in the epidemic of the past summer or of those which preceded it, observed a single fact or circumstance favourable to the belief in any contagious property. On the contrary, I have witnessed the most free and unlimited intercourse between the sick, and those who might be considered subjects for the disease, without the latter being attacked."

Here we have the whole subject. Dr. Geddings had witnessed several epidemics in Charleston, particularly those of 1838 and 1839; he was a member of the Committee which investigated the cause and origin of yellow fever in the latter year; he knew every circumstance in relation to the Burmah; and here we have his testimony. The whole subject—contagion, contingent

contagion, and non-contagion—is here presented. It is easy to understand that a disease is contagious, and that transmissibility means contagion, but it is difficult to understand the refinement of contingent contagion; and it appears to us that a disease must be either contagious or non-contagious, for we have no instance of contingent contagion in any of the numerous ills that flesh is heir to, particularly in smallpox, measles, scarlatina, &c.; and what the contagionists term so is, in our humble opinion, quite as liable to be extended to the shipping as from it. We have several instances of this. In 1817 yellow fever originated, was engendered, in Lynch's Lane, and extended to the shipping; and in 1824 Dr. Simons tells us that the fever was first "exhibited in a narrow, dirty alley, from thence it extended to the shipping," and then generally. In 1838 we have another instance in the Medora, in the case of Ryan, already alluded to; and in 1839 we have indisputable instances in the Chat-ham, Elizabeth Bruce, and Leonore. In 1849 we have the Numa; in 1852 the Clara Bell, with Capt. Cale's lady; and in 1853 we have an instructive example of this kind of contagion in the Barkelew. The shipping have been more "sinned against than sinning."

ART. VII.—*Synopsis of Thirty Cases of Ovariectomy occurring in the Practice of the Author.* By WASHINGTON L. ATLEE, M. D., of Philadelphia.

CASE I.—The first operation was performed on the 29th of March, 1844; patient, Mrs. G. S., aged 61 years. The incision extended from two inches above the umbilicus to the pubis; the tumour cystiform, bilocular, non-adherent, weighing about twenty-five pounds. The colon was involved in a broad pedicle. Both ovaries diseased. Constitution very feeble. *Death* from insidious peritonitis on the sixth day. (Reported in the *Amer. Journ. of the Med. Sciences*, N. S., vol. viii. p. 43.)

CASE II.—Operation, 28th of August, 1844; patient, Miss L. P., aged 24 years. Incision from umbilicus to pubis; tumour extra-uterine, fibrous, weighing nearly two pounds. Pedicle very thick and fleshy. Violent peritonitis. Intestines during the operation very troublesome. *Recovery.* Died three years afterwards of phthisis pulmonalis, an hereditary disease. (Reported in the *Amer. Journ. of the Med. Sciences*, N. S., vol. ix. p. 309.)

CASE III.—Operation, 15th of March, 1849; patient, Mrs. E. K., aged 29 years. Incision from symphysis pubis to middle of crest of ilium on right side, seventeen inches long; tumour ovarian, fibrous, firmly and extensively adherent to the bones of the pelvis and to the iliac vessels of the right side, weighing eight pounds. Poupart's ligament was embedded in the tumour and stretched across it. Complete procidentia uteri. A mixture of one part of chloroform and two parts of ether, liquid measure, inhaled with the happiest effects. *Recovery.* Still living.

Note.—As a matter of physiological interest, it may be well to state that

this patient has been twice pregnant since the operation, but in consequence of the great dread of parturition, abortion, without my knowledge, was artificially induced. (Reported in the *Amer. Journ. of the Med. Sciences*, N. S., vol. xviii. p. 336.)

CASE IV.—Operation, 22d of May, 1849; patient, Miss M. T., aged 33 years. Incision four inches above umbilicus to pubis; tumour uterine, non-adherent, not removed. Anæsthesia. *Recovery*. Died six months after from erysipelas. (Reported in the *Amer. Journ. of the Med. Sciences*, N. S., vol. xix. p. 318.)

CASE V.—Operation, 16th of June, 1849; patient, Miss H. M., aged 25 years. Incision from two inches above umbilicus to pubis. One large cyst extensively adherent to the omentum by its anterior surface; four gallons of chocolate-coloured fluid removed one week before; weight of tumour, forty pounds. Anæsthesia. *Recovery*. Still living.

Note.—Before the operation this patient had been the mother of an illegitimate child. She has been married since, and has given birth to two large, healthy children, a boy and a girl, passing through the period of both gestations without inconvenience, and having easy and speedy labours. (Reported in the *Amer. Journ. of the Med. Sciences*, N. S., vol. xix. p. 328.)

CASE VI.—Operation, 13th of October, 1849; patient, Miss M. B., aged 43 years. Incision from one inch above the umbilicus to pubis; tumour uterine, with large cystiform bodies incorporated with it; non-adherent, not removed. Anæsthesia. *Recovery*. Died between three and four years afterwards from the progress of her disease. Weight of the mass, removed after death, fifty pounds.

CASE VII.—Operation, 24th of November, 1849; patient, Mrs. T. H., aged 39 years. Incision the same length as in Case VI.; tumour extra-uterine, fibrous, non-adherent, weighing six pounds. Pedicle very thick and fleshy, attached to fundus uteri. Menses present at the time of operation, and continued without interruption. Anæsthesia. *Recovery*.

Note.—This patient had recovered perfectly, was about the house, ate a hearty dinner of duck, felling, &c., which brought on an attack of cholera morbus, resulting in death thirty-nine days after the operation. In her best health before the operation, she for years avoided eating duck, because it invariably caused cholera morbus.

CASE VIII.—Operation, 6th of February, 1850; patient, Miss L. N., aged 30 years. Incision same length; tumour cystiform, very adherent; two round, strong, vascular cords bound the cyst to the recto-vaginal *cul-de-sac*, requiring ligatures. Weight, fourteen pounds. Anæsthesia. *Death* from peritonitis on the sixth day.

Note.—This was not an unfavourable case for the success of the operation; and up to the evening of the fourth day, there was nothing to interfere with recovery. That evening she imprudently ate an orange, including the pulp, and soon after inflammation commenced.

CASE IX.—Operation, 16th of February, 1850; patient, Mrs. F. C., aged 48 years, and greatly prostrated by her disease—indeed, danger of death

seemed impending. The stomach rejected all food, and the bowels were obstinately obstructed, as if by the pressure of the tumour. Incision same length; tumour cystiform, inseparably adherent to the viscera; adhering portions of the cyst were detached from the rest, and permitted to remain; spermatic artery cut and tied; weight, twenty-eight pounds. Anæsthesia. *Death* from exhaustion on the third day.

CASE X.—Operation, 19th of March, 1850; patient, Mrs. S. L., aged 40 years, and so enfeebled by disease that her death was looked for daily. The ovarian dropsy was complicated with ascites; for the latter she had been tapped five times. The lower limbs were anasarcaous and ulcerated, weeping away immense quantities of water. Incision same length; tumour cystiform; the omentum was much thickened, œdematous, spread over the cyst, and almost inseparably adherent; the uterus was also extensively adherent. Weight of tumour, twenty-five pounds, in addition to several gallons of peritoneal fluid. Anæsthesia. *Death* from exhaustion on the third day.

Note.—In both these cases the operation was performed only with the hope of arresting the progress of approaching death.

CASE XI.—Operation, 13th of April, 1850; patient, D. S., aged 41 years, coloured single woman. Incision from near the umbilicus to pubis. Tumour uterine, non-adherent, not removed. Anæsthesia. *Recovery.* Still living.

Note.—In this case the intestines were very troublesome. They were forced out, and could scarcely be returned, in consequence of the anæsthetic agent inducing a cataleptic condition of the muscles, which extended to the muscular parietes of the abdomen. A favourable recovery, and improved health since the operation.

CASE XII.—Operation, 15th June, 1850; patient, Mrs. D. H., aged 37 years. Incision same length; tumour cystiform, extensively adherent; a portion of the cyst, as large as the palm of the hand, could not be separated, and was left *in situ*. First cut the pedicle, then picked out the vessels and tied them separately. Weight of tumour, twenty-five pounds. Anæsthesia. *Recovery.* Still living.

Note.—This patient has been pregnant since the operation, but through over-exertion miscarried.

CASE XIII.—Operation, 25th of July, 1850; patient, Mrs. M. B., aged 42 years. Greatly prostrated and anæmic from flooding and miscarriage two or three months before, and quotidian fever ever since; relieves suffering by the constant use of large doses of morphia. Incision same length; tumour cystiform, strongly adherent to the small intestines, uterus, bladder, rectum, and whole basin of the pelvis; a part of the cyst was left attached to the colon; several cysts broke while removing them and flooded the intestines, some of them filled with purulent matter; pedicle slender, applied no ligature to it, using torsion; pulse, at the time of the operation, 130, and exceedingly feeble. Weight of tumour, fifteen pounds. Anæsthesia. *Recovery.* Still living.

Note.—In this case the operation was performed only with the hope of arresting the progress of approaching death. This lady has since become a widow, and again married, and has enjoyed the most perfect health.

CASE XIV.—Operation, 13th of November, 1850; patient, Mrs. J. S., aged 28 years. Had been tapped sixteen times; fifty-six pounds of fluid removed at one tapping; the mass of the tumour was composed of two immense cysts, and weighed eighty-one pounds. She was two months gone in pregnancy at the time of the operation. Incision from a point midway between sternum and umbilicus to pubis; extensive adhesions. Anæsthesia. *Recovery.*

Note.—This patient recovered from the operation, but this was followed by such great irritability of stomach, in consequence of a state of pregnancy, that she could not be nourished, and she died, thirty days after, of starvation. She had previously been reduced to a state of perfect emaciation. There was no miscarriage. The operation was performed in Connecticut. Her physician, Dr. J. Burwell, writes: "Hers was death by anæmia, complete exhaustion from want of nourishment." *Query.*—Would not the production of abortion have been likely to have preserved this patient's life?

CASE XV.—Operation, 16th of April, 1851; patient, Mrs. M. W., aged 29 years. Incision from near the sternum to pubis. Tumour cystiform, firmly and extensively adherent; some peritoneal effusion; pedicle about six inches broad, filled with varicose veins; one very large cyst was filled with partially-washed crassamentum, and could not be diminished in size by the trocar. Weight of tumour, thirty-five and a half pounds. Anæsthesia. *Death* from peritonitis on the third day.

Note.—This operation was performed in the north-west part of Connecticut, and in the midst of that terrific storm that blew down the Boston Light-house. The room occupied by the patient was the best in the house, and yet the wind and rain had free access to it.

CASE XVI.—Operation, 20th of May, 1851; patient, Mrs. T. G. A., aged 45 years. Incision from two inches above the umbilicus to pubis. Tumour extra-uterine, fibrous, non-adherent, weighing six pounds. Pedicle very thick, short, fleshy, and vascular, and attached to the fundus uteri. The omentum and small intestines forced out, remained so during the operation, and were returned with difficulty. Anæsthesia. *Death* from hemorrhage on the third day.

Note.—The anæsthetic agent produced the same effects upon this patient as stated in Case XI.

CASE XVII.—Operation, 20th of December, 1851; patient, Mrs. A. M., aged 42 years. Incision from one inch above the umbilicus to pubis. Tumour extra-uterine, fibrous, firmly adherent everywhere, not removed. During the operation an abscess, deep in the abdomen, was opened, and discharged a quantity of pus. Anæsthesia. *Recovery.* Still living.

Note.—In this case, although the tumour was not removed, the operation was considered to have warded off impending death.

CASE XVIII.—Operation, 3d of January, 1852; patient, Mrs. M. Q., aged 68 years. Incision about seven inches in length. Tumour cystiform, adherent, and weighed twenty-eight pounds. Pedicle very vascular. Anæsthesia. *Recovery.* Still living.

Note.—In this case, in consequence of the great age of the patient, the intense suffering, and rapid prostration, the operation was performed, at the solicitation of the patient, only with the hope of arresting impending death.

CASE XIX.—Operation, 31st of May, 1852; patient, Miss H. S., aged 20 years, exceedingly feeble; constitution broken up, mouth and throat aphthous, appetite wholly destroyed, rapid emaciation, and very frequent small pulse. Incision from one inch below the umbilicus to pubis. Tumour cystiform, firmly and extensively adherent; cysts quite rotten, and filled with purulent fluid; omentum much thickened. Weight of tumour, twenty-five pounds. *Anæsthesia. Death* from exhaustion in thirteen hours.

Note.—With the hope that there was a remote chance of arresting death, I consented, at the earnest solicitation of the patient, to operate, although she was evidently sinking rapidly. The operation afforded entire relief to all pain, and added much to her comfort.

CASE XX.—Operation, 16th of August, 1852; patient, Mrs. E. A., aged 30 years, greatly prostrated and emaciated; stomach rejects all food and medicine; is kept constantly under the influence of morphia; pulse very feeble. Incision from one inch above the umbilicus to pubis. *Both ovaries removed*; right ovary multilocular and medullary; left ovary unilocular, rotten and gangrenous, containing very offensive putrid gas; everywhere adherent. Weight of tumour, about forty pounds. *Anæsthesia. Death* from exhaustion in nine hours.

Note.—In this patient the disease commenced about the middle of March of the same year. The patient was in a similar condition to Case XIX, and was operated upon with the same hope.

CASE XXI.—Operation, 3d of March, 1853; patient, Mrs. M. E., aged 40 years. Incision from two inches above the umbilicus to pubis. Three fibrous tumours, extra-uterine, adherent, weighing four pounds. Two of the tumours had very thick, short pedicles; the other was embedded in the substance of the uterus, and was enucleated. The small intestines were forced out, and with difficulty were replaced. *Anæsthesia. Death* from peritonitis on the third day.

CASE XXII.—Operation, 14th of September, 1853; patient, Mrs. S. R., aged 56 years, enormously enlarged, and a great sufferer. Incision from umbilicus to pubis. Tumour cystiform, extensively adherent; considerable peritoneal effusion; pedicle broad and vascular. Weight of tumour, fifty pounds. *Anæsthesia. Recovery. Still living.*

Note.—The rapid increase in size, the great oppression, weight, and suffering, and the age, in this case, led us to apprehend a rapidly fatal termination, unless arrested by an operation.

CASE XXIII.—Operation, 21st of September, 1853; patient, Mrs. E. S., aged 26 years, very much prostrated, and suffering great agony. Incision from umbilicus to pubis. Tumour cystiform, strongly and extensively adherent; several cysts gangrenous, rotten, and filled with pus. Weight of tumour, over forty pounds. *Anæsthesia. Death*, in twenty-two days, from gangrenous perforation of the jejunum. The opening was about one inch in diameter.

Note.—The disease, in this case, commenced about six months before, and progressed with great rapidity. The vital powers were sinking fast, and the operation was undertaken with the hope of warding off impending death. At the time, gangrenous inflammation was found occupying the right side of the abdomen, yet notwithstanding for two weeks after the operation, there were strong evidences of recovery. Indeed, the wound had healed, the ligatures had come away, and the patient was sitting up. The appearances of the perforated bowel indicated that its vitality had been destroyed at the time of the operation, and that nature had attempted to repair the mischief. It was considered by all present that life had been prolonged by the operation.

CASE XXIV.—Operation, 17th of April, 1854; patient, Mrs. J. C., aged 36 years. Incision five or six inches long. *Both ovaries removed; tumours cystiform, non-adherent, weighing fifteen pounds. Some peritoneal effusion. Both pedicles broad and vascular. Anæsthesia. Recovery. Still living.*

CASE XXV.—Operation, 13th of July, 1854; patient, Miss S. M., aged 31 years. Incision from umbilicus to pubis. *Both ovaries removed; tumours cystiform, very strong, and extensive adhesions, extending to the whole interior of the pelvis. Weight of the tumour, fifty pounds. Anæsthesia. Death on the fifth day, of exhaustion and disease of the stomach and ileum.*

Note.—The *post-mortem* examination revealed extensive disease in this patient. The whole cavity of the abdomen was sprinkled over with tubercular deposits. A large mass of it was attached to the fundus of the bladder. Another mass penetrated the coats of the ileum about thirty-six inches above the valve of Bauhin, and from this point down, the bowel was greatly contracted; above this point the bowels were distended with a yellowish liquid. The stomach and duodenum contained over half a gallon of black liquid, resembling the black vomit of yellow fever. The mucous membrane of the stomach, to half its extent, was discoloured by this black matter, which seemed to penetrate the tissue. It was softened and easily abraded, and the other coats of the stomach were likewise softened wherever this inky discoloration existed.

CASE XXVI.—Operation, 5th of September, 1854; patient, Mrs. W., aged 52 years. Incision four or five inches long. *Tumour cystiform, slightly adherent, weighing twenty-four pounds. Anæsthesia. Recovery. Still living.*

CASE XXVII.—Operation, 30th of September, 1854; patient, Mrs. J. P., aged 59 years. Incision about six inches long. *Both ovaries, and an independent pelvic tumour removed. Right ovary fibrous, of bone-like hardness, and short, thick, fibrous pedicle. Left ovary cystiform and fibrous, having a broad, vascular, flesh-like pedicle. Pelvic tumour fibrous, as hard as bone. Very firm and extensive adhesions. Weight of tumour, twenty pounds. Anæsthesia. Death on the fifth day, from secondary hemorrhage.*

CASE XXVIII.—Operation, 19th of October, 1854; patient, Mrs. S. M., aged 24 years. Incision from above the umbilicus to pubis. *Tumour cystiform, adherent, weighing thirty pounds. Anæsthesia. Recovery. Still living.*

CASE XXIX.—Operation, 31st of October, 1854; patient, Mrs. A. E. L., aged 42 years. Incision from six to eight inches in length. *Tumour cysti-*

form; firm and extensive adhesions. Weight of tumour, thirty-eight pounds. Anæsthesia. *Death* on the sixth day, from secondary hemorrhage.

CASE XXX.—Operation, 16th of December, 1854; patient, Miss D. P., aged 49 years. Incision about five inches long. Tumour cystiform, non-adherent, weighing eighteen pounds. Small intestines forced out and troublesome. Anæsthesia. *Recovery.* Still living.

Note.—The anæsthetic agent produced the same effects upon the patient as stated in Case XI.

ART. VIII.—*A Case of Removal of the entire body of the Uterus by the large Abdominal Section.* By E. R. PEASLEE, A. M., M. D., Prof. of Surgery in the Med. School of Maine, &c. (With a wood-cut.)

THE surgeon who assumes the responsibility of a novel or rare, and, at the same time, dangerous operation, but has not the courage to report the result, unless it be successful, fails to improve an opportunity for contributing to the advancement of his profession. Every such operation is, scientifically, an *experiment*; and to give the results, in any department of science, of those experiments only, out of the whole series of the same kind, which resulted as the experimenter previously hoped they might, would be always wrong, often injurious, and sometimes productive of even fatal consequences.

It is only by knowing *all* the results in the same series of experiments that we can arrive at correct conclusions. It is, therefore, the duty of surgeons to record all their important operations in a form for future reference, if required; and especially to publish the results of all such as are new or very seldom performed. Thus alone can the true value of any operation not yet generally recognized, be decided; and so long as it can be said of any operation that but few of all the unfavourable cases have been reported, a prejudice against it (though perhaps unjust in degree) will very justly be entertained.

To secure the object in view, then, unfavourable cases are indispensable. They are also just as valuable as the favourable ones; provided they are reported with the same care and discrimination—which is, however, very rarely the fact. And we will hope that the time will come when the desire to advance the science and art of surgery may become paramount to that of blazoning forth the operator's boldness or skill, and may more frequently become the real motive for reporting operations in the public journals.

The following is a case of removal of the whole of the body of the womb, by the large abdominal section; the patient having died on the fifth day after the operation, from gangrene produced by the strangulation of two hernial protrusions.

Mrs. Dean, of B——, Vt., a widow, aged 35, the mother of four children, of good conformation, and bilious temperament, first applied to Dr. J. F. Skinner, of B——, in April, 1853; and I obtain from Dr. S.'s first letter to me respecting her, the following history of her case from the commencement till I saw her in August following.

"I found her," says Dr. S. (in April), "with a tumour in the right iliac region, which I judged to be ovarian. She was also suffering severely from tenderness of the whole spinal column, and much pain in the head; the bowels were excessively costive; there was leucorrhœa, with difficulty in micturition, and numbness in the right limb. I learned that her last labour (17 months ago) had been a tedious one, and was followed by much inflammation of the pelvic organs; and that she was several weeks in recovering so far as to be able to walk. The *tumour* was first noticed soon after her confinement, and since my observation, has rapidly increased."

All the troublesome symptoms were relieved by Dr. Skinner, except the difficulty in micturition; "the tumour being so movable that on assuming the erect position, it falls partly into the pelvic cavity, often stopping the flow of urine, and producing such violent spasmodic pains, that she has been obliged to have the bladder evacuated by the catheter exclusively for several weeks past. The same suffering also attends every evacuation of the bowels; and, of late, even the erect position brings it on. When the bladder is distended, the tumour will rise above the umbilicus; and on evacuating the former, it will fall as low as the pubes; and I judge from its mobility that it cannot be extensively adherent, and that an operation for its removal might be advisable. Indeed, I can see no other chance of continuing life long, or even making the patient comfortable while life lasts. Dr. Newell, of Lyndon, has also examined the case, and accords with me in opinion. I would add, she has menstruated several times since her confinement, but not very regularly as to time, and with considerable pain."

I replied that ovarian tumours do not generally produce such suffering from a change of position, and that I had never seen a case attended by all the symptoms he had mentioned; that though I had successfully performed the operation of ovariectomy,¹ I considered it very rarely justifiable, though sometimes *clearly* so, and should never perform it from any other motive than a sense of duty.

Dr. Skinner replied that "he had doubts in his own mind as to an operation, and wished for advice;" and it was arranged that I should visit his patient on the 2d of August: Dr. J. P. Bancroft, of St. Johnsbury, Vt., my friend and former pupil, accompanied me, and assisted in the following examination:—

Examination of Patient, August 2d, 1853.—She is confined to her bed; is pale, and rather emaciated; tongue coated; has little appetite; can sit up but little, so much distress is occasioned by pressure from the tumour when the trunk is erect.

The tumour now lies on the middle line, is about four inches in its bilateral dimensions, and extends upwards to within one inch of the umbilicus. It is very firm, is round and smooth, except at a point two inches below the upper extremity, and to the right, where a small hemispherical protrusion apparently about one inch in diameter can be distinctly felt through the abdominal walls. The tumour is perfectly movable in all directions, but naturally inclines more to the right; and is not sensitive under pressure. The bladder

¹ See a Report of this case in this Journal—April, 1851.

is now distended with urine; but after its evacuation by the catheter, the tumour descends so as hardly to appear above the pelvis; and if the patient now rises, much distress is produced by its downward pressure.

After a thorough evacuation of the loaded rectum by an enema, I proceeded to make an internal examination.

1. *Per vaginam* the finger discovered the os uteri at the normal distance, and in its usual relations (it had been somewhat elevated when the tumour was pushed up by the distended bladder). It was patulous, and had a velvety feeling. The tumour could be distinctly felt in the region of the right ovary, it inclining to that side as it entered the pelvis; and could not be felt at all on the left side. It appeared solid as before, and was not tender on pressure.

2. An examination *per rectum* also enabled the finger to reach and to elevate the tumour on the right side of the uterus, and the body of the last seemed carried somewhat to the left. The same solid feeling before mentioned was also here detected. The tumour was very movable, as before.

3. On introducing a *speculum vaginæ*, the os uteri was seen to be slightly ulcerated, and to be in other respects normal. The *uterine sound* passed through the cervix with much difficulty on account of the very small diameter of the passage; but on arriving in the uterine cavity passed at once to the distance of three and a half inches from the os, and then appeared to reach the fundus, as it could be passed no further. The uterus is very movable, and can be carried in any direction by the sound, except to the left, while the tumour is held still in its place in the pelvis; or, the uterus being fixed by the sound, the tumour may be moved freely to and fro, without moving the womb; unless when carried against the latter to the left or in front.

4. A sound being introduced into the *bladder*, the tumour is also movable in all directions, without imparting its motion at all to that organ.

Diagnosis.—In view of the history of the case, and the facts elicited by the examination, I judged this was a case of tumour commencing in the right ovary, and not yet adherent to any other part or organ, except to the uterus, by its natural but enlarged attachments.

In this opinion I was confirmed by those of Drs. Skinner and Bancroft, who assisted me, and with whom every fact and its bearing was freely discussed during the protracted examination made. Other affections, for which ovarian tumours may be mistaken—especially fibrous and other tumours of the uterus—were constantly kept in mind, and the decision above mentioned was arrived at as the only one consistent with all the facts in the present state of our knowledge of intra-pelvic tumours.

The patient insisted on the removal of the tumour, and I was obliged to admit that this appeared a case as favourable for ovariectomy as is often seen, so far as the size and mobility of the tumour were concerned. But I should not, at any rate, operate till she was in a better general condition; and indeed would not, without further reflection, encourage the idea of operating at all, for the reasons I had previously stated in my letter to Dr. Skinner.

The patient had already been made aware of the risks of the operation by her attending physician; and to the remark that it might itself prove fatal, replied that she was already the same as dead to her two surviving children as far as assisting them was concerned; that she lived for them alone, and therefore wished to seize on any chance for a return of health and strength, however slight it might be, and however great the accompanying risk.

In view of all the circumstances, I finally consented to undertake the operation at some future time, provided she were first in a state of improved

general health so far as might be desirable, and that all the other circumstances became favourable; especially, also, that she should be removed to some not too distant point on a railroad, that I might visit her daily for any required time after the operation.

Everything subsequently becoming favourable, and every arrangement having been completed, the operation was arranged for September 21, precisely three years after the successful operation of ovariectomy before alluded to; it being understood with the patient, however, that if she then had a cough, or was in any way unwell, I should defer it till another time.

Plan of the Operation.—I proposed to make an incision four inches long through the skin and superficial fascia only, and then an opening in this, one to two inches long, into the peritoneal cavity. Then, if I discovered any error in the diagnosis, or that the tumour, for any cause, could not be removed, I should have but a small wound to close. On the other hand, if the tumour were a single sac of fluid, I would evacuate it by a trocar, and draw the sac through this small opening, and tie the pedicle; while if solid, I would enlarge the opening to the smallest required extent only, and thus remove it.

Sept. 19. On making another examination of the patient to-day at St. Johnsbury, Vt., where the operation is to be performed, in order to detect, if possible, any fallacy in the former conclusion as to the precise nature of the case, all things were found as at the first examination in August; except that the patient now appeared in good general health, though as much distressed as ever after micturition by the downward pressure of the tumour, and therefore still confined constantly to her bed. The tumour had, however, evidently increased somewhat in size, and now gave an evident, though not very distinct feeling of fluctuation. The latter fact only served to confirm me in my previous diagnosis.

She was to take a dose of castor-oil on the evening of the 19th, and only milk porridge for diet till the operation (on the 21st.)

At the time appointed the patient was placed upon a table before a window, the pubes having been shaven, and the pudenda having been covered by a cloth folded like a diaper. The precise position of the linea alba was indicated by a line on the skin made by the tincture of iodine, with some lines across it to aid in a more accurate introduction of the needles through the abdominal walls; and the patient commenced the inhalation of the pure sulphuric ether through a tube made by rolling up a common towel and confining it with pins, at half-past three o'clock in the afternoon.

Drs. Jewett and Bancroft, of St. Johnsbury, Drs. Skinner, Pierce, and Masta, of Barton, Drs. Newell and Cahoon of Lyndon, Dr. Carpenter, of Sutton, and Drs. J. V. Lansing, and W. H. Burleigh, my private pupils, assisted in the operation. The temperature of the room during it was kept at 80° (Fah.), and the atmosphere rendered humid by the evaporation of water. I had also prepared an *artificial serum*¹ with which to moisten my hands when it became necessary to touch the peritoneum, or to apply to the latter while exposed to the air.

Before commencing the operation, I remarked to the medical gentlemen present I did not think it possible to arrive at any other rational conclusion than that this was a case of disease of the right ovary; but since the best diagnosticians had erred in cases of pelvic tumours, I should commence the operation fully prepared to modify my previous plan, or even at once to close

¹ A mixture of albumen $\mathfrak{z}\text{v}$ and common salt $\mathfrak{z}\text{ij}$, with water Oiv .

up the wound if a different state of things from what I expected should be found to require it.

I commenced with an incision four inches long through the skin and superficial fascia extending from a point one inch below the umbilicus to another two inches above the symphysis pubis; these layers being pinched up, and together divided from within outwards by a long and slender knife. On doing this, the sheath of one of the recti muscles was opened for the extent of one quarter of an inch; and finding it to be the *right* rectus by the introduction of a probe into the sheath, I thus ascertained the precise position of the middle line. After waiting several minutes for the oozing of blood to cease, I divided the abdominal aponeurosis to the extent of one inch and a half, and found a layer of fat one-quarter of an inch thick presenting itself, and apparently *not* attached to the inner surface of the aponeurosis. Was this, therefore, the subperitoneal layer of areolar tissue unusually laden with fat, or had I already divided the peritoneum together with the aponeurosis?—in which case this layer of fat must be the great omentum. A probe could be freely passed in all directions between the aponeurosis and the fat. Still, on evert- ing the aponeurosis, there was found to be no layer of peritoneum on its internal surface, and therefore the peritoneal cavity had not yet been opened. I therefore divided the layer of fat, and the peritoneum under it, to the extent of one inch and a half, and the tumour was brought into view. Some delay was necessitated by the layer of fat, it being different in appearance from anything I had met with before; but of course, the peritoneal *cavity* had not, till this instant, been exposed.

The tumour was smooth, except where the protrusion was situated which had been felt through the abdominal parietes, and which was seen to be a hemispherical sac filled with fluid, and about three quarters of an inch in diameter. It was of a pale livid colour, and perfectly free and unattached above, as had been predicted. To the touch, a feeling of fluctuation was very distinct; and all the medical gentlemen present accorded with my own opinion, that it was a very dense sac containing fluid.

I accordingly thrust a trocar into the tumour to withdraw the fluid, but *not a drop appeared*, except a few drops of blood as the canula was partially withdrawn. Thus, finding the tumour to be solid, I next enlarged the opening through the abdominal aponeurosis and peritoneum, in order to examine its attachments below. Perhaps a drachm of blood escaped into the cavity of the peritoneum while effecting this, and two minute arteries were tied to prevent hemorrhage in this direction.

The tumour was now drawn out through the incision, when, to my disappointment a second time, I found that it was continuous with the *fundus of the uterus*. Indeed, at first sight, it appeared to be merely *the uterus* very much enlarged; but, on examination, it was found to be a development *from*, and not *of*, this organ.

What was next to be done? To return the mass and close up the incision was the expedient which alone would seem to remain; but the puncture made by the trocar was now bleeding freely, and the idea of returning the tumour in that condition was not to be entertained; while, to arrest the hemorrhage for the time, by sutures, or other mechanical means, would have been merely postponing the risk without removing it. The body of the uterus was elongated and slender at its lowest part in the pelvis, from the traction upwards, probably of the tumour by the distended bladder; and it was unanimously decided, by all the medical gentlemen present, that the risk to the

patient would be no greater of removing the whole mass, including the body of the uterus, at its lowest part, than of returning the former in the circumstances just mentioned, and I decided to remove it. The patient just now began to retch quite violently, and the intestines to protrude. I accordingly at once extended the incision to the symphysis (to six inches), and passed a stout double ligature, of four threads of saddler's silk, waxed, but not twisted, through the uterus as low in the pelvis as possible, and amputated the organ above the ligatures. The left ovary being diseased, was removed with the uterus; the right was left *in situ*, ligatures having been applied so as to prevent any hemorrhage when the necessary incisions were made through the broad ligaments. The intestines were returned by the hands, after being moistened in the artificial serum, which was kept blood-warm and constantly used after the cavity of the peritoneum had been opened. An artery in the remaining portion, the *stump* of the uterus, had not been sufficiently constricted by the ligatures at first applied, and bled quite freely till a separate ligature was applied to it.

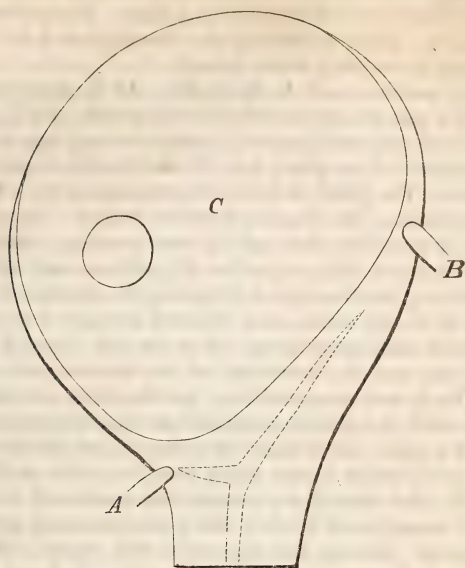
The clots were removed from the pelvis, the hemorrhage not exceeding six ounces in all; and the wound, six inches long through the superficial parts, and five inches through the peritoneum, was closed by six needles placed three-quarters of an inch apart, and seven sutures; five being between the needles, and the remaining two being at the top and at the bottom of the incision. The ligatures came out at the nearest point, between the lowest needle and the lowest suture; and a short gum-elastic tube was left introduced by the side of the ligatures. Adhesive strips were applied over the needles and sutures; a compress, saturated with warm water, and covered with oil-silk, was laid over these, and the patient transferred to her bed.

Examination of the Tumour.—The weight of the whole mass removed is nineteen ounces avoirdupois; it being five and a half inches long, four inches wide, and three inches thick. With the exception of the hemispherical sac, before alluded to, three-fourths of an inch in diameter, it is externally of uniform appearance, and still gives an indistinct feeling of fluctuation. In size and shape it much resembles the uterus enlarged by impregnation. The lower portion being slender, all the part above can be easily *flexed upon that in every possible direction*. The two halves of the mass are, however, not symmetrical, like the impregnated uterus; the right side being the most prominent, and the sense of touch easily distinguishes a large tumour forming in the whole upper part and the right half of the mass. A sound, also, introduced into the portion of the uterine cavity left in the mass, shows that it extends to the left side of the mass and to the distance of three inches; there being a sudden turn to the left at the distance of one inch. The Fallopian tubes were of normal size; but, while the right was nearly in its normal position, so far as distance from the cervix uteri was concerned, the left originated from the mass three inches higher up.

On making an incision through the part where the distinct tumour could be felt, the latter was found to be a fibrous growth, already softening and degenerating in its central portions. It must have originated at the right side of the fundus of the uterus and in its substance, though near the surface; and, as it increased in size, elongated the uterus, also elongating its cavity and turning it to the left. The sinuses of the uterus in its vicinity were much enlarged, and hence the fluctuation already spoken of.

The accompanying diagram gives an idea of the mass removed. A and B represent the right and left Fallopian tubes; C, the tumour bounded below

by the fine line; and the dotted lines show the form of the uterine cavity. The small sac, containing fluid, is seen at the right and below C.



Condition and Progress of the Patient subsequently to the Operation.—

The following account is condensed from an hourly record made by my pupils, Drs. Lansing, Burleigh, and Lunt, who constantly attended the patient in turn till her death.

To avoid repetition, I here remark that she was to take only milk porridge, or bread-water, and, later, broth, as nutriment; to take McMunn's elixir of opium, or acetas morphiæ, to control pain or restlessness after reaction was established; and brandy, to aid in inducing the reaction, if required. The bladder was to be evacuated once in six hours, or oftener, if desired, by *catheterism*, for the first five or six days; and the warm water dressing was to be renewed every four hours. I shall call attention particularly to the pulse, the respiration, and the amount of urine, and divide each day into four parts, commencing at 6 A. M.

The patient completely recovered her consciousness soon after being put in bed, at 5½ o'clock P. M., and complained only of fatigue and pain in the back. At 9 P. M. reaction is established, and she is comfortable. Pulse, 96; respiration, 20; evacuated 3iij urine. Gave gtt. xxv McMunn's Elixir. At 10, pulse 104. She has a desire to evacuate the bowels. Gave an enema of ½ gr. acetas morphiæ in water, 3j. Drew 3j urine, she complaining that all had not flowed before. At 11, had slept fifteen minutes; surface warm; pulse, 102; respiration, 18. Gave acetas morph. gr. ⅙. At midnight, she had slept one-half hour more. She is warm and comfortable, and sleeps one-half of the time from 12 to 1 o'clock. 2 A. M., says she is hungry; has slept most of the past hour. At 3, pulse 98; respiration, 18; obtained urine 3iij. Having complained of thirst since the operation, she had drank, perhaps, 3viii, in all, of water. She now vomited most of this, and felt better afterwards. But she has also been coughing frequently; hav-

ing taken a cold, two nights ago, by sleeping with her window open. As she knew I would have deferred the operation had I known this fact, she had concealed it from me. After the vomiting, till 6 A. M., she remained very comfortable, often sleeping a few minutes at a time. Pulse from 96 to 88; respiration always 18. After examining the dressings at 4 A. M., I left in an early train for my home, sixty miles distant.

First day after Operation.—6 A. M. to 12. At 6, drew urine 3ij. Is now complaining of a pain which she refers to the womb. At 7½, the urine is passing involuntarily in very small quantity; continues only two or three hours. Up to 9, she complained of no pain; but of wind in the stomach which she made efforts to raise. The pulse is now 104, respiration 18. Has coughed frequently since 6 o'clock. At 10, the pulse is weaker. Gave 3j brandy. There is a disposition to vomit, after taking some porridge; which is allayed by some ice. At 11, pulse 108, respiration 15; sickness and efforts to vomit continue. On examining the dressings at 12 (noon), Dr. Lansing found the tube had discharged 3j of bloody serum, and injected through it an ounce of the artificial serum, which came away again. But he also found 2 *hernial protrusions*—a small one near the lower end of the incision, and a larger near the upper end. The former was reducible; but the latter was not. And, as efforts to reduce it made the patient vomit, he at once telegraphed to me the state of things. I replied that it must be reduced, and the air excluded by the dressing till I could arrive there; which must be as late as 4½ to 5 P. M. From 12 to 5 P. M. (the time of my arrival) she continued to have all the usual symptoms of strangulated hernia; the pulse becoming more rapid, but feeble, and the patient restless and oppressed with thirst. Retching still continued; and she had been supported by brandy. I found a portion of intestine as large as a hen's egg protruding between the uppermost needle and the upper extremity of the incision; and another of the size of a walnut, between the lowest two needles. Both were strangulated and irreducible herniæ; both were very livid, and covered with a plastic exudation. Pulse 128, and very feeble. I withdrew the needle below the protrusions in each instance, and returned the strangulated intestine; and then replaced them and added two other needles for greater security, and left a tent in place of the tube. All this still farther reduced the patient, and I supposed that this unfortunate occurrence must turn the scale with her, as a matter of course; and very likely she would not react at all after the shock. From 6 to 8, patient rested well, dozing much of the time. Two oz. of urine had been withdrawn just before the herniæ were reduced. At 9 in the evening, pulse 130, fuller, respiration 17, tongue moist, slight white coat. At 10, the pulse was 125, surface natural. Is lying on the right side, and complains somewhat from uneasiness of position. Changed to lie upon the back, she soon wished to resume the former position. At 11, temperature was diminished; gave brandy and water. Is troubled by her cough. Gave ¼ grain of morphia. Slept at short intervals. 12 o'clock (midnight), catheterism, f3iv. Pulse 125, more feeble; respiration 16. Gave nutriment and brandy. Add more bed-clothing. 1 A. M., pulse 136. Coughed several times, and raised freely a tenacious mucus. Gave ⅙ grain morphia. Slept well afterwards; temperature rising. From this time till 4, she made frequent ineffectual attempts to expectorate mucus, which somewhat exhausted her; the temperature of surface continued sufficiently high; slept ¾ hour quietly at one time. Pulse 125 to 130. Took brandy twice, and ⅙ gr. morphia at half past 3. At 4 A. M., pulse 140, respiration 17; is placed upon the side; complains of being tired; is very thirsty. She continues thus,

with temperature rising till 7, when she had been sleeping quietly for half hour; pulse 135.

Second day after Operation.—6 A. M. to 12. At 8 A. M. feels very comfortable. Compress on abdomen stained with bloody serum that had passed by the tent. Abdomen somewhat distended by gas; no increased tenderness. Introduced catheter; no urine obtained. Expectorates much thick mucus. Slight eruptions on the chest, produced by the morphia. Pain on coughing, all referred to the umbilical region; no pain elsewhere. At 9½ to 10, sleeping quietly; skin moist and natural; pulse 128, respiration 16. At 10¾, obtained 3iv of urine. She was soon after turned on the side, and both brandy and nourishment were given. At noon the pulse was 136. Awoke quite refreshed after an hour's sleep, and took some chicken broth. The tongue is more coated. She continued sleepy, and with a moist skin till 3 o'clock, when about 3vi urine of natural appearance were drawn. Viscid mucus was freely raised several times. At 5 (48 hours after the operation), pulse 140, and fuller, respiration 21; surface less warm. At 6½, 3iv urine were drawn, and patient changed to other side of bed. The gaseous distension of the abdomen not increased; the parts look well. Up to midnight, she had not slept much, and complained of pain in the abdomen. Took morphia twice, and brandy and broth freely. Pulse generally small and feeble, over 130. At 11, drew 3v of urine. Has difficulty in raising mucus still. 12 (midnight), abdomen still tympanitic; in other respects appeared right. She dozes somewhat; complains of feeling faint. Pulse slightly irregular, 118 to 98, respiration 20 to 17; dreamed of home; gave brandy and broth freely. At 5, respiration was 12; probably reduced by the morphia; pulse 125, feeble.

Third day after Operation.—6 A. M. to 12. Temperature natural; tenderness and swelling of abdomen not increased. Withdrew f3ij of urine. Patient inquires how long before she will be able to go home. Tongue as yesterday, except drier. She did not complain at all this forenoon, except in connection with her efforts at expectoration. The pulse improved in character, and continued from 118 to 128, the respiration being 12 to 14. Morphia ½ grain was given at 9 o'clock. At 10, she passed urine quite freely; and slept quietly from 11 to 12. Has, in fact, had a very comfortable forenoon. 12 to 6 P. M. At 12 (noon), she passed more urine, and the catheter withdrew 3ij more. Swelling of abdomen not increased. In the P. M., she makes slight attempts at times to cough, but has difficulty in expectorating. At 3, drew 3v urine. Took morphia at 1 and 4½. The pulse is more distinct and regular, and from 130 to 120; respiration 16. Has slept some twice, and complains of itching (from the morphia). Starts in her sleep, but does not moan; countenance natural; and has had a very comfortable afternoon. 6 P. M. (72 hours after operation). Abdomen less distended than 24 hours ago, and not tender on slight pressure. From this time to midnight, patient slept most of the time very quietly, the skin being moist and natural. At 9, 3ij urine were drawn. The pulse was 124 to 128, and the respiration 16. 12 o'clock (midnight). Remains the same. At 2 A. M., and at 5, about 1 oz. of urine; and for two hours (and then after 4 again) there was great heat and dryness of skin, and thirst. She kept two hours at intervals; being restless at 4, took morphia. Pulse from 128 to 130, and feeble; respiration 15 to 16. Broth and brandy were freely given. Once expectorated freely a thick yellow mucus; and expelled some gas from the stomach.

Fourth day after Operation.—6 A. M. to 12. I spent this day with the

patient. Early in the morning the pulse is irregular, and she first ejects from the stomach a yellow fluid (apparently bile), which she says is very acrid. This continued at intervals all day, the quantity at each time, varying from $\mathfrak{z}\text{j}$ to $\mathfrak{z}\text{iv}$. Brandy and broth are freely given, but the pulse continues small and irregular, and from 125 to 130; respiration being 18. Gave $\frac{1}{6}$ grain calomel every two hours. Urine $\mathfrak{z}\text{j}$ at 9 A. M. Complains of griping pain in the bowels. Give *aq. menthæ piperitæ*. Applied a sinapism to the epigastrium. In the afternoon, used chloric ether, the vomiting of bile continuing; though she says she does feel nauseated. It became quite violent at 3, and the ejected matter of a darker colour. Pain increases in the abdomen; also felt in the chest. Applied fomentations to latter. Pulse from 130 to 140, and feeble; respiration 25 to 30, expiration longer than inspiration. Urine $\mathfrak{z}\text{iv}$ at $4\frac{1}{2}$ P. M. Is less pain in side. 6 P. M. (96 hours after operation). I noticed a faint gangrenous odour on examining the dressings. Gave an enema of soap and water at 9 P. M. She vomited at once. Gave $\frac{1}{2}$ drop creasote every two hours afterwards during night. Vomited often up to midnight. Urine $\mathfrak{z}\text{iv}$ at 10 P. M., reddish coloured. Just before 12, complains of deafness in the left ear; has slept some; extremities cold. Pulse has been 120 to 132, very feeble; respiration 25 to 32. 12 o'clock (midnight). Feet warm, hands still cold. A cold perspiration appears at 1; she vomits afterwards; wishes the window opened at 2; pulse scarcely perceptible at 3; respiration being 24; has moaned somewhat the last half hour, and tosses her head about in distress; thinks at 4 that she is dying, and expresses a desire to die, and a Christian hope. The vomiting continues at intervals. She is moribund at 5; vomits still; but lingers till 20 minutes past 7 A. M., when she expired on the fifth day after the operation.

Post-mortem Examination eleven hours after death.—Present, Drs. Jewett, Newell, Bancroft, and Cahoon, and my pupils, Drs. Lansing, Burleigh, and Lunt.—Slight œdema of skin upon the abdomen, and slight tympanitis. Made a narrow elliptical incision through abdominal walls, and include the original incision and two inches on each side of it. The right rectus muscle was in a gangrenous condition. The original incision was completely united, both externally and internally, except where the two herniæ had occurred; an inch remaining ununited at the upper end of the incision.

The *omentum* was adherent at its right lower border to the small intestine. A little blood, just enough to produce a stain, was extravasated upon the right portion of the stomach and the omentum.

Intestines.—The portion of intestine forming the largest (upper) hernia was livid and gangrenous, and slightly adherent to the edges of the upper part of the incision, which was not yet closed. There was a similar slight adhesion at the position of the lower hernia. There was some adhesion to the peritoneum at intermediate points. No strangulation at any point, but a general livid appearance of the small intestine, apparently having commenced from the two portions constituting the herniæ at first, and corresponding to the track of the wound. Some convolutions were also adherent to the left iliac fossa, and to each other; and others falling into the pelvis adhered slightly to and behind the portion of the uterus still left in the pelvis.

The small intestines were somewhat distended with gas.

Four ounces of bloody serum were found in the pelvis; and the peritoneum there was of a livid hue.

The stump was completely covered with a thick layer of plasma already beginning to be organized, and healthy in appearance. The length of the re-

maining portion of the uterus was two and a half inches. The stump was $1\frac{1}{2}$ inch wide, and 1 inch thick.

Remarks on the preceding Case.

1. Each reader will decide how far the mistake in the diagnosis was a culpable one. I availed myself, I believed, of all the means and appliances afforded in the present state of science to elucidate such cases, and the facts discovered were such, as in my opinion, could lead to no other rational conclusion than that we had here an ovarian tumour. The history of the case, the data afforded by the touch, whether applied externally, *per vaginam*, or per rectum, and by the uterine sound, and the sound in the bladder, all confirmed the idea of an ovarian tumour; and though some of the facts were also compatible with the supposition that the tumour was uterine, others seemed not to be so at all.

So far as form, position, and mobility were concerned, the tumour might be either ovarian or uterine. But the fact that the tumour could be moved in all directions without moving the uterus, while the sound was in the latter, the sound having passed three and a half inches into the uterine cavity and seeming to reach its upper terminus, was considered entirely inconsistent with the idea that the tumour was in the substance of the uterus.

But why did not the sound penetrate further into the uterus? Because, as the post-mortem examination showed, the cavity of the uterus turned to the left at the point reached by the extremity of the instrument. And the displacement of the mass in all directions, while the sound was in the uterus, without imparting motion to the uterus except in the slightest degree (as slight as would have been produced by an ovarian tumour, on being pressed against that organ), was subsequently accounted for by the elongated and slender form of the lower part of the body of the uterus, and its consequent flexibility at and above that point. In another similar case, I should now use a *flexible bougie* in exploring the length of the uterine cavity, after employing Simpson's uterine sound, as before; and finding the latter would pass only the usual distance. It will appear that the diagnosis actually turned upon this single point. If I had succeeded in passing the uterine sound to the upper extremity of the uterine cavity (five and a half inches), I should have consequently detected its enlargement, and also have fixed it so firmly that the flexibility of the lower part would not have deceived me.

But my idea of an ovarian tumour was also confirmed by the distinct *fluctuation* perceived at the examination two days before the operation. Of course a uterine tumour may also fluctuate, though very rarely when so firm and large. And this fluctuation still deceived me, even after the tumour was exposed to view during the operation. It was found to be owing to the fact that the uterine sinuses were enormously enlarged in the layer of uterine structure covering and surrounding the tumour, and were of course full of blood.

Still another thing influenced my opinion in no small degree. I allude to

the small sac containing fluid, which I could distinctly feel through the abdominal walls, on the anterior surface of the tumour. I had before repeatedly felt such projections on ovarian tumours, but never on a uterine tumour; and such a sac had always hitherto proved a true indication of the nature of the mass on which it was found, viz: that the latter consisted of a congeries of sacs of various sizes containing fluid.

I trust that by calling the attention of the profession to these causes of error on my own part, some may be put on their guard against similar results in diagnosis. In another precisely similar case, I should now adopt it as a principle to decide positively and conclusively by various explorations of the uterine cavity, that it was *not a uterine tumour* before I decided that it *was ovarian*. But if, after all, I could obtain only precisely the same data as before, I do not now perceive how I could rationally arrive at any other conclusion than the one erroneously adopted in the present case.

2. But should the uterine mass have been removed when the real state of things became apparent? I have no desire to avoid this inquiry. Many will doubtless, at once reply that the mass should have been returned, though already punctured and bleeding, and the wound through the parietes of the abdomen closed up. All the medical gentlemen present, however, thought differently. I mention this, not to throw any part of the responsibility upon others, but to show that I acted deliberately, at any rate. I should never forgive (any more than I should respect) myself, should I perform an operation, or any part of one, which I myself did not, in the circumstances, consider justifiable, whatever might be the opinions of others. And there were circumstances not likely to occur to the mind of one not present at the operation. The patient was retching violently, and it seemed utterly impossible to return the tumour and retain it there by needles, and the usual appliances in the present spasmodic condition of the muscles, even if the tumour had not been punctured. And it is still my opinion that, if it had been returned *sound*, the cough would have produced the hernial protrusions, which subsequently did occur, to a still greater extent and certainty. The abdominal walls had not been distended as in cases of ovarian dropsy; and, therefore, the removal of the tumour rendered hernial protrusions less liable to occur from leaving the abdomen so much the less full and tense. But to return the mass still bleeding was out of the question, and to stop the hemorrhage for the time, and return it with the certainty that it would again occur internally, was an expedient at once rejected.

I had, however, realized the possibility of an error in diagnosis; and in endeavouring to include all the *possibilities of the case* in my mind, the very thing I now found to exist had occurred to me, and I had previously decided what would probably be best in the circumstances.

But this decision involved the removal of the *entire body of the uterus through the abdominal walls*, an operation which had not been performed in this country, so far as I am aware. This organ had been successfully removed through the

vagina in cases of cancerous affection by Sauter, Recamier, and Blundell. The operation decided on had once been performed by Langenbeck. Dr. Clay, of Manchester, England,¹ had also performed it; the patient dying of hemorrhage seventeen hours afterwards. Aside from the risk of hemorrhage, the danger of life seemed to me even less great of removing the body of the uterus in this way than of removing the entire organ through the vagina.² The shock could be no greater,³ and the peritoneal cavity would not be exposed at all after the incision in the abdomen was closed, as it must be to some extent after its removal through the vagina.

Such were the facts and the reasoning which had previously prepared my mind for the removal of the uterus, if the circumstances seemed to require it.⁴

3. But was not the removal of the uterus the cause of the fatal result? I should reply in the negative. If the removal of this organ proves fatal, I suppose it must be so from the *shock*, from *hemorrhage* at the time, or subsequently (as in Dr. Clay's case), or from *inflammation*, either peritoneal or extending from the seat of the amputation to the surrounding parts. But death from shock would have occurred in a few hours, at all events within two days. I provided for hemorrhage (the cause of death to Dr. Clay's patient), by the use of the elastic tube for the first twenty-four hours; but none occurred at all. I suspected at the time, however, that the desire to evacuate the bowels, soon after the operation, was produced by the pressure of blood flowing from the stump. Again, the stump was not inflamed at all, as found at the autopsy. On the contrary, all progress had been made which was possible towards healing over during the interval after the operation.

What, then, was the fatal element in this case? Merely opening the peritoneal cavity may prove fatal, but then the cause of death is *peritonitis*. In this case, the extent of the peritoneal inflammation was merely proportional to the slightly developed symptoms before death; and it alone, I judge, could not have proved fatal. Besides, its location was such as to suggest the idea that another cause than an exposure of the general cavity produced it. The symptoms, also, during the last two days, were those of gangrene, and not of peritonitis.

I am, therefore, constrained to regard the hernial protrusion, the morning

¹ Lond. Med. Gazette, vol. xxx. p. 309. 1844.

² Still there is a case on record in which the uterus was *torn away* by an ignorant midwife, supposing it to be another child; and yet the woman recovered.—*South's Chelius's Surgery*, vol. iii. p. 569.

Delpéch thinks the removal through the abdominal walls the safer operation.—*Memorial des Hôpitaux du Midi*, Oct. 1830, p. 695.

³ Gendrin thinks that in the cases in which death ensued within forty-eight hours after the operation (through the vagina), it was consequent on the depression caused by the pain from dragging down the uterus during the operation.—*Journal Générale de Médecine*, Oct. 1829, p. 91.

⁴ Since commencing this report, I have learned that Dr. G. Kimball, of Lowell, Mass., has performed this operation *successfully*.

after the operation, and the consequent strangulation and gangrene, as the cause of death. The intestine was strangulated for five hours after they were discovered, and probably ten or eleven hours in all (*i. e.*, from the time when the patient first began to cough—5 to 7 A. M.), and were all this time indirectly exposed to the air. I accordingly, at 5 P. M., found the protruded portions cold, livid, and covered with an exudation. Probably the vitality was so exhausted already, that no effort was made at reaction of the circulation after the intestine was returned. I had not the least expectation that there would be, at the time. The *post-mortem* appearance went to show that the gangrene extended from the portions of bowel which had been implicated, and thence to the peritoneum; while the peritonitis and the adhesions were principally found to correspond with the incision through the abdominal walls. Considering the state of the patient at the time of reducing the hernia, I did not expect she could recover at all from the shock of that procedure, and the necessary removal and reintroduction of the needles. And yet all this would probably have been avoided had the patient not concealed from me the fact that she had a cough.

4. Finally, I recall the idea of using an *artificial serum* in all cases in which the cavity of the peritoneum is exposed to the air. I have since used it in a case of ovariectomy, and, I think, with advantage. It is merely a common-sense idea that the more nearly the peritoneum is kept in its natural state of moisture and warmth, while exposed to the air, the less is the risk of inflammation of the same. And for this object the artificial serum and the warmth and moisture of the air in the room during the operation, were all deemed important.

BOWDOIN COLLEGE, *March 1st*, 1855.

REVIEW.

ART. IX.—*Registration of the Births, Marriages, and Deaths in Massachusetts for the year 1851.* 8vo. pp. 116.

The same for the year 1852. 8vo. pp. 144.

Report of the Secretary of State of Connecticut, relating to the Registration of the Births, Marriages, and Deaths in Connecticut for the year ending August 7, 1848. 8vo. pp. 16.

The same for the year 1848-9. 8vo. pp. 16.

The same for the year 1849-50. 8vo. pp. 28.

The same for the year 1850-51. 8vo. pp. 23.

Report of the Secretary of State of New Jersey of the Registry and Returns of the Births, Marriages, and Deaths in that State, for the year 1851. 8vo. pp. 56.

The same for the year 1852. 8vo. pp. 53.

The same for the year 1853. 8vo. pp. 53.

First Annual Report to the General Assembly of Kentucky, relating to the Registry and Returns of Births, Marriages, and Deaths, from Jan. 1 to Dec. 31, 1852.

THREE years ago, in our number for July, 1852, Vol. XXIV., we gave an analysis of the Registration Reports of Massachusetts, for nine years and eight months, ending December 31, 1850, and also of the two reports which had been published by the State of New York, for the years 1847 and 1848; and we then stated that these were all that were then known to have been published on this subject by any of the States of the Union. But here we beg leave to correct our error, and to state that Connecticut had then printed three annual reports, and New Jersey one report; yet neither of these had then reached us, nor did we know of their existence.

Since that time more has been done in those States, and still more is promised. Massachusetts has continued faithful to her undertaking, and sent forth two valuable reports for 1851 and 1852. Connecticut has sent forth other reports, of which we have received only one. New Jersey has given to the world the reports for 1852 and 1853; and Kentucky and Rhode Island have at once leaped into the field, full armed and energized, and published one able and instructive report, and another for the year 1853.

Some other States, as Pennsylvania and New Hampshire, have enacted laws for the establishment of the registration of the births, marriages, and deaths within their borders, and ordered these facts to be reported to, and published by, the duly appointed authorities; yet nothing has been done, and no such reports have been sent abroad from them.

In some other States efforts have been made to persuade their legislatures to establish this system of registration, to procure the records and returns of the three great facts of human life, and their publication by the public authorities.

The National Medical Association has repeatedly recommended this scheme for universal adoption; and we believe that the physicians in almost, if not

quite all the States have, either individually or collectively, urged it upon their legislators, and their men of political influence, to cause it to be done.

Rhode Island has published a valuable report, which has come to hand since this article was prepared for the press. Here, however, is all that is known to have been done by any of the States: twelve reports for twelve years and eight months from Massachusetts; four reports for four years from Connecticut; three or more reports for as many years from New Jersey; two reports from New York; one from Kentucky; and one from Rhode Island.

Several cities have published, and some for many years, their bills of mortality: Lowell, Boston, Roxbury, Providence, New York, Brooklyn, Philadelphia, Baltimore, Washington, Charleston, New Orleans, Memphis.

There may be others which have done this work; but if they have, we have had no information concerning it; certainly the reports of mortality have come to us from no other cities or towns beside these. We have made earnest inquiry of the authorities of some other of our largest and most prosperous cities for their reports of mortality, and have been informed that they have made no such record, although it would have been very desirable.

This work of State registration is almost new in this country, it has taken no great hold of the people; the medical profession have generally given it their support. Statesmen and political economists see the value of it, as developing the law of mortality and aiding in determining the law of population.

The national enumeration of the people, in 1850, included the inquiry as to the number of deaths during the year next preceding the first of June of that year, the sex, age, color, and cause of death of the person dying. The number of the deaths in each State, so far as the marshals learned and reported them, is published in the first volume of the census already printed. But a much more elaborate and minute account of the national mortality of that year is, we understand, in course of preparation in the Census Department, at Washington, and will be published, we trust, in satisfactory tables and statements in the volume which is to appear in the course of this year. In respect to this we will merely stop now to caution the public not to draw any inference as to the rate of mortality from those mortuary tables already printed. Although the inquiry was intended to reach every death in every district, county, and State, during the year proposed, yet the answers and returns are very imperfect; some whole counties made none, others reported half or less than half, so that the number printed in the tables, pages 12 and 140 of the Abstract, and page XII. of the quarto vol., afford no indication of the total number of deaths in the several States and sections of our country, and no confidence whatever can be placed upon any rate of mortality that may be deduced from those statements.

Nevertheless, we are glad of this inquiry, although it developed such imperfect answers, inasmuch as it has called the attention of the people to the subject; and we trust that the errors of the report in regard to the several States, will incite their people and governments to establish their own local systems of registration and reports, and then give such complete returns of their mortality as will be entirely satisfactory to the inquirer and reliable for those who wish to calculate the law of population and human progress by help of these facts.

The work of registration is essentially connected with civilization. The governments of all civilized nations desire to measure their power by counting their people and calculating the amount of wealth, in order to show what degree of physical force they can present for offence or defence, and what means they have of sustaining themselves. A higher state of civilization is

not content with counting the people merely, but is desirous to measure and weigh them, to ascertain not only how many there are, but what is their actual value. It wants to know not only how many there may be living, but how many are sent into life, and how long that life continues.

In this view of the matter, the political economist sees that there is a great difference in the amount of wisdom, power, and happiness that is contributed to the commonwealth, from the child in his forming period, and from the man in his full maturity; and the difference in the amount of good that has been contributed from the infant and the octogenarian is immense. A wise government, then, not only wants to know how many people exist, but how many are born, and how long they live. And beside all this, it wants to know how much that life is worth; what degree of fulness and vigour is, or may be, enjoyed through it.

These matters are determined, in great measure, by the Bills of Mortality, which show at what periods life is terminated, and what are the causes of death. Those governments that count their people do well; those that determine their duration of life, and the causes of death, do better; and those do best of all which search out the obstacles that stand in the way of the most complete development of life, or impair its vigour, or prevent its maintenance, and endeavour to remove the cause of vital depression, which they may thus discover. This belongs to an advanced state of society, in which the ruling powers shall gather, digest, and diffuse that knowledge which will lead the people to the fullest enjoyment of life.

The reasonableness of this system of registration seems so plain, that we had great confidence that, wherever and whenever it should be once begun, it would go on and the progress would be ever onward, and not backward; that every State or government, that should commence this system, would do more and more, obtain more perfect returns, and publish more complete reports year after year.

Unfortunately for this hope, we are compelled to record again the continued failure of New York, which published two annual reports in 1847 and 1848, indicating an interest in the subject, and desire to come up to the requirements of this philosophy, but since that time has done nothing. The law requiring the return of the facts from the cities and towns is not repealed, but it remains a dead letter, and the people do not regard it, nor do the authorities execute it. A law was passed to the same effect in Pennsylvania, in January, 1852, but no report has been made under it, and we understand that some desire its repeal.

New Hampshire enacted a law for this purpose, in 1849, and amended it in 1851, but has done nothing more. No reports have been issued, and, we suppose, no satisfactory number of facts have been reported from the towns to the officers authorized to obtain them.

Connecticut, which published four annual reports for the four years ending 31st July, 1851, has published none since.

Perhaps one ought not to be disappointed; we do not expect that every scheme of improvement will be adopted and executed as soon as it is proposed; nor even that it will always be carried on after it is once put into operation. The system of registration belongs to the highest philanthropy and cultivation. It was proposed at first, not by legislators nor even by statesmen or political economists, but by physicians and others, who were conversant with vital science, familiar with the depreciation of human life, and anxious to ascertain the cause of our short-comings in this respect. In regard to this subject,

these men are in the advance of the lawgivers, and these are in advance of the people. In America, where all power primarily and ultimately rests with the people, it is useless for the legislature to adopt any law unless it is an exponent of the popular will already existing, or finds a response to it, and a support for it, in that popular feeling after it is adopted.

Thus we find the National Medical Association, and the local medical societies, urging this matter upon their several legislatures. In most States their proposition has produced no action of the government, however much some of the wisest statesmen may have it at heart. In some States, as in Pennsylvania, New Hampshire, and perhaps some others, laws have been adopted to this effect, but no fruits have been produced, no returns gathered, or at least no reports published. In one State, at least—New York—the law was enacted and went into operation, and then it stopped for want of popular co-operation. It is plain that, in the last State, the law of the legislature was in the advance of the people, whose sympathies have not yet been sufficiently enlisted to co-operate in the work and report to their Secretary of State, and enable him to give the world the account of their mortality.

Nevertheless, we hope for better things; we believe the seed once sown, however long it may be in germinating—even though it rot and die—will ultimately spring up and send forth fruit.

We now proceed to analyze the reports which we have before us.

There is in Massachusetts, and in the other States, no such officer as Registrar, or Secretary of the Board of Health, to whom the work of registering the births, marriages, and deaths is especially committed, and who is selected for that office on account of his familiarity with the law of population and of mortality, and who has the exact mental discipline and cautious habits of reasoning, which would enable him to discharge the duties of his office in the most perfect manner. But here and elsewhere, this work is made an incidental part of the duty of some officer who is selected for other purposes, and has other duties sufficient to occupy all his attention.

In Massachusetts, Connecticut, and New Jersey, this duty is confided to the Secretary of State, and in Kentucky to the Auditor of Accounts. These functionaries are already sufficiently burdened with the legitimate business of their offices, and are necessarily compelled to employ some other person to analyze, digest, and arrange the returns from the local authorities, and prepare the reports of births, marriages and deaths.

In Massachusetts, the Secretary being compelled to employ substitutes, fortunately intrusted the work to able and competent hands. The report for 1851 was prepared by Dr. Josiah Curtis, who had prepared the two previous reports. That for 1852 was prepared by Dr. Nathaniel B. Shurtleff. These two reports, like the eighth and ninth prepared by Dr. Curtis, and the fourth, prepared by Mr. Shattuck, are very valuable and instructive; they are rich storehouses of this class of facts. These now under consideration are very similar to each other, and to those which had immediately preceded them. Their main tables are the same; the deductions and the reasoning alike; but there are the same deficiencies, and we feel, as after reading their predecessors, the same want of more of that fulness of concentrated philosophy and sum of wisdom, that might have been drawn from the whole experience of the preceding years.

There seems to be more and more attention paid to this subject by the people and the local authorities; out of three hundred and twenty-two towns and cities, in 1851, only nine towns, with a population of 11,056, failed to make returns

of deaths; eight with 8,040 inhabitants, and nine with 3,573, made no report of births. In 1852, out of three hundred and twenty-four towns, only five, with 6,834 people, returned no births, and no marriages; and four, with 5,537 inhabitants, reported no marriages. There were manifestly imperfect returns, in 1851 and 1852, of births and deaths from Barnstable and Middleborough, with a population of 10,340. It is worthy of notice, that all these omissions occurred in fifteen towns, which contain only 24,384 inhabitants.

We are not sure that all the other towns made complete returns of all those events that occurred within their borders. Indeed, in some there seems to have been a remarkably low rate of mortality, as well as a very small proportion of marriages and fecundity. Nevertheless, these did not vary so far from the average of their neighbourhoods as to render it expedient to deviate from their report.

From the whole State there were reported—

	Births.	Marriages.	Deaths.
1849	25,773	6,936	20,423
1850	27,664	10,345	16,606
1851	28,681	11,966	18,934
1852	29,802	11,578	18,482

These figures show a progress of attention to the requirements of the law. The increase is in a greater ratio than the increase of population.

The main facts are stated in thirteen tables in the reports of 1851, and in seventeen tables for the year 1852. In these two reports, the first nine tables are alike; the other four differ in some points, and four more are added to the last report.

The first table, which is not numbered, states, in regard to each town, the population in 1850; the number of births of each sex; the number of native and of foreign parentage; the number of marriages of Americans and foreigners; the number of deaths of each sex; the average longevity of those whose age is reported. The same facts are given for each county and for the State. This is the same in both reports.

The second table, numbered I., shows, in each county, the number, both of males and females, born in each month and in the whole year. The parentage of the children born, whether American or foreign, is given for each county. In the same table the plurality cases are, as stated, in the same way, except as to parentage.

Table II. states the same facts as to the stillborn.

Table III. shows the marriages in each county and in each month; and, also, the nativity of the marrying parties in the several counties.

Table IV. shows the approximate age of each of the parties at their first marriage, and in those cases where the male has his first and the female her later marriage, and *vice versa*; and, also, where both parties had been previously married.

The next, V., is a table of mortality, and shows the number of deaths of each sex who died at the several approximate ages in the several counties and in the State.

Table VI. shows the mortality of males and of females in the several counties and at the various periods of life.

In Table VII. is a list of causes of death, alphabetically arranged, and the number of males and females that died from each disease. In connection with these facts, one set of columns states, in regard to the different sexes, the number who died in each month, and another, the deaths at each approxi-

mate age. The whole is condensed and stated as to sexes and months, and, also, sexes and approximate ages.

In Table VIII. the causes of deaths are classified, and the number who died from each cause and each class of causes, and in each county, is given. Thus far the tables have been alike in both of these reports, and the statements similar.

The subsequent tables differ in some points.

In the report for 1851, Table IX. gives the classified list of diseases, and, in connection with this, the deaths from each cause, through nine years and eight months, to December, 1850, and in the year 1851, and the males and females who died in the three years, 1849, 1850, and 1851.

Another set of columns shows the per cent. of deaths from each cause in the same periods, and the same division as to sexes in the three years.

A corresponding table in the report for 1852, in connection with the classified list of diseases, shows the number of deaths from each cause through the whole period of registration, eleven years and eight months. In each of the years 1849, 1850, 1851 and 1852, separately, the total of the four years, average annual mortality of these four years, and the percentage of the whole from all causes that died from each cause in each of these periods, except the total four years.

Table X., for 1851, shows the average age of the persons that died over 20 years old, in each occupation, in the year 1851, and in the seven years and eight months previous.

In 1852, the same is shown in the nine eastern counties and in the five western counties, separately for 1852, and, in the whole State, in the eight years and eight months previously.

In Table XI., of the report of 1851, is a list of the principal towns which made full reports, with their census for 1840 and 1850; the annual average through three years, 1849 to 1851, of the births, marriages, and deaths, and the excess of births over deaths; the rate of all these three events to the population of 1850; and the average age at death during these years. These facts are stated in regard to all the towns which are presumed to have made full reports, and to each county and the State.

In the report for 1852, the corresponding table gives what is called a four years' abstract, showing, in regard to each town and county, and the whole State, the population, male and female, in 1850; the births and deaths, male and female, and marriages for the four years, and the annual average; and the average age at death through these four years, 1849, 1850, 1851, and 1852.

In the report for 1851, Table XII. shows the same facts as were developed in Table IV., in respect to the marriages during the six years and eight months ending December 31, 1850.

Table XIII. is "intended to illustrate the influence of domestic condition on longevity;" showing the average age at death of the unmarried, the married, and the widowed, in each county and the State, through four years.

Table XII., in the report for 1852, gives a four years' abstract of the births; showing the number of males and females who were born in each month, and in each county, and in the State; and also, the parentage, whether American or foreign, of each sex in the several counties. The same is stated in regard to the plurality cases and the stillborn.

Table XIV. makes a statement of the marriages in each county and month, and nativity, whether native or foreign, for the counties, through four years.

Table XV. shows the mortality of males and females in the counties and months, through four years.

Table XVI. shows the number of deaths of males and females, through four years, in the several counties, at the several approximate ages.

These are all the tables in the two reports. They are followed by very valuable and instructive commentaries, extending through over fifty pages in the first, and sixty pages in the second of these reports. These dissertations are worthy of the careful attention of people in every State. The observations in the Report for 1851, give an analysis of the population. The remarks upon the births, marriages, and deaths, the fatality from several causes, the laws of mortality, are faithful to the facts which have been before shown, and illustrate the laws of population and of human progress. A similar testimony must be given to Dr. Shurtleff's admirable observations at the end of the report for 1852, which, although less elaborate, and involving somewhat more error than those of his predecessor, yet they contain so much that is valuable that we would commend them to the careful study of all interested in these matters.

We have before said that these reports are more full and satisfactory than those which had preceded them in Massachusetts, or had come from any other State; but they are not complete. Dr. Curtis and Dr. Shurtleff are compelled to complain of the neglect of a few towns, which, we trust, will be induced, by argument or by the law in future, to make perfect and true records and returns of their births, marriages, and deaths.

In both of these reports, the authors of the observations have assumed, when they made their calculations, that the returns of their facts were complete, and given us the rates of births, marriages, and mortality to the whole people, and not to those from whom the returns were actually received.

In the three years' abstract is the report for 1851, and in the four years' abstract of the report for 1852, the calculations are made for the counties and the State, as if no towns had been omitted. In some of these four years, twenty-three omitted to return the births, twenty-six omitted marriages, and twenty-three omitted deaths, and four more made manifestly imperfect returns of their mortality.

In the four years' abstract, the births in twenty-three towns, the marriages in twenty-six towns, and the deaths in twenty-four towns are left blank, probably not having been returned, or so imperfectly as not to justify their being reported; and the deaths in four other towns which are herein printed, are so far below the average of their neighbourhood or county that they are untrustworthy.

We have calculated the probable number of these events in those towns which made no report, or manifestly imperfect reports, from the averages of other towns adjoining, and of the counties to which they belong, where the population was of similar character, and there probably was a similar proportion of these events. With the aid of this calculation, we have a complete statement of the births, deaths, and marriages in Massachusetts, in the several counties and the whole State during four years.

We have also calculated the population of the counties and the State for 1849, 1851, and 1852, and taken from the census that of 1850, and obtained the average population in each of these four years; and thus we are enabled to give the approximate average number of births, marriages, and deaths, and the annual rates of these events for the years specified.

Average Population of Massachusetts for the Four Years 1849 to 1852.

	AVERAGE POPULATION.			Area of Sq. miles.	Inhabitants to one mile.
	Males.	Females.	Total.		
Whole State	497,933	516,698	1,014,631	7198.5	140.9
Six sea-coast counties . .	158,064	161,433	319,497	1853.6	172.3
Bristol and Middlesex . .	117,628	125,876	243,504	1285.4	181.7
Five western counties . .	151,172	151,494	302,666	4049.	74.7
Suffolk	71,069	77,895	148,964	10.5	1418.7
State out of Suffolk . .	426,864	438,803	865,667	7188.	120.4

Average Annual Mortality in Massachusetts for Four Years, 1849 to 1852.

	AVERAGE NUMBER OF DEATHS PER YEAR.			RATE OF MORTALITY.					
				ONE IN			PER CENT.		
	Male.	Female.	Total.	Male.	Female.	Total.	Male.	Female.	Total.
Whole State	9,272	9,616	18,888	53.7	53.7	53.7	1.86	1.86	1.86
Six sea-coast counties	2,580	2,727	5,307	61.2	59.2	60.2	1.63	1.69	1.66
Bristol and Middlesex	1,976	2,084	4,060	59.5	60.4	59.9	1.68	1.65	1.66
Five western counties	2,557	2,731	5,288	59.1	55.4	57.2	1.67	1.80	1.74
Suffolk	2,159	2,079	4,233	32.9	37.5	35.1	3.02	2.67	2.84
State out of Suffolk . .	7,113	7,542	14,655	59.6	58.1	59.0	1.66	1.72	1.69

Average Annual Births and Marriages in Massachusetts for Four Years, 1849 to 1852.

	BIRTHS.		MARRIAGES.	
	Number.	Ratio to Population.	Number.	Ratio to Population.
Whole State	28,588	One in 35.49	9,530	One in 106.4
Six sea-coast counties . .	8,711	36.67	2,966	107.7
Bristol and Middlesex . .	6,652	36.6	1,784	137.
Five western counties . .	7,758	39.2	2,394	127.4
Suffolk	5,567	27.24	2,386	62.43
State out of Suffolk . .	23,121	37.44	7,144	121.1

This table gives us the most and the only complete and reliable rate of mortality that we have seen from any State in this country. It includes the year 1849, when the cholera prevailed to some extent, and other diseases connected with the digestive organs were more than usually fatal. There being 5,577 deaths in that year, against an average of 3,135 from the same causes in the three following years.

The total reported mortality of that year, from all causes, was 20,427, against an average of 18,007 for the years 1850, 1851, and 1852. But, as the mortality of 1849 was greater than the average, so that of 1850 was less, being only 16,606, which was as much below that of the following year as that of 1849 was above it. The average mortality of 1849 and 1850 was 18,514, and that of 1851 and 1852 was 18,713; showing a difference less than the increase of population of 3.3 per cent., which would have given 19,132 as the reported average mortality of 1851 and 1852.

No other State has yet obtained a complete, or so nearly complete, returns of the deaths within its borders as to justify a calculation of its rate of mortality.

There is a difference in the mortality of the sexes in the different divisions of the State, yet a very remarkable and exact coincidence in the whole State. Without doubt there is a difference in the mortality of the sexes in the various ages; one period being more fatal to one, and another to the others. We have neither time nor space to go into this investigation now, yet it is worthy the attention of those who study these matters to look more carefully into the rate of mortality of males and females at the different stages of being.

The various rural districts of the State present some, but not a large, difference of mortality; being the highest, ten deaths among 591 males, and ten among 584 females in the five western counties, the most exclusively agricultural, and ten among 604 females in Bristol and Middlesex, which are near and adjoining the sea-coast, and much engaged in manufacturing, trade, and agriculture; and the lowest, ten in 612 males in the sea-coast counties.

The total mortality of the sea-coast counties was ten in 602, and in the western counties, mostly beyond the highlands, and protected from the east winds, ten in 572. This difference may be due to the difference of social and domestic condition, rather than to the climate, atmospheric or endemic influences. There is a very striking difference between the rate of mortality in Suffolk County, which is composed mostly of the city of Boston, and that of the rest of the rural districts of the State, being in Suffolk ten in 329 males and in 375 females, and ten in 351 of both sexes; whereas, in the rest of the State, these proportions were respectively ten in 596, 581, and 590; showing an excess of 94 per cent. in the rate of mortality of the city over that of the country.

If we compare the twelve cities with the rural districts, we shall find this difference to be still more striking.

We put the facts into a table, to show the comparison for the same period of four years, 1849 to 1852:—

Males.

	Calculated Average Population.	Average Annual Mortality.	RATIO OF MORTALITY.	
			One in	Per cent.
Twelve cities . .	159,336	4,021	39.62	2.52
Rural districts . .	336,261	5,251	64.03	1.56

Females.

Twelve cities . .	130,437	3,983	45.3	2.207
Rural districts . .	338,597	5,633	60.1	1.66

Both Sexes.

Twelve cities . .	339,773	8,004	42.45	2.35
Rural districts . .	674,858	10,184	62.00	1.61

Births.

	Number.	One in	Per cent.
Twelve cities	10,999	30.9	3.23
Rural districts	17,589	38.36	2.60

Marriages.

Twelve cities	4,920	68.	1.45
Rural districts	4,611	146.3	.68

Thus we see that in the different sexes as often as 100 die in the country districts, 161 males and 133 females, and 155 of both sexes die in the cities.

It is well known that the composition of the population differs in these dense and sparse towns. In the former the population is progressive, growing in some of them rapidly.

It is a matter of deep regret that in the volume of the national census, recently published, the abstract of ages is not extended to towns and cities, but is limited to counties. We have no legitimate means of determining the relative ages of the population in the city and country. It is admitted, as a general fact, that there is a difference, and that the population is older where it is scattered and engaged in agriculture, than when it is condensed and occupied in commerce and manufactures. Although this is not now demonstrated in regard to the cities of Massachusetts, yet the statements of the former censuses in regard to these and other cities of the United States, and the analogy of cities in other countries, will justify us in assuming that there are in these cities a greater proportion of people in the younger and middle ages of life—that the population is generally more active, and there are more inducements for marriage in the compact towns than in the country. This is seen by the comparative ratio of marriages and of births. Recurring to the preceding table, we find that, among 10,000 population, the marriages in the country were 68, and in the city 145. The births were 260 in the country, and 323 in the city; that is, as often as 100 were married, and 100 born in the rural districts, 213 were married and 124 were born in the compact towns. This, of course, produces a larger proportion of infants and children, who would increase the rate of mortality, without diminishing the expectation of life.

There is another matter of consideration in the different original elements of population in the cities and country of Massachusetts. In the former is a very much greater proportion of foreigners, especially the Irish, than in the latter. This proportion is greater in Boston than in other cities. Dr. Chickering, in his report on the population and mortality of Boston, states that, in 1850, there were 75,466 Americans and their children, and 63,466 foreigners and their children, in that city. The mortality, through four years was, of Americans, 9,754, and of foreigners and their children, 11,816. The marriages were, in three years, of Americans, 3,932, and of foreigners, 4,643. Births in four years, of Americans, 7,153; foreigners, 13,536. Thus, while there were only 82 foreigners for every 100 American inhabitants, there were 121 deaths, 118 marriages, and 189 births among the former, for every 100 among the latter; showing an enormous disproportionate excess of these events among the foreign population of Boston during the years here noticed.

In 1850, the foreigners constituted 16.5 per cent. of the whole population of the State. From the first table in these reports, we are enabled to distinguish the parentage of those who were married, and of the children who were born, in the years 1850, 1851, and 1852. In these years the births were, of American parentage, 46,384, or 56.2 per cent.; and of foreign parentage, 36,342, or 43.8 per cent. of the whole. The marriages were, American, 22,421, or 68 per cent., and foreign, 10,291, or 31.4 per cent. of all. Thus, while in 1850, for every 1000 natives there were only 199 foreigners, there were in that and the two following years, for every 1000 children born in American families, 457 born in foreign families; and for every 1000 marriages of natives, 778 marriages of aliens.

In this connection it should also be considered that a larger proportion of the foreign immigrants are of the marriageable age, between 20 and 40 years

of age, than is found in any fixed and stationary population; they would, therefore, have a greater ratio of marriages and births than the natives until their first generation shall have passed away.

These foreigners are generally among the poor, and belong to the labouring classes. They perform the hardest work, and are exposed to the greatest dangers of the elements, heat, cold, and wet. They, in the cities especially, live in crowded and unventilated houses and rooms. They are addicted to unhealthy habits of drinking and smoking tobacco. They are subject to many privations, and exposed to many causes of disease more than the natives. Their children have less chance of life, and they are supposed to be subject to earlier death than the Americans.

In calculating the rate of mortality these facts should be considered, and, in determining the expectation of life in any State, this distinction should be made between the foreigners and the natives, who have such widely different habits and such different elements of vitality.

"No table has been prepared to show the relative deaths of natives and those of foreign birth. It seems desirable that, in future abstracts, this element should be regarded, as the subject is one that is now awakening considerable interest among those engaged in the study of the statistics of deaths. A matter of much importance to this class of inquirers would be to ascertain the proportion which exists in this Commonwealth between the deaths of children of American and foreign parents. There is good reason to believe that the early mortality of the offspring of our own countrymen is much exceeded by that of the children of immigrant parents, especially in the large cities and thickly inhabited towns. There is very little doubt that the duration of life in this community is less with the Celt than with those of Teutonic blood, and that everywhere the expectation of life is lower among the needy and destitute, with those poorly clad, scantily fed, and living hived together in underground and unventilated apartments, than it is with the prosperous and well-conditioned."—*Eleventh Report*, p. 112.

In the report for 1852, Dr. Shurtleff gives a very valuable analysis of several diseases—cholera infantum, dysentery, erysipelas, typhus fever, scarlatina, scrofula, consumption, and teething—showing, in regard to each, the annual total mortality in the years 1849, 1850, 1851, and 1852, and the ratio of the whole to the population of each county, the number and proportion dying at each age, and the males and females dying in each month of each year.

These are very important tables, and we hope they will be continued and extended to all other diseases in the future reports. The ratio of mortality from these causes to the population in several counties must be taken with some limitation, inasmuch as several towns made no report, and yet their population is included in the calculation. In so valuable a document as this, which ought to be worthy of all and unhesitating credence, this should have been regarded; and either the mortality from these diseases in the towns of Barnstable, Holyoke, &c., which did not make reports, should have been calculated from the analogy of their respective neighbourhoods or counties, or their population should have been omitted. In either way, the rate of mortality would have been raised and brought nearer the facts. In the county of Barnstable, the ratio of deaths from dysentery would be one in 128, not one in 166 of the people, as here stated. This is the most marked error, as a larger proportion of this county than of any other failed to make the required returns.

Our author reasonably complains of the want of uniformity in the diagnosis

and nomenclature of diseases in the returns which came from the town and city clerks. Some names seem to be more readily used in some counties than in others. Thus cholera infantum, nominally, at least, was much more prevalent in Middlesex, where one in 473 perished from it, than in Berkshire, where it destroyed less than one in over 4,000. Probably some cases which are called by this name in one place are called diarrhœa or bowel complaint, or other name, in another place. Thus, Middlesex, which has so much higher proportion of cholera morbus, has a somewhat lower proportion of other diseases of the digestive organs than Berkshire and Franklin. There seems to have been a similar uncertainty in the use of the term consumption, as 1,386, or 9 per cent. of the whole who perished from it in the four years, were under five years of age. It is hardly probable that their disease was tuberculous phthisis.

The *Reports from Connecticut* are more meagre than those of Massachusetts. We have four of these documents for the years ending with July, 1848, 1849, 1850, and 1851.

These four reports are prepared by, or under the charge of, three different Secretaries of State, and there is no evidence that they were submitted to a competent physician or statistician for preparation or correction. They are made up from the returns as they come from the several districts and towns.

These returns to the secretary were very imperfect. He complained, in the first report, that the law did not allow sufficient time to prepare and send the blanks from the seat of Government to the school committees before some of them had made their customary enumeration of the children, and "they were unwilling to undertake the labour of canvassing their districts a second time, and either made partial and imperfect reports, or wholly neglected the required returns." "Under such circumstances, full and accurate returns were hardly to be looked for." "From thirteen towns no returns were received, and in several others only a small proportion of the school districts made the required reports."

The sum of these reports is shown in the following table, by which we see that there was less progress of improvement in these four years than was reasonably expected:—

Years.	Births.	Marriages.	Deaths.
1847-8	6,850	2,816	4,379
1848-9	7,238	2,920	5,000
1849-50	7,578	2,884	5,118
1850-51	8,360	2,995	4,712

The first report was hastily made from facts hastily gathered, as the law went into operation on the 27th of June, and required the records to be made covering the whole year, ending on the 7th of August next, and then to be returned early to the Secretary of State. The secretary comprised the facts which he received in three tables.

Table I. shows, in regard to each town and county, the population according to the preceding census; the births and deaths, male and female; the marriages in three classes: 1st, both parties resident of the State; 2d, but one resident; 3d, both non-residents; a recapitulation in regard to each county and the State.

Table II. shows, in regard to each county and the State, the mortality, male and female, in each month.

Table III. shows, in regard to each county, the deaths of males and females, at their several ages.

A new secretary does the work of the second report. In this there is an increase of the returns of all the facts, yet the secretary complains of the remissness of the local officers in making the proper inquiries and sending their results to him.

The facts that were obtained are published in four tables.

Tables I. and III. are similar to those in the first report.

Table II. shows the number of births, male and female, in each county and in the several months.

Table IV. exhibits the number of deaths in each county, from the several causes, alphabetically arranged.

In the third report, the third secretary echoes the complaint of his predecessors: "While the returns, on the whole, are somewhat more full than those of the two preceding years, they are yet far from complete, and, in many of the particulars which they were designed to embrace, extremely defective." "From two towns no returns were received;" yet, he adds, "In our own State there are indications of awakening in this subject, and of increasing convictions of the important benefits to be derived from its prosecution."

The facts received are condensed into five tables.

Tables I., II., III. and IV. are similar to those in the previous report, except that the diseases are classified.

Table V. shows the mortality from each class of causes, and also, the proportion in each class to the whole from all specified causes, in each of the last three registered years.

The fourth report is prepared by the same secretary that prepared its predecessor. He still complains of the neglect of the local committees in making returns. He refers to the delinquency of some of these officers in sixty-two towns, and the total omission in two others. Nevertheless there was in the returns an increase of births and marriages, and a decrease of deaths.

He publishes his facts in five tables, all of which are similar to those in the third report.

These facts in all these reports are valuable and reliable as far as they go, but they are incomplete, and allow no ground to calculate the rate of births, marriages, or mortality to the population of the State. They are published almost without note or comment, naked statements for the statistician and the political philosopher to clothe and use for such purposes as they may answer. The nomenclature and classification are those proposed by the National Medical Association. From this we learn, at least, the relative prevalence of each disease, or class of diseases, in Connecticut, although we cannot determine the absolute amount of its influence.

It was an unfortunate direction of the law that required the school committees to gather these facts in Connecticut. The same was tried without success by Massachusetts during the first few years of this law, but a charge from the school committee to the town clerk or register produced a great improvement in the returns. The school committees are elected, not so much for their general business habits as for their interest in, and acquaintance with, education. They are willing to attend faithfully to the main object of their office, but go unwillingly beyond it, and, as we see in both Connecticut and Massachusetts, they have often come short of this unacceptable requirement. We hope Connecticut will take counsel of her sister State, and thus improve her returns and reports of mortality.

The law of *Registration in New Jersey* was enacted in 1848. It required the ministers, justices, and all others authorized to join in marriage, to report

such acts, and sextons to report all burials to the township clerks, and the clerks themselves to make inquiry as to births, record these facts with the several circumstances specified in the law, and send the whole record to the Secretary of State. In 1851, a supplement to the law increased the compensation to the clerks and the secretary for the work required of them.

We have not been able to obtain the earliest report, but we have the last three, covering the years 1851, 1852, and 1853.

In the report to the legislature, the secretary, who is the same through these three years, complains of the remissness of the local officers, and urges the legislature to clothe him with some authority to compel their obedience to the law. He says, in the report, printed in 1852: "General complaint is made by the township clerks that ministers, magistrates, sextons, and physicians do not make report to them as directed by the law. Physicians are especially, and almost universally, spoken of as remiss in this particular. A penalty for such neglect is affixed to the three former classes. If a penalty is proper in any case, *physicians ought to be included.*"

In the following report, printed in 1853 and 1854, the secretary repeats his complaint and recommendation of more stringency in the law.

Out of one hundred and eighty-four townships in the State, ninety-three sent returns for 1850, one hundred and fifty-six for 1851, one hundred and fifty-seven for 1852, and one hundred and forty-one for 1853.

The facts returned to the secretary are condensed in the following table:—

Years.	Births.	Marriages.	Deaths.
1851	11,861	2,859	5,089
1852	10,683	2,682	6,371
1853	12,107	3,388	5,651
	34,651	8,929	17,111

These are stated in seven similar tables in all the reports; showing, in respect to each town and county, and the whole State, the number of deaths from the several causes.

The number of deaths in several decennial periods of life.

The number of male and female children born, and the general occupation of the father.

The number of marriages.

These facts are published without note or comment, leaving it for the statistician to make what deductions he may from them.

The nosology is a limited one; it includes only twenty-one diseases in the reports for 1852 and 1853, and twenty-three in that for 1851.

We are not to suppose that these were the only fatal diseases in New Jersey during these three years; for there is another large and ominous class, one very convenient to the reporters, but very inconvenient to the analyzers, under the name of "all others." There were 17,111 deaths reported, and of these the causes of 14,323 were given; and of those whose diseases were returned, 3,069, or more than 21 per cent. were placed by those who prepared the report in this class of "all others."

We have read and examined the *Report from Kentucky* with great satisfaction. This is the first report which that State has made. They have not there minute local divisions into small towns and municipalities, with the local officers, select-men, town-clerks, committee-men everywhere; they have only counties for divisions, and only the county courts or assessors to make the inquiry. It is, therefore, a matter of congratulation that their first report

is so full, so nearly complete, and so instructive. Great credit is due to the officers who, throughout that great State, gathered all these facts, and especially to Dr. W. L. Sutton, of Georgetown, Ky., who prepared the report, arranged his facts in well-digested tables, and sent them forth with the wise observations that followed them. We commend this example of Kentucky to other States that are doubting whether they shall begin or pursue this all-important inquiry into their vital and mortuary condition.

This report of Kentucky fills one hundred and twelve pages. There were received from the assessors in the counties and published in this report the record of—

Births, 25,906; marriages, 7,430; deaths, 13,048.

The facts are put forth in ten tables; showing, in respect to each of the one hundred and one counties, the population, white and coloured, separately; the births of each sex, white and coloured, separately; the deaths of each sex and colour, and proportion of births and deaths, white and black, to the whole population of their respective colours; the average age of the whites and the coloured at death, and the number of marriages. The last are given in two columns; one furnished by the county clerks, which shows the number of licenses issued, and the other gathered by the assessors.

Table II. shows the number of each sex born in the several counties during each month.

Tables III. and IV. show the same facts in respect to the plurality cases and the stillborn.

Table V. shows the marriages in the several counties in each month.

Table VI. shows the ages of the marrying parties in the cases separately; 1st, when it was the first marriage of both; 2d, when it was the first of the female and a later one of the male; 3d, when it was the first of the male and a later one of the female; and 4th, when it was a later marriage of both.

The numbers and sexes of those who died in the respective counties are shown, in respect to each month, in Table VII., and, in respect to each age, in Table VIII.

Table IX. is a list of the causes of death, alphabetically arranged. This nosology is, with few exceptions, the same as that proposed by the American Medical Association and adopted by Massachusetts.

Against each disease is shown the number of deaths, of whites and of blacks, of each sex; the number who died in each month, and of those who died at each period of life.

Table X., A shows the number who died from each of the twelve classes of causes of disease in the several counties. B shows the number who died in the State and in each county from each specified cause, and also, the proportion which the mortality from each disease bore to the whole throughout the State.

Here is, indeed, a very comprehensive list of tables and of facts, and a work of great value to those who wish to study the law and progress of population and the sanitary condition of Kentucky, and who desire to compare its diseases, and some of the circumstances of its mortality, with those of other States. Kentucky, although it has no great variety of latitude, yet it is widely extended from east to west, and has a great variety of soil and geological structure. The mountain regions of the east are probably subject to very different endemic influences from those of the alluvial, and, in some places, low districts of the west. The counties are small, and enable the student to discriminate very nearly the various localities. The diseases of each county are

specified; and thus the connection between these localities and diseases and the effect of local influences can be traced, and, in some measure, determined.

These returns, although so full and gratifying, are not complete. One county, with 7,393 inhabitants, returned no deaths. Ten other counties, with 5,095 coloured inhabitants, reported no deaths of that race. All the other counties show some mortality both of the whites and of the blacks, yet some of these are evidently imperfect. Jefferson County, which in 1850 contained near 60,000 people, and included the city of Louisville, with a population of 50,000, returned only 390 births and 205 deaths of all colours. There are only about one-fifth of the number that is stated by the marshal, who took the census in 1850, as having been born or died in that county in course of the preceding year. Two years and a half had elapsed, and the population considerably increased and undoubtedly added to the number of births and deaths.

The manifest incompleteness of these returns in many of the counties allows of no calculation of the rate of mortality, or of births or marriages in them, or in the whole State. It might have been very properly made in regard to those counties whose returns bore evidence to Dr. Sutton, from his knowledge of their circumstances, that they contained all these facts that had happened within them. These would have been satisfactory; the others are at least doubtful, and some of them plainly erroneous. We were, therefore, sorry to see the calculation made and the result stated, in the first table of this report, of the rates of births and of deaths of each colour, to the white and the black population in each of the counties, which reported even only one of these events.

The ratio of marriages to the population is more reliable; for those are generally taken from the registers of the county clerks, whose license is necessary for the legality of this act. But the enumeration of the births and deaths had no such ground of certainty. They were gathered as the assessors had power or will to obtain them, or as the people of whom they inquired were able or willing to give the information. There is a great difference in the facts reported, and consequently a corresponding difference in the rates deduced, in the separate counties. Among the whites there is stated to have been one death in 687 in Muhlenburg, one in 423 in Floyd, and all intermediate rates up to one in 321 in Hickman. Among the blacks, this rate is one in 1,152 in Lincoln, one in 605 in Adair, up to one in 17 in Marshall. In thirty-two counties the mortality was less than one per cent., and in six counties less than one-half per cent., among the whites; and in twenty counties less than one per cent., and in seven counties less than one-half per cent., among the blacks. The average rate of mortality was one in 78.28 whites, and one in 65.07 blacks. Now, no one acquainted with the law of mortality will put any confidence in these deductions.

The statements of the births bear more internal evidence of completeness, yet they do not seem to justify the calculations which are here given of the rates of the fecundity of the different colours in the several counties. It is not to be supposed that there should be such a wide difference as one birth in 22 people in Butler, Hancock, Laurel, and Marshall, and one in 188 in Jefferson, and one in 317 in Muhlenburg among the whites; and one in 13.1 in Marshall, one in 21.5 in Warren, one in 22 in Shelby, and one in 259 in Muhlenburg among the blacks; or that in Caldwell there should have been one birth in 36 whites and one in 157 blacks; in Greenup, one in 45 whites and one in 164 blacks; in Perry, one in 21 whites and one in 120 blacks. Between these extremes there are all intermediate grades, owing to the various and defective returns.

This defect is what should have been expected. The same has been the

experience in all other States in their first attempts to gather these facts. Indeed, in Kentucky it seems to have been less than many other States. These returns seem to come much nearer the actual facts than any of the four reports of Connecticut, the last three of New Jersey, the two of New York, or the first of Massachusetts and England. Great credit is due to the assessors of Kentucky, who wrought so well in a field entirely new, and to Dr. Sutton for the faithful and successful labours he has performed; and we hope he will be again and again intrusted with the same work until it shall be brought to perfection.

Having given a general account of those reports from these four States, we would now compare their facts and the condition they represent, so far as they have principles or facts in common.

The relative prevalence of the various causes of death and the degree of mortality, is a matter of the first interest. Unfortunately for this comparison, the reports of Massachusetts alone make sufficient approach to completeness to allow a calculation of these rates to the numbers of the people. The nosology of New Jersey is so meagre that we have no great confidence that the numbers stated to have died from the several classes of disease contain the whole, for we know not how many fell into that receptacle of small numbers, "all others." Nevertheless, with this caution to our readers, to make all due allowance for what may have fallen therein in New Jersey, we give the following table of the mortality, and the comparison of the relative prevalence of the various classes of disease, and of several of the most important specific causes of death in the four States now under consideration:—

So far as this limited observation justifies any inferences as to the relative prevalence of the various causes of death, it appears that it arises from the diseases of the endemic, epidemic, and contagious class, in Massachusetts, in a proportion a little more than half as large; and in New Jersey less than half as large as in Kentucky. The proportion of diseases of the nervous system is, in Massachusetts, greater; and in Connecticut, sixty-five per cent. greater than in Kentucky. There is an observable difference in the relative fatality of diseases of the lungs. The proportion of consumption is nearly equal in all the three eastern States, and more than twice as great in these as in the western State. But the proportion of every other pulmonary disease, except of pleurisy, which was equal, was higher in Kentucky than in Massachusetts. Pneumonia, scarlatina, and croup, show a much higher ratio in the former than in the latter State. A similar difference is manifested in regard to asthma, bronchitis, hydrothorax, laryngitis, quinsy, and the unspecified diseases of these organs. Deducting consumption, the result shows that of all other diseases of the respirative organs, 1,585 died in Kentucky, and 915 in Massachusetts, out of 10,000 deaths from all reported causes; and the whole burden of pulmonary diseases was relatively not so much greater in the eastern than in the western State, as the burden of the diseases of the digestive organs was less.

There was a marked excess of ratio of dysentery, cholera, and fevers in Kentucky, over those in the other States. The ratio of deaths from old age was greater in Massachusetts and Connecticut than in Kentucky; but there was a larger class in which these deaths should take place. According to the census of 1850, the proportion of persons over 60 years old to the 10,000 of all ages was, in Massachusetts, 610; Connecticut, 773; and Kentucky, 357. If the ratio of mortality was the same as this in all, the deaths from old age would be in the ratio of 170, 216, and 100 in these States respectively;

whereas, they are in the ratio of 260, 305, and 100, indicating the probability, at least, that the warmer region is less unfavourable to the advanced period of life than the colder.

The deaths by violence include accidents of all sorts, from railroads, machinery, steam, drowning, &c. as well as personal assaults and murders.

From the tables of mortality in the several months, in the reports from Kentucky, Connecticut, and Massachusetts, we are enabled to form some estimate of the comparative fatality of the several seasons in those States. Our calculation includes one year in the first, and four years in the latter two. The order of mortality, in the several months, differs very materially among them. Beginning with the least fatal, it was, in Kentucky: February, January, May, April, March, June, November, December, October, July, September, August. In Connecticut: December, November, October, September, January, August, February, June, April, May, March, July. In Massachusetts: June, November, February, May, January, April, December, July, March, October, August, September.

The warm season, in all of these States, was more fatal than the cold; there was the greatest difference in the months in Kentucky. Making corrections for the difference of length, there were, in that State, 150 per cent. more of deaths in August; and in Massachusetts 61 per cent. more in September than in February; and in Connecticut 55 per cent. more in July than in December.

Comparative Mortality of the different Seasons. Ratio of 10,000 Deaths through the Year.

	KENTUCKY. One year, 1852.	CONNECTICUT. Four years, 1848 to 1851.	MASSACHUSETTS. Four years, 1849 to 1852.
Fourth quarter, Oct., Nov., Dec. . . .	2,488	2,170	2,340
First quarter, Jan., Feb., March . . .	1,760	2,589	2,230
Second quarter, April, May, June . . .	1,858	2,658	2,105
Third quarter, July, Aug., Sept. . . .	3,823	2,580	3,324
Cold season, fourth and first quarters	4,269	4,760	4,560
Warm season, second and third q'rs.	5,750	5,239	5,529
Winter, Dec., Jan., Feb.	1,882	2,333	2,177
Summer, June, July, Aug.	4,284	2,704	2,701

The mortality at different periods of life is shown in these reports. It is not important here to state this at all the shorter quinquennial or decennial periods, but we are content with showing this at the great epochs, as in infancy, under 5; in the forming and developing period, 5 to 20; the active and responsible period, between 20 and 60, when man assumes the burdens and bears almost the whole responsibilities of life; old age, 60 to 80; and extreme old age, 80. As the whole value of this statement rests upon the ratio of the mortality in each epoch to the number living in that epoch, in the State observed, we have calculated the ratio per 10,000, living at each of those periods in these States.

Comparative Mortality at various Ages in different States. Ratio of Population and Deaths, at each period, per 10,000 of all Ages.

AGES.	MASSACHUSETTS.		CONNECTICUT.		NEW JERSEY.		KENTUCKY.	
	Ratio of population, 1850.	Ratio of mortality, 4 years.	Ratio of population, 1850.	Ratio of mortality, 4 years.	Ratio of population, 1850.	Ratio of mortality, 3 years.	Ratio of population, 1850.	Ratio of mortality, 1 year.
Under 5	1,238	3,601	1,367	2,368	1,327	4,246	1,293	4,030
5 to 20	2,970	1,108	2,092	1,619	3,390	1,212	2,943	2,057
20 to 60	5,181	3,299	5,765	3,409	4,794	2,874	5,406	2,941
60 to 80	546	1,366	684	1,740	439	1,207	321	731
Over 80	64	682	89	856	50	477	36	250

If the returns included the entire mortality of these States, it would be easy at once to determine the ratio of deaths to the living at every epoch in each, but the returns are incomplete in all. Supposing, then, that those which we have represent the ages of the others which we have not, we assume that the whole mortality of each State was distributed through the several epochs of life, as these are. We can then see, whether this distribution of deaths, through the several ages, bears the same relation to the distribution of the living in all of those States.

In this way we see that the burthen of mortality in infancy was the least in Connecticut, greater in Massachusetts, and almost twice as great in New Jersey and Kentucky. In youth, this burthen is nearly equal in Massachusetts and New Jersey, and greater in these than in Connecticut and Kentucky, in which it is nearly equal. In the period of active manhood, the burden rests more lightly on New Jersey and Kentucky than in the other States, in which it is not very unlike.

We have not much confidence in the value of deductions drawn from the average age at death, except when it is taken from large numbers and in connection with the ages of the living. In a growing population, the families are younger and children more abundant than in a stationary community, which has a larger proportion of persons in the advanced periods of life. The children and youth have the same liability to death in both; and, as there are more of the young and fewer of the old to die in the former, its bills of mortality must show a lower average longevity than the other. For this reason, most American cities, which are growing rapidly and have a younger population, present a lower average age than the rural districts whose living population is stationary and older.

The calculation of the average longevity increases in value with the number of persons who are included in the observation; and it is worthless if it includes only a small number. Accident may make a great difference in this average and the apparent expectation of life in the district, or the class from which the calculations are made.

The Kentucky report states that the average age at death of the blacks in Morgan County was *one day*, and in Lincoln thirteen days. On looking to other columns we find that in each of these counties there was only one death. If in next year the child's grandmother should die, the average may be raised from one day to eighty years; showing a great apparent increase of healthfulness and expectation of life in the district. In Clinton, the average of the blacks was forty-five years, and based upon the same extent of observation—a single death.

Comparative Mortality from different Causes. Ratio per 10,000 from all specified causes in each State.

	MASSACHUSETTS. Eleven years and eight mos., May, 1841, to Dec. 1852.	CONNECTICUT. Three years, July, 1848, to July, 1851.	NEW JERSEY. Three years, 1851-52 and 1853.	KENTUCKY. One year, 1850.
Specified causes	138,550	12,827	15,638	10,411
Ratio	10,000	10,000	10,000	10,000
<i>All specified causes.</i>				
Zymotic diseases	2,937	3,232	2,506	5,643
Sporadic diseases				
Uncertain seat	1,217	763		578
Nervous organs	969	1,106	885	669
Respiratory organs	2,794	2,807	2,406	1,727
Circulative organs	208	152		68
Digestive organs	655	408	223	439
Urinary organs	44	70		35
Generative organs	114	149		125
Locomotive organs	55	61		55
Integumentary organs	9	17		15
Old age	612	717	577	235
Violence	386	513		420
All diseases of lungs	3,144	3,132	2,781	2,505
All diseases of digestion	1,858	2,024	1,322	3,208
Consumption	2,229	2,132	2,023	920
Pneumonia	441	520	282	579
Fevers	717	750	259	1,547
Dysentery	787	1,253	1,031	1,839
Scarlatina	411	255	462	104
Measles	104	82	110	318
Cholera and cholera infantum	316	271	65	824
Croup	217	210	258	443

In the last Massachusetts report a similar variance is shown in the average age of persons dying in some of the occupations. The average longevity of railroad agents and conductors in the western counties was twenty-four years, and in the eastern counties forty-six years. Artists in one district died at the age of fifty-one, and in another at twenty-seven. Coppersmiths lived in one part of the State to their fiftieth year, and in the other only to their twenty-third.

No possible good can be derived from such statements as these. No inference can be drawn as to the healthfulness of the places or occupations, or the value of life among those who reside or are engaged in them. On the contrary, they may mislead those who look only to these columns, and not to the facts on which they are founded, or who are not used to this kind of investigation; for they may be thereby induced to determine their location or employment.

The average age of death is calculated in Massachusetts for the last four reported years, and stated as to each town and county and as to the whole State. We again express our regret that this calculation was not extended to the whole experience of eleven years and eight months. The whole facts have been reported and are in the office of the secretary, and they would have given a very greatly increased value of these averages. And when so much might have been done, and so much added to the truths of vital science, it is

a matter of pain and wonder that the opportunity was neglected, and a comparatively inferior report sent forth to the world.

This calculation includes 72,715 deaths in Massachusetts. In the State, and perhaps in the counties, it gives a fair average of the duration of life; but in many of the towns, probably, a wider and larger observation would vary the result. The longevity is the highest in the small agricultural towns, which, beside being the most favourable to health, have a comparatively stationary population generally engaged in agriculture, the most healthful employment.

The excess of their youth go elsewhere to find other fields of occupation, and none come in. Some of them present a high average longevity. In Chilmark, with 36 deaths, it was 57 years; in Burlington, with 32 deaths, it was 52 years; in Medfield, with 73 deaths, it was 51 years; in Dalton, with 39 deaths, 50 years. In fourteen towns the averages were over 45 years, and in fifty-four towns over 40 years. On the contrary, there were ninety-four towns whose average was under 30; thirty-three under 25; and five under 20. All of the last, and most of the last but one of these classes, were places of recent and sudden growth and seats of manufactures.

The averages of the fourteen counties were, respectively, 20, 25, 26, 27; three counties 29, 31, 32, 33, 34, 35 and 39 years and a fraction. The lowest was Suffolk, whose population is rapidly increasing, with a large proportion of young Irish families among them. The next was Middlesex, which has three growing cities and many large manufacturing establishments. The average longevity of the whole State was 27.02 years.

The average in Kentucky was, among the whites in four counties, over 30 years; in twelve counties, 25 to 30 years; in thirty-four counties, 20 to 25 years; in forty-six counties, 10 to 20 years; and in three counties, under 10. Two were not reported.

Among the blacks in one county was one death at 45, and, in another, 146 deaths at the average of 35. In the other counties the averages were in six over 25 years; in twenty, 20 to 25 years; in fifty-two, 10 to 20 years; and in eight, under 10 years. Thirteen were not reported. In the whole State, 9,631 whites died at an average age of 20.55 years, and 3,367 blacks at an average of 17.59 years.

We hope Dr. Sutton, in the coming reports, will add the experience of future years to that of 1852; and, probably, as the State grows older and more cultivated, the average longevity will be very materially increased.

The average age is not stated in the reports from New Jersey and Connecticut, nor do we think it now worth our while to calculate them.

Occupation.—The reports from Massachusetts have, from the beginning, given tables of the longevity of those who were engaged in the several occupations, and died at an age of 20 years and over. We have already, in our *Journal* for July, 1852, spoken of the defects of this table, of the minute division, and of the want of concentration of the whole sum of the observations in the State, and of the bad classification into groups of occupations which are dissimilar in character and sanitary influence. Here we have occasion again to utter our complaint. We do it with sorrow; for, where there is so much that is satisfactory, it is not agreeable to find fault; but, in the report for 1851, the former defects are repeated, and, in that for 1852, the same are continued and another is added.

In the tenth report, the number, sum of ages, and average age at death of those who belonged to the several occupations, are given—not for the whole ten years and eight months—the record of which, in regard to the males, had been received; but for seven years and eight months, ending with 1850 in one class, and for the year 1851 in another, and these are not added together.

In the eleventh report, the statements, including the deaths in the whole State during the eight years and eight months, ending with 1851, are in one column, and those who died in the year 1852 are in two other separate columns; one covering the five western counties, and the other including the nine eastern counties. We cannot understand why this last division was made. We know of no difference in the circumstances of life and the sanitary exposures of a blacksmith or shoemaker in Middlesex or Essex, and a blacksmith or shoemaker in Worcester or Berkshire.

This division of occupations is not only unscientific, but needless; for, in many of these, the numbers of deaths were already so small as to render their average of no value. In these cases, the accident of a few of the young or old dying in different places determine very wide and singular differences in the statements of the value of life among them. Thus, the average longevity of the bakers was, in the eastern part of the State, 42.82 years, and, in the west, only 26. In the east, the paper-makers lived 39.23 years, and, in the west, only 21.

The average is stated in regard to each occupation, which is well. But, inasmuch as many of them present but few deaths, and of course a very variable and unreliable average of longevity, they need to be condensed into classes, according to the affinities of their circumstances or their effect on health.

Our authors make eight classes of the males and one of the females. These are: 1, agriculturists; 2, labourers; 3, mechanics; 4, merchants; 5, paupers; 6, professional men; 7, public men; 8, seamen; 9, females.

The names of all these classes, except the seventh, probably explain them. But this class of public men is one of the anomalies of science. We have in vain sought to find some law of affinity, some vein or bond of common character or circumstance running through or surrounding them, which should unite them together and separate them from others. We give their names: Baggage-masters, bank-officers, brakemen, brokers, chimney-sweepers, comedians, cooks, drivers, drovers, engineers and firemen, express-men, fencing-masters, ferrymen, gentlemen, innkeepers, judges and justices, lighthouse-keepers, news-carriers, pilots, public officers, railroad agents and conductors, sextons, sheriffs and constables, soldiers, stablers, teamsters, ticket-masters, victuallers, watchmen, weighers and gaugers. Perhaps they are all, in some sense or other, public men; that is, they either appear abroad before the public, or the public has the privilege of coming to them in the way of their employment. In this respect, the broker and the merchant, the pedler and the teamster, the ferryman and the labourer, and, indeed, most of the others in this and some in the other classes are in the same category. Yet there are some whose occupation neither call them to the public nor invite the public to them; such as cooks and chimney-sweepers. Even admitting that these had no affinities with, and must be separated from others, yet there is, at least, an unnatural juxtaposition of occupations that have nothing but mere humanity in common, and whose exposures and influences are as dissimilar as possible. What affinity there is between the judge, whose labour is all of the brain, and the chimney-sweeper, whose work is all of the muscle; between the gentleman, who is usually a capitalist, without special occupation, and the stabler, whose occupation is among horses, harness, and filth; between the cook, who lives over the fire, and always hot, and the pilot and teamster, whose life is necessarily abroad, and often in the cold and storm; between the broker, who lives by calculation, amid the anxieties and fluctuations of the money-market, and who has a life of physical ease, and generally of inaction and great comfort, and the watchman, whose labour is at night, and who generally adds to this as

much work in the day as he can endure; what similarity there is between these and other occupations of this class, in the sanitary influences of their employments, or of the circumstances which surround them, in the exposures to which they submit, in the power of self-protection, in the comforts they may enjoy, in the effect of their various kinds of cultivation, habits, and position upon human health and constitution; these matters we are unable to understand, and respectfully ask the authors of these otherwise valuable documents, to explain them in their future reports.

The paupers are included in a single and separate profession, and form a class by themselves, and they have a very high longevity, the highest of the whole. Truly pauperism seems to be a very healthy occupation; and the poor appear to have at least the blessing of length of life, as a compensation for their worldly privations. This would be a very pleasant consolation to these people in their sufferings if it were true. On the contrary, the universal testimony is, that the longevity of the poor is less, and generally much less than that of those whose circumstances supply them with the comforts of life. This last report states that the average longevity of paupers was 66, 70, and 71 years in the different years or different parts of the State. When put in comparison with the longevity of the other classes, this statement gives very wrong notions of the comparative worth of life in the various conditions or employments.

A large portion of the paupers are not old because they belong to that class, but they belong to this class because they are old. They sustain themselves through the active periods of life by their own exertions; and when, with advancing years their strength declines, they fall upon the public for support, and of course swell, in a very disproportionate degree, the average longevity of the class to which they belong at death. For many of these, pauperism is rather a condition than an occupation, and it might as well be designated old age as pauperism. A much more appropriate arrangement is to classify the paupers with the labourers.

Yet they have a questionable title to that class, inasmuch as so many of them belonged to other occupations, and were mechanics of every sort—farmers, seamen, and even merchants, &c., as long as their strength allowed them to attend to business or work. Strictly speaking, the paupers should be analyzed, and distributed among the various other classes from which they came to the poor-house; then we might see what employment exhausted them earlier or later, and deprived them of the power of self-sustenance, and what carried them on to old age when they could no longer work. For want of the means of making this analysis and distribution, we have omitted them in our classification.

The proper classification is not according to names, but according to sanitary influence. In a former article on this subject, we spoke at length of this, and gave an arrangement according to a law which seemed then to be suited to the nature and effect of the several employments on the health of those engaged in them. Time and further study have not changed our opinion in this matter; and we now repeat it, in order to explain the table of longevity in the occupations and classes of occupations.

Until we shall have very many more facts than we now possess in respect to mortality, we would condense all the occupations into ten classes:—

I. Farmers, gardeners, and all who cultivate the earth. They have constant, and sometimes laborious, exercise in the open air. But they have the means of living comfortably, and are free from anxiety.

II. Labourers, servants, brakemen, chimney-sweepers, coachmen, stagemen, hostlers, well-diggers, and workmen in powder mills. These men are gene-

rally subject to hard labour and much exposure. They live abroad. They are often deprived of some or many of the comforts of life.

III. Active mechanics employed in the open air. These are boat-builders, brickmakers, carpenters, caulkers, masons, millwrights, riggers, sawyers, ship-carpenters, stonecutters, and tanners.

IV. Those who are otherwise occupied actively abroad, but not in the cultivation of the earth: these are army-officers, baggage-masters, butchers, drovers, engineers and firemen, expressmen, ferrymen, lighthouse-keepers, news-carriers, pedlars, pilots, sextons, sheriffs and constables, soldiers, stablers, stevedores, surveyors, teamsters, watchmen, weighers, and gaugers.

V. Active mechanics, &c., working within doors. These are but little exposed to the elements; they work in warm rooms, and exercise constantly and sometimes severely: axe-grinders, bakers, basket-makers, blacksmiths, bleachers, bookbinders, brush-makers, cabinet-makers, calico-printers, card-makers, carriage-makers, chair-makers, chaise-makers, churn-makers, cloth-makers, clothiers, comb-makers, confectioners, cooks, coopers, coppersmiths, curriers, cutlers, distillers, dyers, file-cutters, founders, furnacemen, glass-blowers, glass-cutters, gunsmiths, harness-makers, hatters, leather-dressers, machinists, match-makers, morocco-dressers, nail-makers, painters, paper-makers, pianoforte-makers, pill-makers, potters, pump and block-makers, reed-makers, rope-makers, sail-makers, sash-makers, screw-cutters, soap-boilers, tallow-chandlers, tinsmiths, tobaccoconists, trunk-makers, turners, wagon-makers, weavers, wheelwrights, upholsterers, victuallers, and wool-sorters.

VI. Sedentary and quiet mechanics, &c., who are occupied, but without much exercise, in shops, &c., and protected from the elements. These are artists, barbers, cigar-makers, comedians, dentists, engravers, goldsmiths, jewellers, musicians, operatives, portrait-painters, printers, saddlers, shoemakers, shoe-cutters, silversmiths, tailors, watchmakers, whip-makers.

VII. Seamen and others, employed on the ocean and on the lakes, exposed to the severities of the weather, without protection, and deprived of many of the comforts which are usually enjoyed on the land. These are sailors, fishermen, marines, and naval officers.

VIII. Persons engaged in trade and finance; they are occupied in their houses, offices, or places of business. They have very little occasion for bodily exercise at home or abroad; but their brain is, in all, exercised to some extent; and, in fact, to a very great extent, with calculations, watching the variations of the market, and with the anxieties of business. These are merchants, traders, grocers, apothecaries, druggists, booksellers, stove-dealers, provision-dealers, clerks, manufacturers, speculators, bankers, bank-officers, brokers, capitalists, gentlemen, inn-keepers, postmasters, railroad agents and conductors, ticket-masters, and superintendents of factories.

IX. Persons whose occupations are almost exclusively of the brain, and who have but little or no occasion to labour with the body, in the discharge of their professional duties. These are clergymen, judges, justices, lawyers, physicians, professors, teachers, students, civil engineers, editors, and public officers.

X. Mechanics, whose occupations are not specified.

XI. Females.

There are many other things in these reports, to which we would gladly call the attention of the readers of the *Journal*; but here is all that we have space and time to present. We commend all of these documents to the public for the lessons of truth and wisdom they in various degrees teach; and we commend the example of those governments that have produced them, to all others who have not done like unto them.

E. J.

BIBLIOGRAPHICAL NOTICES.

- ART. X.—1. *Transactions of the Tennessee State Medical Society, at its Twenty-Fourth Annual Session, convened at Nashville May 4, 1853.* 8vo. pp. 79.
2. *The Transactions of the Iowa State Medical and Chirurgical Society, Third and Fourth Sessions, held at Fairfield May, 1852, and Davenport June, 1853.* 8vo. pp. 48.
3. *Transactions of the Medical Society of the State of New York, at its Annual Meeting, in the City of Albany, held February, 1854.* 8vo. pp. 148.
4. *The Minutes of the Fifth Annual Meeting of the Medical Society of the State of Georgia, held in the City of Macon April 12, 1854.* 8vo. pp. 55.
5. *The Transactions of the New Hampshire Medical Society, Sixty-fourth Anniversary, held at Concord June 6th and 7th, 1854.* 8vo. pp. 56.
6. *Proceedings of the American Pharmaceutical Association, at the Third Annual Meeting, held in Cincinnati July 25 and 26, 1854.* 8vo. pp. 40.

WE have always regarded the published *Transactions* of the several State Medical Societies as among the most interesting portions of our medical literature; and we have, consequently, felt it our duty, as journalists, to give to them as early a notice as possible. Our shortcomings, in this respect, are to be attributed, not to any neglect on our part, but to the number of works in the different departments of medical science constantly claiming our attention, as well as to the irregularity with which the volumes of *Transactions* reach us.

The value of these *Transactions* is due partly to the contributions to medical knowledge, and to the ethics of our profession, furnished through the several addresses, reports, and other papers of which they are composed; but mainly, we conceive, from the evidence they furnish of organized and concentrated action among the physicians of the United States to elevate the standing of the profession and assert its rights.

In perusing the annual volumes of *Transactions* issued by the leading State Medical Societies, amid, it is true, some chaff, much valuable grain is invariably to be found. Were the reports and papers, communicated at the stated sessions of these societies, to be confined strictly to the personal observations and experience of the physicians located in the different sections of each State, in reference to the diseases of their respective localities, these *Transactions* would do much towards elucidating the etiology, pathology, and therapeutics of those maladies the American physician is called upon to treat. As it is, the facts recorded on their pages, and through them made the common property of the profession, have already had no inconsiderable influence in furthering the progress of medical truth. As the record of the proceedings of congresses of physicians, having for their object the educational, ethical, and social progress of the profession, these *Transactions* have especial claims to our notice.

"This country," says Dr. Kneeland, in an address delivered before the Onondaga County Medical Society, and contained in the *Transactions of the New York State Medical Society* for 1854; "this country has, more fully than any other, and this age, more than all past ages, come to be characterized as the home and the era of voluntary association for mutual benefit."

"Association in social, scientific, and fraternal conferences, teaches us and makes us feel that we are brethren; that our interests are common stock, so far as professional honour and standing are concerned; and that any means which lowers in the public mind our brother's character for honour and skill, takes so much capital from the joint-stock concern, and diminishes our own dividend of respectability.

"In meeting with each other, away from the petty rivalries and anxieties of our private practice, our opinions and motives are more likely to be seen and judged with kindness and impartiality; and we are brought to feel, on these occasions, that we are neither compelled 'to tread the wine-press' of our professional trials 'alone,' nor are we, if honoured and successful, 'alone in our glory.' And also, that being the oracle of a small circle of adoring, but undisciplined minds, is not *true and undisputed greatness*. That making use of our noble calling, as a mere money-making trade, is a vile desecration of a time-honoured and truly philanthropic vocation. On these occasions, we are brought to realize that our manhood, and the noble profession of our choice, demand higher aims and nobler aspirations.

"Our opinions and sentiments are here subjected to the test of kindly criticism, and, whatever of discovery or improvement we may suggest, will alike honour the author and benefit his auditors. Our errors and mistakes will more likely be corrected and remedied than if never tested in the crucible of scientific discussion.

"Knowledge is a species of wealth which is increased by imparting, and if our associations are made what their founders designed, and what their self-sacrificing sustainers hope them to become, every medical meeting will add to the practical knowledge and skill of all who attend, and we shall leave behind us some mistake, or crude, unformed opinion, and in its place bear with us to our fields of labour some fact or thought of practical value. Nothing quickens intellectual vigour more than earnest collision of mind with mind."

"By meeting our compeers we break the tether of selfishness, and escape from our daily, weekly, yearly round of drudgery and toil, wherein we are likely to form a hard-beaten track of routine practice, bare of food for the mind's growth. We enlarge our range of vision and mental nutriment, by coming in contact with minds which, having been subjected to somewhat different educational influences, and having looked at the same truths from another stand-point than we occupy, can teach us, even by differing from us, this, if no more, that knowing a thing is much short of knowing all about it, with its influences *upon* and varied relations to all other knowledge.

"Mark those physicians who enjoy but the intercourse of unprofessional minds, without finding their level by frequent contact with professional compeers; they stand aloof from medical associations, fearing they shall not receive due honour, with all that the admiration of partial friends and flatterers can do to puff and dilate them into overgrown self-importance, together with all that the ignorant and credulous may feel or fancy of their power."

"No class of men would be more benefited by frequent seasons of professional intercourse, away from the sick-room, away from the daily scrambles for power, and wealth, and fame; and, did they not fear the danger of losing their greatness, by getting the attenuated balloon of self-conceit ruptured by too rough a collision, we might oftener induce them to favour us with their august presence.

"Another, and by no means the least benefit to individuals, furnished by medical association is, that quackery, in all its protean forms, may be more effectually discovered and exposed by concerted action, than by every physician attempting to 'fight the common enemy on his own hook.'"

"Association may effectually aid us in elevating the standard of *professional morality*. By enforcing our discipline, and requiring obedience to our code of medical ethics, we may drive those demi-quacks who only use their connexion with us as a screen for debasing arts, and appliances to win public favour, forth to companionship with new-system empirics and shameless charlatans. And since *character*, and not numbers merely, is the chief means of our influence for good, let us guard the avenues to membership in our associations, so that no man, whose want of moral worth and character would unfit him for our personal esteem, shall be received among us as a brother; and as associational is but the totality of individual character, let us maintain, each man for himself, a high standard of professional integrity and honour; not the *mere* observance of etiquette, and obedience to codes and forms, which keep the selfish time-server within the statute from dread of penalties, but that lofty and universally

applicable rule which teaches us to *do to others* what they would have a right to expect *from us* in a change of relative circumstances."

"Association entered into with elevated views of its utility and importance, would tend to bind us firmly together, and to make us true to each other. Having decided upon the worthiness of a physician, and received him as a brother, we should be careful of his reputation. Having opened our arms to receive him, let us 'grapple him to our hearts with hooks of steel;' and if envy, malice, or detraction assail him, make common cause in his defence."

The foregoing extracts from Dr. Kneeland's address, express with great force and truth, in plain, familiar language, some of the leading advantages the medical profession derives from organization and association. We would add another, which the doctor has not noticed—the attainment of all those reforms, educational and ethical, which the wise and good within our ranks have so long laboured to achieve. We feel persuaded, that by a unanimity of action, which can be brought about only through an organization of the profession throughout the United States, legitimate medicine, practised by a body of well-instructed and skilful physicians, may be again made to exert its proper influence, so as to command the respect and confidence of the community at large. Though the schools may withhold from us their aid in the furtherance of this important object, though there be many in our own ranks who will do everything in their power to oppose it, having private interests to subserve, adverse to those of the mass of the profession, still, we only want unity of action among those resolutely bent upon attaining the desired end, to insure ultimate success.

1. The *Transactions of the Tennessee State Medical Society* contain a very sensible address by the President, Dr. John M. Watson, on the retrospective, perspective, and prospective views of medicine. It is true, we cannot entirely agree with the author in his anticipation of the period "when we shall have a separate branch of medicine for every organ"—"a name and a doctor for every part of the system, whose care and duty it shall be to treat the diseases of one particular organ in an open, scientific way," as one in which "we may expect all that can be achieved by man" in the preventing and cure of disease, "aided, as he will be, by strong collateral lights on every hand." We are not perfectly convinced that, by dividing and subdividing the practice of medicine into specialties, each having for its object the maladies of a particular organ, our acquaintance with therapeutics and general pathology will be much improved. The mutual relationship and influence of the morbid conditions of the several portions of the system must always be taken into consideration, in order to arrive at correct views in respect to the different maladies to which the human frame is liable—their pathology, their prevention, and their treatment—and this cannot be done if the diseases of each organ are studied and treated apart from those of every other organ.

The Address of Dr. Watson is followed by a "Report on the Adulteration of Medicines, Chemicals, Drugs, &c." The subject of this report is one of vital importance to the physician. To secure the purity and genuineness of the drugs and chemicals imported from abroad, is a wise protective measure, but is not of itself sufficient to eradicate the evil against which it was aimed, so long as there is no provision for restraining the adulteration and sophistication of drugs and chemicals subsequent to their importation; a practice carried on, we fear, to a much greater extent than is generally suspected.

The report of Dr. Currey closes by offering a few suggestions as to the best mode of remedying this evil, so far at least as the medical faculty is concerned.

Dr. Currey would have every physician so instructed as to be a judge of the genuineness and purity of the medicines he administers; not to be merely able to arrive at a *prima facie* judgment of their character from an acquaintance with their physical aspect, but to determine positively that they are pure and genuine by chemical analysis. He would have no one permitted to engage in the compounding and dispensing of medicines who has not qualified himself for the task by a thorough pharmaceutical as well as practical training. Thirdly, Dr. C. remarks, that foreign adulterated and deteriorated medicines having been declared contraband by Congress, it devolves upon our State legislatures to

devise the means by which home adulteration and sophistication may be arrested. More, however, in this respect, he believes, is to be gained by popular opinion, and by pharmaceutical and chemical education, than by legal enactments and penalties.

The history of a case of cerebral disease, one of false aneurism, and one of Fallopian pregnancy, and a description of a set of instruments, by which their inventor, Dr. J. L. Park, thinks "some of the inconveniences attending the operation for the radical cure of fistula in ano, whether by the knife or ligature, may be obviated," complete the professional papers embraced in these *Transactions*.

2. The *Transactions of the Iowa State Medical and Chirurgical Society* open with the annual address of the President, Professor D. L. McGugin, in which he presents a graphic sketch of the toils and anxieties, the sacrifices and exposure, the duties and responsibilities incident to the life of the physician; commencing from the period of his novitiate as a student of medicine to that of his death; for few members of our profession can hope to acquire the means to enable them to retire in season from its "exacting, sacrificing, and crushing" cares, and spend their latter days in quietude and peace.

The address of Dr. McGugin does not present only "the toil and conflict" of the physician's life, he also depicts his hours of triumph and rejoicing, his "seasons of enjoyment, if not of repose;" "his moments of happiness," although "not always seasons of personal comfort;" his fond hopes and Elysian dreams, though "not founded in the prospect of wealth, fame, or glory, but simply in the consciousness of having discharged his duty." Were it not for these bright spots strewn along his path, and rendered still brighter by their contrast with those of deep gloom he has daily to encounter, the life of the physician would present a tissue of unmitigated darkness, his feelings would become misanthropic, or he would abandon a profession fraught with all the elements of discomfort and discouragement. The inherent benefits of our science, and the capacity of its professors for the accomplishment of good, constitute its pillars and its props, and at the same time a source of proud satisfaction and consolation to those who devote themselves to its service.

A short address on medical education, by Dr. J. B. Elbert, delivered at the opening of the fourth session of the Society, held June 8, 1853, follows that of Professor McGugin.

The first of the *Reports* is devoted to the diseases of Des Moines, Louisa, and Washington Counties. It is very concise, and presents no prominent points of general interest.

The second report is on surgery, by Dr. John F. Sandford. It embraces the history of a number of interesting and instructive cases.

The third report is on obstetrics and the diseases of females. It is drawn up by Dr. D. L. McGugin, and comprises the details of four cases; the first of a separation of the symphysis pubis occurring during a rapid natural labour, followed by inflammation of the bladder, uterus, ovaries, and peritoneum. The case was managed by an energetic antiphlogistic treatment; by drawing off, by the catheter, the urine from the bladder as it accumulated; by the application of a roller, to prevent any movement of the injured parts by the action of the muscles of the pelvis and lower extremities; and by opiates, to procure rest and relieve the harassing cough. This treatment was followed by the slow, but steady, recovery of the patient.

The second case is one of excessive leucorrhœa, resulting from a deep fissure in the posterior lip of the os uteri; cured by the local application of a strong solution of nitrate of silver, followed by the *al. tinct. iodine* to the tumid lips of the os uteri and around the cervix; with chalybeates internally, porter, and a generous diet. The leucorrhœa was soon arrested, and the patient restored to a reasonable condition of health.

The third case is one of fissure of the nipple, cured by the application of a strong solution of nitrate of silver.

The fourth case is one of *post-partum* uterine hemorrhage, treated successfully by pressure upon the abdominal aorta.

Two short papers, the one giving an account of prolonged vital contractility in the heart of a fish, and the other of the *tenia solium* of the catfish, complete the contents of the volume.

3. The *Transactions of the Medical Society of the State of New York* open with the annual address of the President, Dr. Jenks S. Sprague. The subject of the address is medical education. The examination of the question, What manner of men ought those charged with the high trusts of practitioners of the healing art to be, in order to command the confidence and respect of an intelligent community? A considerable portion of this very excellent address is devoted to a consideration of the absolute necessity of practical and morbid anatomy to the physician and surgeon, and the duty of legislative bodies to afford every facility to the medical teacher and practitioner for procuring the material necessary to the prosecution of these important branches of professional inquiry, so far as this can be done without violating the noblest and most sacred sympathies of the surviving relatives and friends of the dead.

This address is replete with good sense and a manly defence of the rights and interests of a properly-educated and well-organized medical profession. The presence of the members of the New York Legislature at the delivery of the address, gives to the several positions assumed by the author, and the manner in which they are enforced, additional point and interest.

A case of extra-uterine conception is reported by Dr. W. H. H. Parkhurst, of Herkimer County. We see no reason for supposing this to be a case of extra-uterine conception; it resembles a number upon record, where it was evident that the fetus, when near its full term, had escaped from the cavity of the uterus into that of the abdomen, through a rupture of the parietes of the former. The interest of these cases arises from the fact of the patient surviving the immediate effects of the accident, and the residence of the fetus, for so many years subsequently, in the peritoneal cavity, without the occurrence of extensive and fatal inflammation.

A very able paper, on a subject "of great practical interest to the whole medical profession," the treatment, namely, of penetrating wounds of the abdomen, with punctured wound of the intestines, and of penetrating wounds of the trachea, by Dr. Alden March, is the paper next in order.

The principles of treatment of wounds of the abdomen, as laid down by Dr. M., vary according to the character of these wounds.

"1. Wounds in which the viscera protrude, but unattended with injury of the latter. "In cases of this description, the sooner the protruding part or parts are reduced, the better. If it be omentum, there is no great difficulty in accomplishing the object; but if an intestine has escaped at a small opening, and has been some time out, and is more or less distended with air or feces, the operation may be attended with some difficulty; and, rather than use harsh means, by too much handling and force, the wound should be moderately dilated, upon the same plan as that adopted in relieving the seat of stricture in an ordinary case of strangulated hernia.

"2. In penetrating wounds of the abdomen, with *protruded* and *wounded* intestine, the treatment will depend upon the extent of the wound in the intestine.

"*First*. If it be quite small, not larger than the end of a probe, the protruded intestine is to be gently reduced, without the application of ligature or suture to the puncture, and intrusted entirely to the recuperative power of nature for closure. "In such cases, the mucous or villous coat being long and loose, by moderate pressure is thrust into the puncture, so as to protrude at the opening in the peritoneal coat, and thus plug the orifice, where it is maintained by the gentle and equable pressure of the abdominal muscles, and the other portions of the abdominal viscera. "Nature's next step is to provide the means for, and to effect a permanent closure. This is accomplished by the exudation of plastic lymph from the exterior of the wound, by which this local point becomes fixed and adherent to the peritoneal surface with which it may chance to come in contact. This may be the peritoneal membrane lining the abdominal wall, or it may be the peritoneal membrane of another fold of intestine.

"*Second.* When the wound is considerably larger—say, of the size of a puncture made in venesection—the lips of it may be seized by the claw or 'mouse-tooth' forceps, deep enough to embrace the whole extent of the puncture, and by a fine silk ligature all that has been elevated by the forceps immediately below their extremities secured. "The extremities of the ligature should be cut off close to the knot, and the intestine cautiously and gently reduced. In this case, adhesive inflammation takes place before the ligature is cast off, so that the opening is permanently closed, while the ligature is said to find its way invariably into the alimentary canal, and pass off with the contents of the bowels."

"*Third.* In a class of wounds of the intestine, still larger, whether *longitudinal, transverse, or oblique*, the principles of treatment do not appear to be quite so well defined and settled. Some surgeons are averse to the use of any and all kinds of sutures, trusting entirely to the resources of nature; while others are advocates for sutures of various kinds, and variously applied, as the interrupted, the uninterrupted or continuous, and the Glover's suture. I shall not stop to describe the various modifications of these, or any sutures, but shall merely describe the one which appears most simple and philosophical, and recommended by some of the most modern authors.

"A small needle is to be armed with a single thread of silk or fine linen, having a knot fixed at the long end, and the extremity of the wound pierced with the needle from within to without, so that the knot may occupy the inner face or cavity of the gut; next, turn in the lips of the wound, so that the two peritoneal surfaces will come in contact with each other, and continue the Glover's suture, striking the needle through from within, so as to appear at least a quarter of an inch from the border of the wound on the peritoneal surface of the intestine. In this way, three, four, or half a dozen stitches may be applied, according to the extent of the wound; and at last cut the thread near the intestine. The two peritoneal surfaces adhere readily, and the ligature, as it ulcerates out, finds its way into the intestinal tube, and is removed with the feces.

"It has been advised by some surgeons, after securing the wound as above described, or by the overcast or whip-stitch, to bring the unexhausted portion of the suture out at the external wound, and there fix it, so that a greater amount of certainty may be secured in causing adhesion of the wound in the intestine to the wound in the abdominal wall. In this case, if the wound in the bowel should give way, it would not be likely to close till after its position had been fixed by adhesion, when the patient might recover with an artificial anus."

"The danger from *penetrating wounds of the larynx or trachea*," Dr. M. remarks, "may be immediate or remote.

"1. The immediate danger depends on hemorrhage in an excessive degree; as when the carotid and jugular have been wounded, or the thyroïdal and laryngeal branches of the carotid.

"2. When the patient is in a half-insensible condition, a moderate flow of blood from the wound may find its way into the larynx, and accumulate in the air-passages of the lungs so as to induce asphyxia. The second, or more remote danger, is liable to occur in the course of three or four days after the infliction of the wound, at the period of the development of inflammation. The inflammation of the wound and the mucous membrane may be such as to cause a rapid and almost perfect occlusion of the air-passage, especially when the mucous secretion is increased in quantity and vitiated in quality, and becoming so tenacious that the feeble efforts of the patient are inadequate for its removal. Under such circumstances, the patient is in great danger of suffocation.

"The third, and still more remote danger, will depend on the occurrence of inflammatory action of a severe character, descending through the whole length of the trachea and its ramifications in the lungs, and terminating in a severe form of bronchitis.

"The indications of treatment are few, simple, and easily understood, and yet liable to be forgotten or disregarded in the excitement and hurry of the occasion. I believe, however, it is the fact that the young physician more often forgets that a *penetrating wound* of the windpipe is more dangerous and requires

a different mode of treatment from a simple incised wound in other parts of the body. He has been instructed to secure bleeding vessels, cleanse the wound, approximate its margins, and secure perfect apposition, by which union by the first intention may be effected.

"The first indication of treatment is the same in both cases, that is, to arrest the hemorrhage, if it has not already subsided. This is accomplished by ligatures to the divided arteries, and pressure upon the wounded veins.

"The mistake which the ignorant or careless practitioner is liable to fall into is, in closing the wound tightly, in the larynx and in the skin, with sutures."

"If the chasm be at once drawn tightly together, the immediate risk is greatly enhanced, as already stated; and yet this is an error very frequently committed in the hurry of actual practice. There is another danger liable to occur: the air being likely to be infiltrated into the cellular tissue, during expiration, causing troublesome and dangerous emphysema."

The paper of Dr. March is followed by the history of a case of dislocated femur, by Dr. Samuel Shumay, of Essex County, New York. The reduction in this case was effected in the mode first taught, as Dr. S. informs us, by Dr. N. Smith, of Yale College, as early as the year 1815, and subsequently revived by Dr. Reid, of Rochester, in 1852.

The dislocation of the femur was backwards and upwards, on the dorsum ilii. Ether was administered, "so as to blunt sensibility without producing full anæsthesia." The leg was then flexed on the thigh, the thigh on the pelvis, diagonally to the opposite side, "thus using the femur as a lever, and the trochanter as a fulcrum, to throw the head of the femur on a level with the margin of the socket; and then, by a sweep outward and downward, throw the head of the bone in the socket."

The annual address of Dr. J. V. P. Quackenbush, before the Albany County Medical Society, follows next in order. The subject of this address is prolapsus of the bladder, as it occurs during labour, followed by biographical sketches of the late Doctors Lewis C. Beck and David Martin, of Albany. The account given by Dr. Q. of prolapsus of the bladder, as illustrated by three cases detailed by him, is highly interesting and instructive. The accident referred to, as the doctor remarks, is of very rare occurrence. In fact, it ought never to occur in any instance where the obstetrician is called in at the commencement of labour. Produced by "the pressure exerted by the descending head upon the fundus or middle portion of the bladder, at a time when it is partially distended with urine," it must be evident that the accident may be avoided in every case in which the obstetrician—as he should in the early stage of every labour—satisfies himself as to the empty condition of the bladder, as well as of the lower portion of the intestinal canal.

"The symptoms attendant on prolapsed bladder are very distressing; there is a painful sensation of fulness, tension, and pressure downwards in the situation of the pubes, with a feeling of dragging from the navel, or rather the midspace between the navel and pubic symphysis, constant desire to micturate, and inability to void urine on the exercise of the will, and sometimes an involuntary escape on each return of uterine contraction. On examination per vaginam, the finger will detect a soft, fluctuating tumour, filling up the anterior part and one or both sides of the pelvic cavity, below the foetal head, and the patient will complain if steady pressure be made on it. The introduction of the catheter, which may be easily accomplished, will at the same time withdraw the fluid, cause the swelling to disappear, and relieve the characteristic suffering. Keeping in mind the possibility of the bladder being pressed downwards, as described, it is an essential duty never to puncture any fluctuating tumour in the pelvic cavity without being first assured that it is not vesical, by the removal of the urine by means of the catheter.

"A chronic prolapsus of the bladder is likely to follow its accidental descent in labour."

From the address of Dr. Kneeland, before the Onondaga County Medical Society, which follows that of Dr. Quackenbush, we have already, in the commencement of the present notice, made some copious extracts.

The next article is a very manly vindication of rational medicine, or the regular practice as it is termed, in an appeal to the public, by Dr. Merrit H. Cash, of Orange County, New York. The address is short, pithy, and much to the point. It presents a just picture of legitimate medicine, "in the hands of an educated, professionally trained, discriminating faculty." We fear, however, that the picture of Dr. Cash, well drawn and correctly coloured though it be, will not attract very generally the attention of that public for whose benefit it was executed; and although the production of one who, being "no longer a practitioner," has evidently "no personal interest to subserve or feeling to gratify in respect to medicine, other than devotion to the medical good of the community," but few of those who may be induced to look upon it will, from its correct representation of the claims to public confidence and support of "rational medicine and the regular faculty," be induced to eschew "nostrums, patent medicines, and the various phases of medical empiricism which assail popular credulity, and bind it captive to their triumphal car."

A biographical sketch of the late Dr. Harvey W. Doolittle, of Herkimer, and an abstract of the proceedings of the annual session, complete the contents of the volume before us.

4. The minutes of the fifth annual meeting of the *Medical Society of the State of Georgia* contain, besides the minutes, properly speaking, of the session, a catalogue of the officers and members, and the constitution and by-laws of the Society, only two reports of a professional character. The first of these is on the relative value of lithotrity and lithotomy, by Dr. L. A. Dugas. In this report, Dr. D. has very ably and clearly set forth the advantages which lithotrity possesses over lithotomy in cases equally favourable to either operation; secondly, the intrinsic advantages of lithotomy in cases equally adapted to either; thirdly, the disadvantages of lithotrity and lithotomy respectively; fourthly, the obstacles in the way of lithotrity; and fifthly, those which contraindicate lithotomy. The general conclusions of the reporter in reference to the relative value of the two operations will be learned from the concluding paragraph of the report:—

"Having," he remarks, "now presented, even at the risk of tedious repetitions, the arguments which might be urged for and against both lithotrity and lithotomy, in cases deemed equally adapted to either of these operations, and also indicated the circumstances that should be taken into consideration in determining upon the adoption of one or other operative procedure, it may be expected of us to be more definite, and to avow a preference for either lithotrity or lithotomy as a general plan of treatment. In deciding upon the propriety of surgical operations, or upon the merits of particular processes, it will be often found a wholesome rule, and one by which the solution may be readily obtained, to suppose ourselves in the condition of the patient, and then to ask what would be our election under such circumstances. We have very frequently, in this manner, determined at once the course to be adopted in cases previously involving much doubt and hesitation. With regard to lithotomy and lithotrity, we are free to acknowledge that, if we were affected with stone, and the circumstances were equally favourable to the success of either operation, we would not hesitate a moment in giving the preference to lithotrity. The advantages of lithotrity are numerous and important; whereas the only intrinsic or valid objections that can be alleged against it, under the circumstances referred to, are to be found in the facts that the operation may have to be repeated, and that it leaves the patient more liable to a recurrence of the disease than lithotomy. The advantages of lithotomy unquestionably very far outweigh all the objections to it. So thought those celebrated practitioners, Lisfranc, Dubois, Boisseau, &c., who did not hesitate to do homage to lithotrity and to the skill of Civiale, by submitting to be lithotritized by him."

The second report is on chloroform in surgical and obstetric practice. It is by J. A. Eve, M. D., and advocates in the strongest terms the use of chloroform, more especially to relieve the sufferings of parturition, in the more difficult and painful cases, in the prevention and subduction of puerperal convulsions, and in cases of instrumental labour.

We admit the propriety of the reporter's directions for the employment of

the remedy when the propriety of resorting to it has been decided upon. We cannot, however, agree with Dr. Eve in his general and unqualified eulogium upon the propriety and perfect safety of a resort to chloroform in surgical and obstetric practice. Is it strictly true, as asserted by the reporter, that "the safety of chloroform, in all departments of medicine, may be regarded as established, by the *almost perfect impunity* with which it has been of late years so extensively and *indiscriminately* used, or I might perhaps with more propriety say *abused*," that "it may be unhesitatingly affirmed that no remedy, of even half the power, has ever been so generally and *recklessly* employed with so few fatal results"?

Does the reporter himself agree with his own assertions, as given above, when he says that "we would desire most earnestly to impress on our younger brethren the *importance of the greatest care and circumspection in its administration*? Instead of concealing the fact that *fatal cases have occurred*, we would rather point to them as beacons to warn the unwary of the *danger that may result from the careless and indiscriminate use of this truly potent and valuable agent*." "I would not be understood as regarding chloroform, or any other anæsthetic agent, *entirely devoid of danger* in obstetric practice, or that it might be given *recklessly and with impunity* in all cases." "It should always be exhibited *cautiously*, and never when the patient is excited; her mind should be calm and composed before inhalation is begun, *otherwise its effects might prove very unpleasant, if not dangerous*." "I would not advise its indiscriminate use in all cases."

We regret that the reporter has not given us the evidence upon which the following statement is based: "In *many* cases of reported death from this agent (chloroform), which have been investigated, it has been found that chloroform had not been administered, or else that dissolution might be fairly attributable to some other cause."

5. The *Transactions of the New Hampshire Medical Society* contain but little of a strictly professional character—only one paper, in fact—an essay on "The Chemical Changes that take place in the Human Body while in a State of Disease," by William H. H. Mason. This is, unquestionably, a most important subject, demanding a thorough investigation on the part of those who have the time, opportunity, and talents for the task. In the essay referred to, Dr. Mason merely indicates the general bearing of the morbid chemistry of the living body in reference to the pathology and therapeutics of disease.

In the transition from the doctrines of absolute humoralism, which, at one period, reigned dominant in the medical schools, to those of exclusive solidism, which subsequently, and for a long period, held an equal sway, the morbid changes which take place in the fluids of the human body, and the part these changes play in the production of disease or of its phenomena were, in a great measure, if not entirely, lost sight of. It is only recently that the morbid chemistry of the blood and other fluids has begun to attract the attention its importance merits, and the true character of the abnormal states of these portions of the living body subjected to accurate observation. With Dr. Mason, we are fully convinced that "a great field is here opened for further research—that pathological chemistry will yet discover new and highly important changes in the blood, which will lead to new remedies and a more correct treatment in many diseases." We agree with him, that it is even more than probable "many deficiencies of the blood, if well understood, might be made up by a change in diet, or by medicine, calculated to supply such deficiency; and more particularly, when certain principles are in excess, neutralizing and counteracting agents could be applied with much more certainty of success." We fear, however, that even after we shall have become fully acquainted with the nature and extent of the chemical changes in the fluids of the body during a state of disease, unless we shall, at the same time, be so fortunate as to detect the true pathological conditions under which those changes occur, and the causes upon which they depend, the anticipation indulged in by Dr. Mason, "that every part of the science of medicine will be reduced to a mathematical certainty," will be very far from being realized.

A very admirable address, on "Conservatism in Medicine," was delivered
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before the Society on the first day of meeting, by the President, Dr. Albert Smith, of Dartmouth College. The address is a well-written production, and inculcates sound principles, which, were they generally recognized and acted upon by the members of our profession, would do much to enhance its dignity and promote its usefulness.

The conservatism which Dr. Smith inculcates is not a stagnant acquiescence in existing doctrines and practices, a dormant indifference to all change and improvement, but a determined adherence to established truth and right, knowledge and experience, and a firm resistance to every innovation that bears not their manifest stamp. We fully concur with Dr. Smith, that "we need an element that shall have some restraining power over excitable and imaginative minds, that shall make them think and reason before they yield to delusion and folly. Even if conservatism grows so rigid as to occasion scepticism, it is far better for our art than too much credulity. It is this principle only on which we can rely for safety, amid all the dangers and extravagances which these inventive and progressive times impose; it is conservatism that must keep out errors—that must protect us from the follies of the visionary and the absurdities of the credulous."

"We need a conservative principle like this, to preserve the integrity and consistency of our profession. In an age like the present, an age of progress and improvement, unexampled in the history of the world, without some restraining or regulating force, there will be a tendency to excess and extravagance in both the theoretical and practical parts of our profession. It will not be empiricism alone we shall need to guard against—that will be too palpable and open to be mistaken—and will, of course, be shunned; but we shall need to be guarded against the new theories, the strange doctrines, the extravagant spirit of innovation and reform, that come not through science and experience; we shall need to turn a deaf ear and a cold shoulder to much that comes to us even under the sanction of the regular profession."

But, while Dr. Smith advocates a conservatism which shall guard the members of our profession from being carried away by every new theory, by false facts, one-sided observations, the vagaries of genius, or the brilliant dreams of the imaginative, with whom close investigation is folly, and analogical reasoning more convincing than the results of experience, he at the same time would warn them, on the other hand, against that spirit of conservatism which would wed them to that which is old, merely because it is old.

In the following remarks, made in reference to the recovery from disease, which occasionally occurs under the inefficient treatment of the homœopath, there is much truth:—

"The getting well, or not dying, is not all that is required of the healing art; there is such a thing as getting well 'right,' as justly remarked by an old, discriminating physician; that is, with all the inflammatory action subdued, with every vestige of the disease eradicated, and a restoration of all disturbed organic functions to a healthy condition. It is quite another thing to be left with organs struggling with half-subdued disease, and the kind, recuperative power of nature, striving to surmount it; only made tolerable, but, perhaps, never removed, by time, which, next to death, wears out all affections left lurking about the system."

The late Dr. Parrish was accustomed to remark, that, while regular scientific medicine is open to the admission of all truths, from whatever source derived, and is strengthened and perfected by every new accession of truth, all systems of empiricism, on the other hand, are adverse to every truth not originally embraced in the dogmas upon which they are founded; for, the moment other truth is received, they must necessarily fall to the ground. A somewhat similar idea is thus expressed by Dr. Smith, in the address under consideration:—

"Are we blind to truth, too conservative, because we are unwilling to acknowledge an indebtedness to any of the forms of empiricism, when we know to the contrary, and hold them all in the utmost detestation and loathing? The very few truths they contain do not save them from the most utter contempt. Why need we go to any sect or class to learn medical truth? It is ours the

world over. Any useful fact or discovery, by whomsoever made, belongs to us; and we have the undoubted right to say that, if there is any true system of medicine, we possess it. On what better basis could any profession be founded than ours—on antiquity, on science, and on experience?"

"There is no medical truth, or plan of medication under the sun, better than that we now employ, that we should not at once adopt, let who will make a hobby of it, or come from what source it may. All remedies, the world wide, are ours." "Unlike the systems of charlatanny, we are tied to no prescribed course or theory, but we are bound to know and to make use of *everything* that promises to be useful."

The reference made by Dr. Smith to the medical periodical literature of the day, is just and pertinent:—

"We know," he remarks, "how essential this is to the progress of our art, and how much it aids in bringing out and recording all the important facts and hints that occur, ready at hand, when a sufficient number have accumulated, to be used in developing medical truth. It is a means by which we may also record the results of our own experience, and perfect ourselves in arguing and expressing our thoughts in written language. It keeps us advised of all the important passing events in our sphere; heralds forth fresh discoveries, new modes of treatment, new uses of old articles, new phenomena, and strange appearances. We learn of the advent of new books; we are advised of their value, whether they will repay careful perusal. They stand the worthy exponents and representatives of our body, ready to vindicate us, and do battle, if necessary, among all the hostile legions of empirics and 'irregulars' of every grade. By their ability, dignity, and bearing, they give character and power to the profession; they infuse a right spirit into our body—a spirit of inquiry and observation, of perseverance and enterprise, that are of incalculable value. It is the life's blood of the profession, and none but a low and unworthy conservatism could urge a word against such important auxiliaries. Our medical periodical literature must be sustained, and it can be without much burdening the profession. It cannot be said they are too numerous. They have thus far only kept pace with the demand, and hardly equal the periodical literature of other great interests in society.

"At the present time, no physician, who has ambition to claim a respectable standing, can really attain it, unless he is careful to be thoroughly versed in, and familiar with, everything relating to his vocation. How shall he do this? No man can do it without effort; he must read and think, and, while striving to learn much unknown before, also to *unlearn* much that has hitherto constituted his medical capital, which, he is now satisfied, is wrong and unfounded. The medical journals must be carefully read, new books must be procured and consulted, and every discovery and improvement thoroughly investigated. We should have a horror of falling behind the age. The physician will find no ease; his scanty leisure from business engagements will leave little time for study—none for indolence, for gossiping, or lounging about the town or village; and does he wish to be a *full* man, he will industriously gather up these fragments of time, and, amid fatigue, embarrassments, and interruptions, yet acquire a large amount of knowledge."

The address just noticed is followed by an oration by Dr. Andrew McFarland, on "The Poetry of the Medical Profession." It is a well-written, pleasing production; disfigured, it is true, by an occasional conceit, and not always happy in its delineation of the true poetry of the life, and duties, and pursuits of the physician; the lights and shadows, the trials and triumphs, which checker with pain and pleasure the career of the most successful.

Appended to these *Transactions* are: 1st. A graphic account, given by Dr. Nathan Sandborn, of his escape from the Norwalk disaster, May 6, 1853; 2d. A short biographical notice of Dr. Josiah Bartlett, who was among the victims on that occasion; and 3d. Extracts from a Lecture on the Rights and Liabilities of the Physician and Surgeon, by Joel Parker, LL. D., Professor of Medical Jurisprudence in the Medical School of Dartmouth College, in the Boylston Medical School, at Boston, and in the New York Medical College. These extracts have been already extensively circulated in our medical journals.

6. The last of the publications, the title of which is placed at the head of the present notice, is the *Proceedings of the American Pharmaceutical Association*, at its annual meeting of 1854. Upon the proceedings of this Association, from its first organization in 1851, we have looked with the deepest interest. "The science of pharmacy should ever be regarded as the handmaid to the science of medicine;" it must at least be evident that whatever is calculated to elevate and improve the condition of the first will react favourably upon the interests of the latter. "The pharmacist stands between the physician and his patient. As the compounder of his prescriptions, the physician's success is, to a certain degree, in his hands. Such a responsible position requires a thorough pharmaceutical as well as practical training;" and we look with great confidence to the American Pharmaceutical Association as an efficient agent to elevate the profession to its proper position, by requiring of its members higher qualifications, both moral and educational, than merely the ability to perform the mechanical and financial business of a drug establishment. We look also to the action of this Association for the establishment of means calculated to insure the purity and genuineness of the drugs and medicines used and dispensed in the various pharmaceutical establishments of the United States.

The several papers which form the appendix to these proceedings will be read with interest by both physician and apothecary. D. F. C.

ART. XI.—*Reports of American Institutions for the Insane.*

1. *Of the Maine Insane Hospital, at Augusta, for 1853.*
2. *Of the Massachusetts Lunatic Hospital, at Worcester, for 1853.*
3. *Of the New York Insane Asylum, at Utica, for 1853.*
4. *Of the Bloomingdale Asylum, New York City, for 1853.*
5. *Of the New Jersey Lunatic Hospital, at Trenton, for 1853.*
6. *Of the Pennsylvania Hospital for the Insane, near Philadelphia, for 1853.*
7. *Of the Pennsylvania State Hospital, at Harrisburg, for 1853.*
8. *Of the Western Asylum of Virginia, at Staunton, for 1853.*
9. *Of the Ohio State Asylum, at Columbus, for 1852 and 1853.*

1. At the *Maine Insane Hospital*, the number of

	Men.	Women.	Total.
Patients at the beginning of the year was . . .	50	34	84
Admitted in course of the year	65	59	124
Whole number	115	93	208
Discharged, including deaths	54	35	89
Remaining at the end of the year	61	58	119
Of those discharged, there were cured	28	17	45
Died	11	4	15

Deaths from apoplexy 4, consumption 3, general paralysis 6, pneumonia 1, and laryngitis 1.

No epidemic prevailed, and no suicide occurred during the year.

We commend to the physicians of other asylums the method pursued by Dr. Harlow, in reporting the complete list of patients. The following is a synopsis of it.

		Disch'd cured.	Improved.	Unimp'd.	Died.
Persons admitted	1033	404	190	234	118
“ re-admitted	194	79	43	33	18
“ admitted a 3d time	50	26	2	7	2
“ “ 4th “	17	10	1	3	2
“ “ 5th “	11	8	2	0	0
“ “ 6th “	5	4	1	0	0
“ “ 7th “	5	2	2	0	0
“ “ 8th “	2	1	0	0	1
“ “ 9th “	1	0	0	1	0
Cases aggregate	1318	534	241	278	141

Of these, 71 were homicidal, and 129 suicidal; yet no accident has ever occurred from either of the former, and but two of the latter have destroyed themselves at the asylum. One of the homicidal men, removed from the institution against the advice of its officers, killed a man with an axe, in September, 1853.

Another wing for female patients is to be added to the establishment.

2. The *Massachusetts State Lunatic Hospital* went into operation in 1833. Under the energetic executive guidance of the late Dr. S. B. Woodward, it soon became very extensively known; and it has generally been esteemed, even to a comparatively recent period, not only as one of the best conducted, but also as one of the best constructed establishments of the kind in the country. But its Trustees now assert, in their report prefixed to that of Dr. Chandler, that it "has not only ceased to be regarded as a model institution, but it has fallen into the rear rank in the march of improvement." The halls are "low-studded, being only eight and a half, and nine feet high. They are warmed by furnaces which are very dangerous, and now nearly worn out. Their ventilation is so imperfect as not to deserve the name. The frequent occurrence of erysipelas is but one of the indices" of these defects. "There are forty-eight strong rooms, or cells, nearly all of them constructed of solid masonry, with iron doors." Some of them "are totally unfit for human habitations. There is an entire want of suitable yards (airing courts) connected with the buildings."

For these reasons, the Trustees, after having visited twenty-six public institutions, ten of them for the insane, in several of the States, recommend the construction of another establishment, and the evacuation and sale of that which now exists.

Let us follow these gentlemen in their tour of observation. "The Superintendents of the Lunatic Hospitals laid us under particular obligations for their generous courtesy, and the very frank and unreserved manner in which they exhibited their establishments, together with their methods of management and modes of treatment, pointing out improvements and criticizing defects. * * * No cell was found without a wooden floor, a wooden door, and plastered or ceiled walls. There is not a brick and mortar cell, with iron doors in either of the public establishments of the great States of New York and Pennsylvania, nor in the New Jersey State Lunatic Hospital, the design of which is quite generally regarded as a model.

"In the large establishment on Blackwell's Island, containing at the time of our visit, 574 patients, but two were found locked up, and those only temporarily, for a part of the day. The Superintendent, Dr. Rawney, stated that half-a-dozen strong rooms would be sufficient for that establishment, as not more than five or six a day were ever locked up, and those only for a few hours at a time. These rooms were used as sleeping apartments, and did not differ materially, in size, form, and appearance, from the other rooms occupied by patients, except the doors, which were stronger. These remarks in reference to the strong rooms are applicable to all the other establishments visited by us. In nine hospitals out of New England, containing about 2,250 patients, the number found under restraint, by being locked up, was only six. In one instance only had that restraint been continued more than two days, and that one had not exceeded a week.

"In the State Hospital, at Utica, but one person was found under restraint, and that one was confined in what they called a chicken-coop bedstead. There, they were in the process of demolishing their strong rooms, as useless. Their hospital was built some ten years after ours, and their strong rooms were comfortable apartments compared with ours, having wooden floors and plank doors, and were each furnished with a bedstead and bed.

"There are no more perfectly warmed and ventilated establishments in the country than the New York Hospital, the Pennsylvania Hospital, the New Jersey State Lunatic Hospital, the State Lunatic Hospital at Harrisburg, and the State Lunatic Hospital at Utica. In all these establishments, steam is the agent employed, and it gives perfect satisfaction. The plan at Utica, being

the most recent, is believed to combine more of modern improvements than any other. The Hospital at Utica, in all its appointments, may justly be regarded as a model institution, although its ground plan, in our opinion, is inferior to that of Trenton and Harrisburg."

That the buildings of the Massachusetts Hospital are very defective as compared with those of the institutions of recent origin, there can be no doubt; but it is very evident, that the Trustees have placed those defects in as bold relief as possible, by exhibiting them in the light of the broadest contrasts.

In reference to the practical application of the doctrines of Pinel, the Trustees make the following remark:—

"*Dr. Tuke, being the Superintendent, and possessing the whole control, found but little difficulty in testing them in practice at the York Retreat.*" No "Dr. Tuke," and no man named Tuke, was ever Superintendent of that Institution. We never heard, in England, of any Doctor Tuke. This is not the first time, however, that a *myth* of that name has been introduced into the profession, by persons on this side of the Atlantic. Samuel Tuke, for many years one of the Trustees of the Retreat, and well known by his publications upon insanity, was a tea-merchant, in York. Of the occupation of his ancestor, who was a member of the first Board of Trustees, we know nothing.

We now come to the report of Dr. Chandler.

	Men.	Women.	Total.
Patients in the Hospital, Dec. 1, 1852	264	268	532
Admitted in the course of the fiscal year	136	152	288
Whole number	400	420	820
Discharged, including deaths	134	166	300
Remaining Novembr 30, 1853	266	254	520
Of those discharged, there were cured	65	80	145
Died	21	20	41

No patients were received excepting such as were sent by order of the Courts, or by the Overseers of the Poor of towns. The Hospital was intended for a number of patients not exceeding 400; yet, at one time in the course of the year, there were 567. Of the 520 at the close of the year, 216 were foreigners; that is, not natives of Massachusetts; and of the latter number, 140 were Irish. "The Irish," says the report, "are almost invariably State paupers. Only three instances have come to my knowledge of their bills, or any part of their bills, having been paid by themselves, or by their friends."

	Men.	Women.	Total.
Whole number of patients from 1833 to 1853 inclusive	2239	2225	4464
Discharged, recovered	994	1059	2053
Died	253	239	492*

Causes of Death.—"Marasmus 78, consumption 58, apoplexy and palsy 53, maniacal exhaustion 49, epilepsy 45, disease of heart 20, disease of brain 20, suicide 19, lung fever 18, diarrhoea 18, erysipelas 15, old age 13, typhus fever 11, dysenteric fever 9, inflammation of the bowels 8, dropsy 8, hemorrhage 6, gastric fever 5, cholera 4, cholera morbus 4, chronic dysentery 4, mortification of the limbs 3, disease of the brain from intemperance 3, bronchitis 3, hydrothorax 3, congestive fever 2, convulsions 2, land-scurvy 1, concussion of brain 1, disease of the bladder 1, fright 1, rupture 1, asthma 1, cancer 1, pleurisy 1, jaundice 1, chorea 1."

Dr. Chandler gives a table, in which 406 of the patients who died are included, showing the relationship of insanity to longevity. Its substance is as follows:—

* In one table the report gives but 491. The number of admissions in 1835, is stated in one place as 113, in another, 119. This discrepancy affects the whole number, making it but 4458, if the 113 be correct. We have quoted from the table in which the number of each sex is given.

	Years.	Months.	Days.
Average age of 201 males when attacked	42	8	10
Average age of 205 females when attacked	39	1	10
Average age of the whole number, 406	40	11	1
Average duration of insanity before admission, 201 males	4	2	9
Average duration of life after admission, 201 males	1	9	24
Average duration of life after the attack, 201 males	6	0	3
Average duration of insanity before admission, 205 females	3	3	6
Average duration of life after admission, 205 females	1	7	29
Average duration of life after the attack, 205 females	4	11	5
Average duration of life after attack of the whole number, 406	5	5	20
Average age at death, 201 males	48	8	13
Average age at death, 205 females	44	0	15

"The chance of life," says the report, "for persons in health at corresponding periods, as calculated and acted upon by life-insurance companies, is four times greater than is here exhibited for the male, and more than five times greater for the female. This shows, pretty conclusively, that insanity, when not recovered from, tends to shorten life."

3. The movement of patients at the *New York State Asylum*, in course of the fiscal year ending November 30, 1853, was as follows:—

	Men.	Women.	Total.
Patients at the commencement	215	210	425
Admitted	251	173	424
Whole number	466	383	849
Discharged, including deaths	227	176	403
Remaining at end of the year	239	207	446
Of those discharged, there were recovered	95	74	169
Died	19	20	39

Deaths from phthisis, 11; exhaustion, 7; general paralysis, 5; epilepsy, 3; exhaustive mania, 2; apoplexy, cerebral effusion, tumour of brain, pericarditis, hypertrophy and dilatation of heart, typhoid fever, dysentery, chronic diarrhoea, phlegmonous erysipelas, erysipelas from wounds received prior to admission, contusions received before admission, 1 each.

"The seven cases reported as having died of exhaustion, were wasted by disease or vicious habits. Three of them, at the development of mania, had been purged, blistered, and profusely bled, and were brought to the Asylum on beds. This injudicious treatment cannot be too strongly condemned. The recuperative powers in these were so far exhausted that no amount of care, stimulation, and nutrition could arouse them. It may be proper to state that a number of persons received in a state of extreme feebleness, after long nursing, watchfulness, and free stimulation, recovered."

"No case of suicide has occurred for more than two years."

The number of men admitted was greater than in any preceding year; that of women was diminished by an inability to receive them, on account of the extensive alterations in their department, necessary for the introduction of the new apparatus for heating and forced ventilation. Sixty applications were rejected, and forty-seven incurable cases discharged, to make room for curables.

In eleven of the cases admitted, the insanity was complicated with epilepsy. "In six of them, epilepsy came on at puberty, preceded the derangement, and was the exciting cause. In the others, the epilepsy commenced in childhood, and early induced dementia. In the table of causes, two of these cases are put down to the intemperance of the father; the father being represented as in a state of beastly drunkenness most of the time for a few years previous to their birth. Nine of the whole number of epileptics had a drunken parentage, and, in some, this vice extended several generations back.

Of seven cases in which the mental disorder was accompanied by general paralysis, six were of intemperate parentage, and three of the six "had a drunken and licentious ancestry." In fourteen patients, ten men and four women, the insanity is ascribed to "spiritual rappings."

"No epidemic prevailed during the year. One case of variolous disease appeared in May, which was immediately isolated, and the entire population of the house vaccinated, after which no other case occurred. This was a case of acute dementia, of eight months' standing and seven months' residence. *The mental affection entirely disappeared simultaneously with the full eruption of the disease.*"

There may be imprudent haste, as well as unwise delay, in regard to the removal of insane persons to the institutions devoted to their treatment. Especially is this true when the removal involves a long and wearisome journey, which the patient, disabled by physical disease or debility, is unqualified to bear. A woman mentioned in this report, while labouring under acute puerperal mania, "was brought from a distant State, *a journey of eight hundred miles, THREE WEEKS AFTER DELIVERY*, and was wasted to a skeleton, and not able to speak when received. She had not taken food or drink for four days, nor slept for seventy-two hours." If any reader should have a doubt as to the result of this case, he may remove that doubt by referring to the report.

In the record of general results already quoted, it will be perceived that 169 cases are reported as "recovered." In a subsequent and more specific table, these recoveries are arranged under two heads, viz: "Well," and "in usual health." Of the former there are 126, and of the latter 42. Our former notices of the reports from Utica have given Dr. Benedict's reasons for this distinction. A similar method of reporting is adopted in some of the German institutions. It is, probably, a more accurate method than that which is the most generally in vogue.

As the results of the industry of the female patients and attendants, it is stated that they repaired all the clothing and bedding, and made 5,760 garments and articles of domestic use. The tailors' shop produced 60 coats, 149 vests, and 205 pantaloons; and the carpenter shops, numerous articles of household furniture.

Aggregate of patients, from Jan. 16, 1843, to Dec. 1, 1853	3,923
Discharged recovered	1,625
Died	446

4. From the general statistics of the *Bloomington Asylum*, we abstract the following:—

	Men.	Women.	Total.
Number of patients, Jan. 1, 1853	52	67	119
Admitted in course of the year	73	62	135
Whole number	125	129	254
Discharged, including deaths	69	61	130
Remaining, Dec. 31	56	68	124
Of those discharged, there were cured	21	28	49
Died	13	9	22

Deaths from typho-mania, 4; chronic mania, 4; abscess in the brain, 3; epilepsy, 3; *paralysis générale*, 3; puerperal mania, 2; mania-à-potu, 1; apoplexy, 1; serous effusion within the cranium, 1; pleurisy, 1.

Seven of the patients died within one week after admission.

The subjoined extract presents the views of Dr. Brown in regard to the numerical method as applied to insanity:—

"The terms *recovered*, *improved*, and *not improved*, as used in reports of this character, must necessarily be in some degree indefinite in their signification; they represent only the *opinion of the reporter* on the cases embraced in the opposite numerals. That this opinion will be determined, or modified by the observer's temperament, is a fact so well known to those familiar with the sub-

ject that the 'statistics of insanity' are very generally regarded as collections of individual opinions, rather than as reliable scientific data. With the sincerest desire to arrive at entire accuracy, it is not unfrequently difficult to determine the exact state of the mind at the moment of the patient's discharge. While, in one instance we may be discomfited by the sudden relapse and return of one dismissed as convalescent, our chagrin may be soothed by the assurance that another, whose removal we had strongly resisted as imprudent and critical, has progressed to complete restoration. It may even be somewhat questionable whether that degree of improvement which justifies enrolment among the 'recovered,' can, in every case, be adequately determined as the patient is leaving the asylum, as yet unsubjected to the test of association with the world, and unexposed to influences which may have produced his disease.

"The marked contrariety of opinion as to the justice of characterizing certain phases of a still existing malady as an *improvement*, may well qualify confidence in the numerical method of estimating results of treatment in mental diseases. The subsidence of agitation, noisy declamation, and violence, followed by a prolonged period of calm, does not necessarily indicate a better condition of mind; nor do improved physical health, and discontinuance of bad habits, invariably point toward recovery. Yet, each of these supposed changes is desirable as an improvement on its antecedent state, and, while some physicians *exclude* from the class of *improved* all cases in which an approach towards recovery from the mental derangement be not apparent, others, with equal respect for truth, *admit* all in which the above-named desiderata are attained."

So fully have we concurred in the opinions advanced in the paragraph last quoted, that, for several years, in making our extracts from the statistics of the reports, we have entirely omitted those under the heads, "much improved," and "improved." That the temperaments of the superintending physicians of the various asylums differ, it is reasonable to suppose; and that the judgment of each physician is somewhat influenced by his specific temperament, is a proposition which will not be contested by any person much versed in physiology and psychology. But, that this influence is sufficient to destroy our confidence in the statistics of *cures*, reported agreeably to a sincere conviction of truth, we cannot believe. If it be, the sooner the practice of reporting them is discontinued the better will it be for the progress of true science.

There is much truth, as well as appropriateness, in the following remarks near the close of the report before us:—

"We have been too prone to regard the *balance sheet*, the *farm account*, and the *report of articles manufactured* as matter of special solicitude, contemplating the patient as an agent in the industrial hive, rather than as the object of all the accumulated means of treatment. In the lunatic hospital, as in society and in the State, the individual must be prominent. The very disease for which he is admitted tends ultimately to destroy individuality. For this reason his identity must be preserved, his just claims recognized, his self-respect encouraged, and his mind incited to useful or refining occupation. In this kind of moral treatment, some of our co-labourers of the Old World excel us. To emulate their merit, we need a courageous zeal which shrinks from no obstacle, a generous enthusiasm that waits not to weigh restored minds against a diminished credit balance, and the stimulating conviction that laurels yet ungathered line the steeps above us."

Large additions to the "lodges" of the Asylum have recently been erected, and the new method of heating, in connection with a forced ventilation, introduced into those buildings. The number of applicants for admission into the institution is greater than the means of accommodation. In 1836, there were upwards of 160 patients. The departments now occupied by patients are at least fifty per cent. more extensive than at that time; yet Dr. Brown proposes to limit the number, in future, to 150. This recognition of the importance of sufficient room is one among many evidences of improvement.

5. By the report of Dr. Buttolph, it appears that the number of patients in the *Asylum at Trenton*, Jan. 1, 1853, was

	Men.	Women.	Total.
Admitted in course of the year	91	91	182
Whole number	56	63	119
Discharged, including deaths	147	154	301
Remaining January 1, 1855	49	47	96
Of those discharged, there were cured	98	107	205
Died	27	26	53
	10	7	17

Deaths from general exhaustion 4, consumption 3, epilepsy 3, apoplexy 4, congestion of brain 1, congestion of lungs 1, chronic diarrhoea 1.

The patients enjoyed "a remarkable exemption from all acute and epidemic diseases" throughout the year.

The liberal donation from Mr. Randolph, mentioned in the last preceding report, has been devoted to the construction of an octagonal stone building, thirty-two feet in diameter, lighted from the top, and surrounded by a portico eight feet in width. The interior will be finished in a style appropriate for a handsome reading-room and museum.

	Men.	Women.	Total.
Patients admitted from May 15, 1848, to Dec. 31, 1853	320	314	634
Discharged, recovered	108	106	214
Died	36	35	71

To this brief report are appended the "Propositions relative to the construction of Hospitals," and those "On the organization of Hospitals," which have been issued by the Association of Medical Superintendents of American Institutions for the Insane. Had that Association achieved no other good, the production of these two documents would alone have been a sufficient recompense for all its labours. In future, should there be a hospital for the insane erected, and put in operation, with the imperfections of those which were established twenty years ago, it will not be for the want of available means for their prevention.

6. The report of Dr. Kirkbride, for 1853, furnishes the following statistics of the movement of the inmates of the *Pennsylvania Hospital for the Insane* in the course of the year:—

	Men.	Women.	Total.
Patients December 31, 1852			215
Admitted since that time			191
Whole number	205	201	406
Discharged, including deaths	93	78	171
Remaining Dec. 31, 1853	112	123	235
Of those discharged there were cured			88
Died	10	5	15

Deaths from acute mania 4, softening of the brain 3, exhaustion from long-continued refusal of food 2, tubercular consumption, chronic inflammation of the lungs, chronic diarrhoea, disease of the bladder, sloughing of the perineum, and old age, 1 each.

Of seven patients prematurely removed from the hospital, five were believed to be curable.

	Men.	Women.	Total.
Patients admitted since the opening of the hospital	1299	1099	2398
Single	708	427	1135
Married	536	530	1066
Widowed	55	142	197
Cured	622	515	1137
Died	142	103	245

Insanity commenced before the patient was 10 years of age, in 5; between 10 and 20 years, in 282; 20 and 30 years, in 915; 30 and 40, in 577; 40 and 50, in 290; after the fiftieth year, in 229.

During the whole of the past year, "the institution has been rather more than comfortably filled, the average number being 229, while 220 is regarded as the capacity of the building." The highest number was 248. Some applications for admission were refused. The elaborate system of moral management heretofore pursued at this hospital, and pretty fully described in our previous notices of the reports emanating from it, is still continued. We still await, however, the introduction here, as well as at all the other similar establishments in the United States, of one feature in the general treatment, without which it is believed that no institution for the insane can be perfect. We allude to an active, thorough, energetic system of disciplinary, gymnastic, hygienic, physical and mental improvable and curative management of the chronic cases—even of those who may have been more or less demented, torpid and stupid, and perhaps given up as incurable for years. In short, a school for idiots, technically speaking, is needed in every large institution for the insane. We have wonderful results from those schools in Germany, Switzerland, France, England, and, to some extent, in this country, where the subjects were congenitally imbecile. We anticipate success no less eminent among those whose demention is acquired, for we are fully convinced that the physical lesion to be overcome is, in a large proportion of cases, a less discouraging obstacle in the latter than in the former. All things, and especially the *power*, by his facilities for the acquisition of the means, point out Dr. Kirkbride as the man to become the pioneer in this undertaking. The hospital under his superintendence already approximates so nearly to perfection, that there is some danger of his becoming the Alexander of his sphere, and weeping that there are no more realms to conquer. But while among his patients, one imbruted remnant of that which was once a man, moves only in obedience to the calls of nature, and of his attendant, perhaps to the latter alone, everything is not accomplished. While along the benches, or on the floors, in corners or partially secluded nooks, lying, sitting, crouching, or standing in listless inactivity, are those who still bear some relic, how slight soever it may be, of their former intellectual manhood, so long will the necessity be indicated for that systematic, physical, and mental schooling, which has been mentioned. Be it understood, that we are pointing to an entirely new era in the history of our institutions, specially devoted to the insane; an era within the first gleams of the aurora of which we have been brought by the progress of the last half century. We believe, that at the present time, the class of patients in question receive no more efficient treatment in any of our Asylums, than in the Pennsylvania Hospital; and we have alluded to them there by partial description, only because we know that such a class exists, and, but too often, a very numerous class, in every establishment of the kind.

Among the important improvements of the past year, mentioned in the report before us, is a serpentine carriage-road through the pleasure-grounds of the department for females. A similar road is in progress through the grounds devoted to the men. When the latter is completed, a drive of one mile and three-quarters can be taken within the walled inclosure of the Hospital.

Gas has been introduced, for lighting the buildings, and with three times the amount of light formerly furnished by oil, the actual expense is less.

Dr. Kirkbride devotes several pages to the discussion of the question whether insanity be increasing in a greater ratio than the population. "It is not difficult," he remarks, "to understand that there may be elements in operation in this country more likely to produce mental derangement than in most others; but at the same time, it must also be conceded that other causes, elsewhere prevalent, are here absent; and which, different as they are in their general character, tend to produce nearly the same effects. While the general prevalence of comfort among our whole population, the comparative ease with which nearly every individual may earn a livelihood, and the absence of tyranny and a grinding oppression of the poor and dependent, ought to contribute no less to the mental than to the physical well-being of the whole community; still,

some of the characteristic traits of our people, originating in this happy state of things, tend to a different result. The very active and wide-spread commercial speculations of our citizens, the incessant taxing of the mental and physical powers to their utmost, the absorbing pursuit of business, aiming at rapid success, and the hasty accumulation of wealth, is a state of constant mental anxiety, of labour without relaxation; and it is too often a mere lottery, in which great and sudden good fortune is the exception, and loss and disappointment the more common, though less noted results." The man of business, when able to retire, is unfitted for the change; "he discovers, with surprise, that long habit has rendered the excitement, the toil, and the anxieties of business, great as they may have been, among the necessities of his existence; and irksome as he may occasionally have found them, he now concludes that they are infinitely preferable to the *ennui* which presses so heavily upon him. Fortunate is the man thus situated, who can take a hearty, permanent interest in other pursuits, who can engage in works of benevolence or of public utility that will render him not only a benefactor to his species, but will also preserve him from an indulgence in habits that may ruin him physically, and from yielding to feelings which may seriously impair the functions of the mind." After mentioning other causes, the conclusion is arrived at, that, "it will probably be found that the number of cases (of insanity) among us has *not* increased in a greater ratio than that of the general population." The greater prominence, during the last few years of the subject of insanity, and of its subjects is mentioned as only an apparent, not a real indication of the increase of the disease. The filling up of the hospitals, also, "does not prove that insanity increases more rapidly than the population." "Philadelphia, in 1830, had accommodations for 385 insane, with a population of 188,961. At the end of 1840, with a population of 258,037, she could provide for about 530, and now, with half a million of inhabitants, her different institutions can receive 630 patients." Thus, in regard to that city, the provisions for the cure of the insane have not kept pace with the population.

But further accommodations are needed, and hence Dr. Kirkbride suggests, "that a new Hospital, replete with every modern discovery, and all the improvements suggested by a large experience, and capable of accommodating 200 male patients, should be erected on the seventy acres of land now comprising the farm of this institution, and directly west of its present inclosed pleasure-grounds; while the present buildings, with everything included within our external wall, should be given up for the exclusive use of a similar number of females."

7. We glean from the report of Dr. Curwen, the subjoined sketch of the movement of the population of the *Pennsylvania State Hospital*, in 1853.

	Men.	Women.	Total.
Patients in the Hospital Dec. 31, 1852	59	47	106
Admitted in course of the year	95	68	163
Whole number	154	115	269
Discharged, including deaths	55	32	87
Remaining, December 31, 1853	99	83	182
Of those discharged, there were cured			27
Died			17

Causes of Death.—Epilepsy 5, exhaustion consequent to chronic mania 5, paralysis 3, "disease of the lungs" 2, acute inflammation of the brain 1, gradual decay of the vital powers 1.

"The general health of the household has been good. We have been spared the visitation of any epidemic, and only a few cases of disease incident to the season were under treatment during the summer and autumn." "Several of those who were much improved at the time of their removal, subsequently regained their former mental vigour." "A little girl, three years and four months old, evincing unequivocal symptoms of mental disorder, was admitted in the early part of the year. The mental disorder was recent. This case, so

interesting on account of the age and mental peculiarities, still continues under treatment."

In the table of supposed causes we find the following: "Millerism 1, spiritual rappings 1, religious excitement 2."

Small libraries have been established in some of the wards. "Pictures of a cheerful character hung on the walls, and mottoes suggestive of pleasant ideas, and printed in large letters, have been introduced into the wards, more particularly of the excited classes." The donations from Philadelphia, collected by Miss Dix, and mentioned in our notice of the report for 1852, amounted to \$5,182. "The museum and reading-room buildings have been finished. They are 42 feet long, by 25 feet wide. A portico runs nearly the whole length of the front, from which a very pleasant view is obtained. They are placed one on either side of the front of the building; and each is easily accessible from the wards of the sex for which it is intended. It is proposed, so far as can be done, to procure the mineral and geological productions of the different parts of the Commonwealth, and to give to each County so much room as may be needed to exhibit the specimens obtained."

8. In the twelve months preceding the 30th September, 1853, the number of patients at the *Western Asylum of Virginia*, exceeded, by twenty-two, that of any preceding year. No malignant or epidemic disease occurred among them. Neither was there a case of suicide. Of the 1264 patients received since the opening of the Asylum, only five have terminated their existence with their own hands. In two of these there was no certainty that the death was not accidental.

	Men.	Women.	Total.
Patients at the beginning of the year	202	138	340
Admitted in course of the year	69	51	120
Whole number	271	189	460
Discharged, including deaths	54	29	83
Remaining at the end of the year	217	160	377
Of those discharged, there were cured	26	15	41
Died	17	10	27

The diseases terminating fatally are not reported. Among the causes of insanity, we observe, that the "excessive use of tobacco" is mentioned in *three* of the cases, "inhaling tobacco fumes" in *one*, and the "excessive use of tobacco and ardent spirits" in *one*.

Of the 460 cases, the insanity commenced before the age of 20 years, in 67; between 20 and 30, in 160; between 30 and 40, in 93; between 40 and 50, in 50; after the fiftieth year, 18; unascertained, 72. The great preponderance of the decennium from 20 to 30 years will be perceived.

	Men.	Women.	Total.
Aggregate of patients admitted since July 1, 1836	711	474	1185
Discharged cured	270	184	454
Died	150	86	236

Aside from the statistical tables, the report of Dr. Stribling is almost exclusively occupied in the description of improvements recently made upon the premises, and the suggestion of others. Gas was introduced for the purpose of lighting the apartments, on the 1st of January, 1853. "We are now satisfied," says the report, "that the institution can be supplied from the gas-works with an amount of light far greater than that which it formerly derived from oil, lard, and candles, for a small fraction of what these materials cost."

As this establishment and the Eastern Asylum can accommodate but about 700 patients, and as there are within the State, according to the last census, 922 insane whites, and 945 white idiots, many of the latter probably not congenitally idiotic, the Doctor urges upon the Legislature "to make at once a liberal appropriation for the erection of another Asylum" for 250 patients.

of the patients alluded to, there was no delusion or insanity, other than that which might exist in regard to the so-called "spiritual manifestations."

Forty-eight of the patients admitted had the suicidal propensity. Thirty-five of them had attempted self-destruction. Of the thirty-five, thirteen had recovered from their mental disease at the close of the year.

	Men.	Women.	Total.
Whole number of patients, 1839 to 1853, inclusive	1220	1135	2355
Discharged recovered	601	570	1171
Died	188	135	323

Diseases which proved Fatal.—"Exhaustion and general decay, without discoverable local lesion, 56; consumption, 52; dysentery, 31; diarrhœa, 30; epilepsy, 28; fever, 28; inanition, 26; apoplexy, 16; palsy, 9; inflammation of the lungs, 7; dropsy, 6; inflammation of the brain, 4; inflammation of the liver, 3; chronic inflammation of the peritoneum, 3; tabes mesenterica, 3; suicide, 3; inflammation of the pericardium, 2; inflammation of the stomach, 2; erysipelas, 2; organic lesion of the brain, 1; caries of the vertebræ, 1; chronic inflammation of the bronchia, 1; bilious colic, 1; ulceration of the bowels, 1; inflammation of the kidney, 1; cancer of the womb, 1; cutaneous cancer, 1; gangrene of the face, 1; exhaustion from journey, 1; confluent smallpox, 1; cause not assigned, 1."

The State Legislature has made appropriations for warming the buildings by steam, in connection with forced ventilation, and for the construction of an infirmary.

P. E.

ART. XII.—*Report of the Select Committee of the Senate of the United States on the Sickness and Mortality on board Emigrant Ships, August 2, 1854.* 8vo. pp. 147.

THE present report is that of a select Committee appointed under an order of the Senate of the United States, passed on the 7th of December, 1853, "to consider the cause and the extent of the sickness and mortality prevailing on board of emigrant ships on the voyage to this country; and whether any and what legislation is needed for the better protection of the health and lives of passengers on board of such vessels." To which Committee were also referred a petition of the American Medical Association, and a petition of inhabitants of New York, in relation to the same subject.

The Committee appear to have given to the task entrusted to them, all that attention which its importance, as well with reference to the business interests of the country as to the cause of humanity at large, demands.

"With a view to the procuration of accurate data from which to form a correct judgment, a circular was prepared, containing fourteen questions relative to the police, and sanitary management of emigrant ships, to which specific answers, based as far as practicable upon experience, were requested. Copies of this circular were addressed and sent to members of the medical profession (whose opportunities of forming correct opinions on the subject had been furnished by experience, and therefore are entitled to consideration), to merchants, to navigators who had been engaged in the passenger trade, to persons connected with the collection of the customs, to presidents of benevolent societies for the relief of indigent emigrants, and to others throughout the country, and in the cities at which the landing of foreign passengers has chiefly taken place."

The answers received to these circulars were very numerous, and, in many instances, are drawn up with great ability, furnishing materials in abundance for a most interesting report; and such the one before us unquestionably is.

It is well known that the sickness and mortality that has prevailed, for many years past, on board of emigrant ships arriving at the different ports of the United States, has been, in many instances, to an extent truly appalling.

Nor is it at all surprising that typhus fever, and cholera, and smallpox should occur, in their most malignant forms, among the passengers on board of emigrant vessels, destroying, every year, many thousands during the voyage, and as many more subsequent to their arrival here, when we consider the impaired health, the squalid and depressed condition of a large number of the emigrants when they enter on board, the over-crowded condition of the vessels, the want of adequate ventilation, the too general inattention paid to cleanliness, the badly cooked food—itsself, frequently, of an inferior quality—the neglect of exercise and of a proper sanitary police generally, throughout the voyage.

From the returns received by the Committee of the Senate, it appears that the percentage of mortality among the passengers on board of New York vessels has been considerably greater than among those on board of vessels trading to Boston and other ports.

“This,” in the words of the report before us, “might have been, perhaps, expected, for a variety of reasons. New York being the great commercial emporium of the Union, passengers from every country in Europe have been induced to regard it as the point to which they should direct their courses. Hence, the huge structures, furnished by the enterprise of that great metropolis for the transportation of passengers, have been crowded to excess, and, as a necessary consequence, the causes of disease have existed on board of those vessels to a greater extent than on any other. In general, the percentage of deaths decreases in proportion as the number of passengers is less; and it is found that where passengers have been distributed in smaller numbers, disease and death have been less prevalent. The table (annexed to the report) shows that the smallest percentage of deaths has occurred on vessels from ports of Europe other than those of Liverpool, London, Bremen, Hamburgh, and Havre, which, being out of the great thoroughfares of commerce, have presented fewer attractions to the great mass of emigrants. The vessels from these ports, being less crowded, are more easily ventilated and kept clean, and present greater facilities for the proper preparation of the food of the passengers, and for their exercise in good weather.”

The means by which the occurrence of disease and the consequent destruction of life among the passengers on board of emigrant vessels may be obviated are sufficiently evident. By a proper apportionment of the number of the passengers to the available accommodations afforded by each ship; by excluding from on board persons already diseased, and all foul and infectious baggage, as well as all individuals who are in a condition strongly predisposed to an attack of any contagious malady and liable to propagate it among the entire ship's company, in connection with the strict enforcement of a proper system of nautical hygiene throughout the voyage, the ratio of sickness and of deaths on board of emigrant vessels would be reduced even below that of the communities at home from which come the major portion of the emigrants to this country.

But while all who have paid any attention to the subject must be convinced as to the means best adapted to preserve the health and lives of emigrant passengers, there stands in the way of carrying these means into effect an almost insuperable obstacle. The enactment of the most precise and stringent regulations by the Congress of the United States would not be sufficient; the accomplishment of the important object aimed at by such regulations could not be attained unless they were legalized and enforced in every port in Europe where the embarkation of emigrants to this country takes place.

“The great difficulty,” says the report before us, “of preparing a general law which shall embrace in its operation all the minute points involved in the management of passenger ships, and effectively place the passenger and ship owner on the best possible footing, must present itself to every mind. In the first place, our country being the general receptacle of emigrants from almost every country of Europe, it becomes necessary so to legislate as to avoid coming into conflict with the legislation of the countries whence these people come, and, at the same time, protect their interests and the interests of American citizens at home and abroad. Great Britain, France, the various nations of Germany, the Hanseatic cities, have all of them their passenger laws, in which they prescribe

the terms upon which those who emigrate from among them shall be conveyed across the ocean. These laws or regulations include not only the equipment and nautical management of the ships engaged in this trade, but also prescribe such dietary provisions as to their makers respectively appear essential to the health and comfort of passengers. To legislate in this country, therefore, so as not to come in conflict with these foreign enactments on some of the many minute points which present themselves in the treatment of such a subject, requires an extent and accuracy of information on details difficult to attain, and would involve a minuteness and variety of legislative enactments suited to the local requirements of each country, whence emigrants seek a home on our shores, and liable to become oppressive whenever a change of policy suggests a change in those foreign enactments. It is, of course, the intention of every law-maker, that the laws passed by him shall be enforced; and to place enactments on the statute book which cannot be carried into effect without subjecting those who are governed by them to difficulty and annoyance from foreign countries, is worse than useless. That such a state at present exists, under the passenger laws now in force, has been proved by experience; and those engaged in the transportation of passengers do not hesitate to say, that the laws under which they are acting are in many respects impracticable, unless at great expense and loss to the American ship-owner. It is only necessary to allude to one case of the kind by way of illustration. The acts of Congress require that every passenger ship shall be furnished with provisions of a certain description to a given amount. An American ship goes to Bremen or Hamburg, and there takes on board a cargo of passengers. The laws of those cities require that all passenger ships sailing from them with passengers shall be supplied with a prescribed amount of certain provisions, which are specified. Now, in order to comply with the regulation of the port whence he sails for America, the American captain must provide a supply of the articles required by the local law, and, at the same time, to comply with the law of his own country, he must be provided with the food called for by the act of Congress. This single case involves a double expense in the provisions made for the subsistence of passengers."

To obviate the difficulty above referred to, the Committee have endeavoured to frame a law which shall, in its general operation, provide for the safety and comfort of passengers, and, at the same time, leave the management of minute details in the hands of those whose interest as well as business it is to be thoroughly acquainted with them. They consider it to be utterly impossible for Congress to know what is required to make each ship comfortable and healthy. Thus, one vessel may be ventilated and made perfectly wholesome for passengers by a process which, in another vessel, and under different circumstances, would be totally inadequate. The Committee have, therefore, believed it best to leave the means by which ships are made safe and comfortable mainly at the disposal of their owners; and, at the same time, to *make these responsible* for any untoward results that may attend the administration of their own affairs.

The Committee refer to a report on Quarantine, presented to the British Parliament, in 1849, in which the subject of the means for preventing sickness and death on board of passenger vessels are elaborately discussed. The Commissioners by whom this report was drawn up, Lords Carlisle and Ashby, and Edwin Chadwick and T. Southwood Smith, remark that, "the experience of the mercantile navy itself affords an example of the successful working of a large preventive principle of jurisprudence; that is to say, *the principle of concentrating responsibility on those who have a direct interest in prevention, and who possess the best means of securing it.* We would request attention to those examples for international consideration."

The report referred to then goes on to say:—

"It is stated that when the system of transportation was first adopted in some of the earlier voyages, full one-half of those who embarked were lost; later, on the passage to New South Wales, as in the 'Hillsborough,' out of three hundred and six who embarked one hundred were lost; and in another ship, the 'Atlas,' out of one hundred and seventy-five embarked sixty-one were

lost. Yet there were no omissions palpable to common observation, or which could be distinctly proved as matter of crimination to which responsibility might be attached. The shippers were, no doubt, honourable men, chargeable with no conscious designs against the lives of the human beings committed to their care, and with no unusual omissions; but their thoughts were directed, by their interests, exclusively to profits; they got as much freight as they could, and they saw no reason why convicts or emigrants should not put up with temporary inconveniences to make room for cargo.

"By a simple change—based on the principle of self-interest, the most general, uniform, and, when properly directed, really beneficent of all principles of action—by the short alteration of the terms of the contract, so as to apply the motive where alone there was the effectual means of prevention, by engaging *to pay for those landed alive*, instead of paying for *all those embarked*, these extreme horrors were arrested; the generation of extensive mortal epidemics was, in a short time, prevented, and clean bills of health might have been given to all the ships which before would have been entitled to none. From the report of the select committee on transportation, in the year 1812, it appears that in one period, namely, from 1795 to 1801, out of three thousand eight hundred and thirty-three convicts embarked three hundred and eighty-five died, being nearly one in ten. But, since 1801, after the principle of responsibility began to be applied, out of two thousand three hundred and ninety-eight embarked only fifty-two have died, being one in forty six. The improvement has continued up to the present time, when it amounts to only one and a half per cent., or even lower than the average mortality of such a class living on shore. The shippers themselves, without any legislative provisions, or any official supervision or regulations thereto, appointed medical officers or surgeons, and put the whole of the convicts under their charge; the shippers attested their own sense of the propriety, sound policy, and efficiency of the principle, by voluntarily adopting it and applying it to each ship-surgeon in charge, whose remuneration was made dependent upon the number of passengers landed alive."

The same principle, we are told, has been adopted in contracts for the transportation of troops, and found to operate equally well; also to the transportation of pauper emigrants, with a like result; and the authors of the report to Parliament think the adoption of it will become general in the transportation of passengers, as it puts an end to all the difficulties occurring between the passengers and the officers of the ship, who are, of course, disposed to remain on the best of terms with those whom they carry.

"Why, then," asks the committee whose report is before us, "cannot or should not this principle of self-interest, as universal as the existence of man, be adopted in the transportation of passengers generally? As the law stood formerly, wages could not be demanded in case of shipwreck; and even now it must be made to appear that every effort has been made by the officers and crew to save the ship, before a claim for wages can be sustained."

"Is there any sufficient reason why similar responsibility should not be thrown upon those who have the entire management of the ship and the means of keeping it in proper condition? Should merchandise be on a better footing than human life? And if there be any propriety or justice in the old common law maxim, that freight was the mother of wages, why should not the safe delivery of the passengers at the port of destination be the foundation of any claim for passage-money? The committee can see none, and have therefore adopted the principle, and regarded it as, perhaps, the most important feature in the bill which they recommend to be enacted into a law. If they are not very much mistaken, the change here made in the relation between the shipper and the passenger will bring about an entirely new era in the history of passenger-ships; as it will be the interest of shippers to land their passengers in safety, so it will be their pleasure to provide their ships with whatever may be necessary to make them healthy and comfortable. Nor will there hereafter be the same carelessness in taking enfeebled, broken-down, or diseased passengers on board, as has heretofore existed; shippers will be cautious in seeing, at all events, that the individual, of whose life during the passage he is to become virtually the insurer, is not labouring under serious sickness at the time of embarkation.

The adoption of this principle will further have a most powerful influence in creating a rivalry between ships as to which is the cleanest and healthiest; passengers will always prefer those which have the reputation of being the most lucky, or, in other words, the cleanest, best managed, and affording the greatest comfort to those who embark in them. Nor will the ventilation, disinfection, &c. of these vessels hereafter require the interference of Congress; each ship-master will be as desirous to have his ship sweet, wholesome, and pleasant in its arrangements, as the hotel-keeper who desires to attract patronage by the superior neatness, cleanliness, and good order of his establishment. The owners of steamers on the noble rivers of our country, and stage-coaches on its great thoroughfares, require no legislation in regard to the spaciousness and airiness of their apartments, or the good driving and safety of construction of the other, simply because self-interest dictates to them the comfort and security of the travelling public as the best means to secure custom. So will it be with the owners of passenger-vessels when left to themselves; and they will provide for the health and comfort of the poor emigrant as well as of the millionaire, because, in doing so, they will protect their purses and their own interests."

The following is an outline of the measures recommended by the committee, after a careful examination of the subjects committed to their consideration:—

1. That a space be reserved on the upper deck, and kept clear, for the enjoyment of air and exercise by the passengers. If space cannot be allowed for the accommodation of all the passengers at once, they to be divided into squads, and take their exercise in turn at certain hours of each day should the weather permit. The committee very correctly remark, that, "nothing is more conducive to the cure of sea-sickness, and the preservation of the strength and good spirits of passengers at sea, than the necessity of moving about and becoming interested in what is going on around them. At the same time, the temporary absence of passengers from the sleeping apartments will afford an opportunity for cleansing them, and removing whatever may be offensive or injurious to health."

2. That the former limitation of two passengers to every five tons register be restored, but without diminishing the space allowed under the present law to each passenger, to prevent the crowding, that now takes place, of vast numbers in any one ship.

3. That during the winter the number of passengers be limited to one for every three tons, as the inclemency of the weather and consequent necessity of keeping the hatches closed obstruct ventilation, and prevent the passengers from taking exercise in the open air.

4. That no passengers be allowed to occupy the orlop deck, which, lying next the keelson, and entirely under water, must, it is very evident, be almost entirely deprived of air and light.

5. That the number of privies be increased, with separate ones for the females. As the committee remark, the present allowance of one to every hundred passengers, is totally inadequate. "Common decency would seem to indicate the propriety, or rather necessity, of having separate accommodations for females, whose health and comfort are often destroyed by the fear of exposure to the brutal remarks of the vulgar and obscene among the male passengers and the crew."

6. That, from the entire impossibility of hundreds of people, some of them feeble from sickness, doing their cooking in a proper manner, at a caboose four feet by one and a half in size, the provisions be cooked as well as furnished by the ship.

7. That, with a view to confer upon the captain ample power to maintain discipline, without being subject, as at present, to vexatious prosecutions, certain rules be adopted and published throughout the ship, the observance of which to be enforced by the master in case of resistance to his proper orders and authority; a statement of the facts to be entered at the time on the log, and signed by the surgeon—if any be on board—and mate, and read to the offender. The statement so made to be *prima facie* evidence of justification in all suits or complaints brought against him, or against any who aid in carrying out his orders.

8. That the ship be made responsible to the extent of the passage-money, in the event of death during the passage.

9. That a full and accurate return be made of the names and descriptions of passengers, and of the deaths on board.

These regulations, so far as they go, appear perfectly judicious, and, with the requirement that in every case the shipper shall become virtually the insurer of the passenger's life during the voyage, would no doubt be sufficient to put a stop to the excessive amount of sickness and mortality which has heretofore prevailed on board the majority of the emigrant vessels arriving in the ports of the United States.

This able report, drawn up, we believe, by the Hon. HAMILTON FISH, of New York, is in the highest degree creditable to the industry, intelligence, and philanthropy of this eminent statesman, and it is to be hoped that Congress will, at the earliest opportunity, carry out the measures recommended.

D. F. C.

ART. XIII.—*Puerperal Fever, as a Private Pestilence.* By OLIVER WENDELL HOLMES, M. D., Parkman Professor of Anatomy and Physiology in Harvard University. 8vo. pp. 60. Boston, 1855. Ticknor & Fields.

On the Induction of Puerperal Fever, by Inoculation, so called. By Z. PITCHER, M. D. Published at the request of the Detroit Medical Society. Peninsular Journal of Medicine for February, 1855.

THAT puerperal fever, so called, is not, strictly speaking, a disease peculiar to the parturient female; that, under circumstances, of by no means unfrequent occurrence, an affection, attended by the same general symptoms, and in which, after death, the same morbid lesions are found to exist, is liable to occur in the virgin and non-pregnant female, are positions now so well established that they can scarcely admit of a reasonable doubt.

The close relationship between puerperal fever and erysipelas is another fact we believe to be clearly and incontestably proved. As remarked, long since, by Dr. Gordon, of Aberdeen, the two diseases prevail, most generally, as concomitant epidemics. They begin at nearly the same time, keep pace with each other, and cease simultaneously, or nearly so. It would require more space than it would be convenient to devote to a single bibliographical notice to present even a brief abstract of the clear and positive evidence in support of this position, accumulated in the records of our profession.

There is still another fact in relation to this disease which appears to us to be as unquestionably established, as either of the preceding; namely, that puerperal fever may be induced by the introduction into the vagina or uterus, during or subsequent to parturition, by the hands of the accoucheur, a virus derived from patients affected with erysipelas, or from the bodies of those who have died of that disease or of erysipelas.

We are inclined to believe that another fact may be predicated of erysipelas and puerperal fever; namely, that both diseases are propagated by contagion; and that the two are capable of reciprocally producing each other. It is true, that the contagious character of erysipelas and puerperal fever is by no means so positively established as either of the preceding positions. When a disease, as is most commonly the case with both these, prevails as an epidemic or endemic, it is not always an easy matter to determine, with certainty, whether its spread is due to the general morbid condition of the atmosphere, which unquestionably prevails at the time, or to a poison, received either directly or indirectly, from the bodies of the sick. Still, there are on record a long series of well-authenticated facts which would seem very certainly to establish the contagiousness of both the diseases under consideration, and many have fallen under our own immediate observation in reference to the spread of childbed fever which, we confess, we should find very difficult to explain in a satisfactory manner without admitting the contagious character of the disease.

Dr. Holmes has collected and arranged all the leading facts in proof of this

side of the question adduced by different observers, previously to the year 1843. The character of these facts, and the sources from which the majority of them are derived, press them strongly upon the attention of those engaged in the practice of obstetrics, as well as of the profession generally. Their force cannot be evaded by the merely negative experience of any obstetrician, or by an attempt to disparage the professional standing, and the logical acumen, of those by whom they are recorded, or by underrating their opportunities for accurate observation. They demand a cautious, candid examination. If they can be fairly explained in accordance with the hypothesis that puerperal fever is a simple phlegmasia of the reproductive organs and peritoneum in the parturient female, and that it is not capable of being communicated from those labouring under the disease to any other female in childbed; let such explanation be given. Nay, if the supposed facts can be proved to be the result of inaccurate, or imperfect, or one-sided observations, let this be done; but, at the same time, let it be always borne in mind that it is with the asserted facts themselves we have to deal, and this fairly, candidly, and logically. If it can be shown that they are inadequate to sustain the doctrine they have been adduced to support, good and well. But if this cannot be done, they may be sneered at, their authors denounced as simpletons, and those who believe in their validity as dupes, but the facts themselves cannot by this means be erased from the records of our profession, nor until they are refuted, or the interpretation given to them shown to be erroneous, can their force be evaded.

The essay of Dr. Holmes, the title of which is placed at the head of this article, was first published some twelve years ago. The edition before us, although, so far as the essay itself is concerned, an exact reprint, "without the change of a word or syllable," of the first, we have esteemed worthy of a special notice, as well from the deep, the painful interest which every practitioner must feel in the important subject discussed in it, as from the very piquant introduction with which the author has thought proper to accompany it, and in which he has handled "with ungloved hands," such as deny the slightest validity to, or obstinately close their eyes against, all observations, no matter how extended the series, or respectable the sources from which they emanate, that militate against any hypothesis they shall think proper to put forth as truth; as well as those who most illogically attempt to set at naught positive by mere negative testimony, finished facts by such as are void of completeness.

The essay of Dr. Holmes is pronounced by Ramsbotham, to be "the best paper, in any language, with which he is acquainted, written to prove the highly contagious nature of puerperal peritonitis. A masterly performance, and well worth perusal by any sceptics on the subject." It demands the candid attention of every one who feels an interest, and what physician does not? in one of the most important questions connected with the etiology of one of the most important diseases to which the child-bearing woman is liable. Let no one who has not made himself master of the facts embraced in the essay under consideration, who has not subjected them to a careful and candid analysis with the view of testing their true bearing upon the question at issue, attempt to dogmatize in reference to the contagious or non-contagious character of puerperal fever. Ample as his own individual experience may be, the results of that experience must be tested by the positive experience of all other observers within his reach, before it can be made the basis of any general law in either pathology or therapeutics.

In his introduction, Dr. Holmes, after a reference to Dr. Meigs' recent work on childbed fever, remarks:—

"One unpalatable expression, I suppose, the laws of construction oblige me to appropriate to myself, as my reward for a certain amount of labour bestowed on the investigation of a very important question of evidence, and a statement of my own practical conclusions. I take no offence and attempt no retort. No man makes a quarrel with me over the counterpane that covers a mother and her new-born infant at her breast. There is no epithet in the vocabulary of slight and sarcasm that can reach my personal sensibilities in such a controversy. Only just so far as a disrespectful phrase may turn the student aside from the examination of the evidence, by discrediting or dishonouring the witness, does it call for any word of notice.

"I appeal from the disparaging language by which the Professor in the Jefferson School of Philadelphia would dispose of my claims to be listened to. I appeal, not to the vote of the Society for Medical Improvement, although this was an unusual evidence of interest in the paper in question, for it was a vote passed among my own townsmen; nor to the opinion of any American, for none know better than the professors in the great schools of Philadelphia how cheaply the praise of native contemporary criticism is obtained. I appeal to the recorded opinions of those whom I do not know and who do not know me, nor care for me, except for the truth that I may have uttered; to Copland, in his Medical Dictionary, who has spoken of my essay in phrases to which the pamphlets of American 'scribblers' are seldom used from European authorities; to Ramsbotham, whose compendious eulogy is all that self-love could ask; to the Fifth Annual Report of the Registrar-General of England, in which the second-hand abstract of my essay figures largely, and not without favourable comment, in an important appended paper. These testimonies, half forgotten until this circumstance recalled them, are dragged into the light, not in a paroxysm of vanity, but to show that there may be food for thought in the small pamphlet which the Philadelphia teacher treats so lightly. They were at least unsought for, and would never have been proclaimed but for the sake of securing the privilege of a decent and unprejudiced hearing.

"I will take it for granted that they have so far counterpoised the depreciating language of my fellow-countryman and fellow-teacher, as to gain me a reader here and there among the youthful class of students I am now addressing. It is only for their sake that I think it necessary to analyze, or explain, or illustrate, or corroborate any portion of the following essay. But I know that nothing can be made too plain for beginners, and as I do not expect the practitioner, or even the more mature student, to take the trouble to follow me through an introduction which I consider wholly unnecessary and superfluous for them, I shall not hesitate to stoop to the most elementary simplicity for the benefit of the younger student. I do this more willingly, because it affords a good opportunity, as it seems to me, of exercising the untrained mind in that medical logic which does not seem to have been either taught or practised in our schools of late, to the extent that might be desired.

"I will now exhibit, in a series of propositions reduced to their simplest expression, the same essential statements and conclusions as are contained in the essay, with such commentaries and explanations as may be profitable to the inexperienced class of readers addressed:—

"I. It has been long believed by many competent observers, that puerperal fever (so called) is sometimes carried from patient to patient by medical assistants.

"II. The express object of this essay is to prove that it is so carried.

"III. In order to prove this point, it is not necessary to consult any medical theorist, as to whether or not it is consistent with his preconceived notions that such a mode of transfer should exist.

"IV. If the medical theorist insists on being consulted, and we see fit to indulge him, he cannot be allowed to assume that the alleged laws of contagion, *deduced from observation*, in other diseases, shall be cited to disprove the alleged laws *deduced from observation* in this. Science would never make progress under such conditions. Neither the long incubation of hydrophobia, nor the protecting power of vaccination, would ever have been admitted, if the results of observation in these affections had been rejected as contradictory to the previously ascertained laws of contagion.

"V. The disease in question is not a common one; producing, on the average, about three deaths in a thousand births, according to the English registration returns which I have examined.

"VI. When an unusually large number of cases of this disease occur about the same time, it is inferred, therefore, that there exists some special cause for this increased frequency. If the disease prevails extensively over a wide region of country, it is attributed without dispute to an *epidemic* influence. If it prevails in a single locality, as in a hospital, and not elsewhere, this is considered proof that some *local* cause is there active in its production.

"VII. When a large number of cases of this disease occur in rapid succession, in one individual's ordinary practice, and few or none elsewhere, these cases appearing in scattered localities, in patients of the same average condition as those who escape under the care of others; there is the same reason for connecting the cause of the disease with the *person* in this instance, as with the *place* in that last mentioned.

"VIII. Many series of cases, answering to these conditions, are given in this essay, and many others will be referred to which have occurred since it was written.

"IX. The alleged results of observation may be *set aside*; first, because the so-called facts are in their own nature equivocal; secondly, because they stand on insufficient authority; thirdly, because they are not sufficiently numerous. But, in this case, the disease is one of striking and well-marked character; the witnesses are experts, interested in denying and disbelieving the facts; the number of consecutive cases in many instances frightful, and the number of series of cases such, that I have no room for many of them except by mere reference.

"X. These results of observation, being admitted, may, we will suppose, be *interpreted* in different methods. Thus the coincidences may be considered the effect of *chance*. I have had the chances calculated by a competent person, that a given practitioner, A, shall have sixteen fatal cases in a month, on the following data: A, to average attendance upon two hundred and fifty births in a year; three deaths in one thousand births to be assumed as the average from puerperal fever; no epidemic to be at the time prevailing. It follows, from the answer given me, that if we suppose every one of the five hundred thousand births of England to have been recorded during the last half century, there would not be one chance in a million million millions, that one such series should be noted. No possible fractional error in this calculation can render the chance a working probability. Applied to dozens of series of various lengths, it is obviously an absurdity. Chance, therefore, is out of the question as an explanation of the admitted coincidences.

"XI. There is, therefore, *some* relation of cause and effect, between the physician's presence and the patient's disease.

"XII. Until it is proved to what *removable condition* attached to the attendant the disease is owing, he is bound to stay away from his patients so soon as he finds himself singled out to be tracked by the disease. How long and with what other precautions, I have suggested, without dictating, at the close of my essay. If the physician does not at once act on any reasonable suspicion of his being the medium of transfer, the families where he is engaged, if they are allowed to know the facts, should decline his services for the time. His feelings on the occasion, however interesting to himself, should not be even named in this connection. A physician who talks about *ceremony*, and *gratitude*, and the *treatment he got*, surely forgets himself; it is impossible that he should seriously think of these small matters where there is even a question whether he may not carry disease, and death, and bereavement into any one of his families, as they are sometimes called."

The leading object of Dr. Pitcher's paper is to disprove the communicability of puerperal fever by any cause connected with the person of the accoucheur, or with that of any of the attendants upon the parturient female. The question is one of absorbing interest, and certainly demands a close and candid examination. We do not think, however, that Dr. Pitcher has met it fully and fairly. He has not investigated or collated the numerous and startling facts that have been adduced in support of the affirmative side, while he has attempted to establish the negative simply by a reference to certain generalities, which, though in themselves correct, have very little weight in the settlement of the point at issue.

Dr. Pitcher admits "that epidemic puerperal fever is an erysipelatous inflammation," that it "may be the occasion of abortion, of premature labours; that it is often developed before parturition, and still more frequently excited into activity by the accidents of labour." That in "a majority of cases" it is "essentially a traumatic erysipelas."

While he recognizes the frequent dependence of the disease upon an epidemic cause, he admits, also, that, within a certain limitation, its spread is due to a special contagion. The sphere of activity of this contagion is, however, he maintains, very limited; it may, he thinks, multiply the cases of puerperal fever among the inmates of the same lying-in ward, but beyond this is perfectly harmless. If, he remarks, the disease were of an unqualifiedly contagious or infectious character it should be communicable "to the pregnant as well as the parturient female, and why not as well to persons of either sex or any condition?"

Dr. Pitcher repudiates the idea of the propagation of puerperal fever "by the touch of the accoucheur." "How," he remarks, "so grave a disease can be transplanted by the touch of a person in perfect health, who has had no communication with the disease for days, and, perhaps, weeks, is as difficult for him to comprehend as that the royal touch should heal the scrofula."

Puerperal fever is either contagious or not contagious; if contagious, as Dr. Pitcher admits, it must be so absolutely; we have no knowledge of a contingent, occasional, or partial contagion. It is true, that the contagion of puerperal fever, like that of every other contagious disease, is active only within a certain circumscribed sphere, and the intensity of its action is augmented in close, crowded, and ill-ventilated apartments. We are to remember, however, that the contagious matter of disease is capable of adhering to the persons and clothing of individuals; to the bedding, and, also, it is probable, to the walls, etc., of rooms that have been occupied by the sick; in this manner smallpox and typhus fever have been communicated to persons who had never been in contact with patients labouring under these diseases, and in places remote from those in which the contagion was generated.

That the persons and clothing of individuals may become the media by which contagion is conveyed so as to produce disease, while the individuals themselves exhibit no indications of sickness, is proved by the occurrences at the Oxford assizes, in 1577, at those of Exeter and Taunton in 1586, and at those of the Old Bailey in 1736 and 1750; and by similar occurrences in various lazarettoes, barracks, hospitals, and transport and passenger vessels. How long the contagious matter may thus adhere to the person and clothing without losing its power to generate disease, it is impossible to say, in the absence of any accurate series of observations, undertaken to test the question. It is very certain that the contagion of puerperal fever is of a particularly adhesive and persistent nature. It has been found extremely difficult to expel it from the ward of a lying-in hospital in which the fever has prevailed. Even after the apartment has been vacated, thoroughly cleansed, and exposed to the freest ventilation for many weeks, immediately upon its being re-occupied it is no uncommon thing for the disease again to break out among its inmates, while no cases of it occur elsewhere.

Dr. Pitcher thinks that if puerperal fever is unqualifiedly contagious it should be communicable to the pregnant as well as the parturient female; to persons of either sex and every condition. Now we believe that it is actually thus communicable. We think that there can be no doubt that the affection denominated puerperal fever, and certain forms of erysipelas are in their true pathological character identical; generated by the same endemic or epidemic cause, and capable, by a contagion, of reproducing each other, reciprocally. During the prevalence of epidemic erysipelas, peritonitis is an ordinary concomitant in both men and women, while all the lesions, usually detected in cases of death from puerperal fever, are frequently met with in the virgin as well as in the pregnant and parturient female. We have reported, from a great variety of sources, cases, almost innumerable, of the production of puerperal fever by the contagion of erysipelas, while, on the other hand, instances are detailed on unquestionable authority; among others, we would instance those of Mr. Robert Storrs, of Doncaster, England; in which the contagious effects of puerperal fever on the male subject, as well as on non-parturient persons, was evinced; actual contact or close approximation with patients labouring under childbed fever, producing inflammation of the peritoneum or other serous membranes, accompanied by low fever, both in the male and female subject; erysipelas, local or

general, in both males and females; and typhus fever, with its various accompaniments, and in a variety of forms.

That the parturient female should be more subject to the influence of the epidemic constitution of the atmosphere productive of that peculiar pathological condition in which, we presume, both erysipelas and childbed fever essentially consist, and that, in her, the lesions should be chiefly detected in the pelvic viscera is not at all surprising.

But it is not only by contagion, in the proper sense of the term, that puerperal fever may be propagated, but it may also by direct inoculation; by the hands of the accoucheur introducing into the vagina or uterus a virus derived from patients labouring under severe forms of erysipelas, or in examinations of the bodies of those who have died of peritonitis, or phlebitis, or, probably, from the diseased secretions of the vagina in certain cases of childbed fever. The possibility of the induction of puerperal fever in this manner is very fully established by the experience of the physicians having charge of the lying-in departments in the general hospital of Vienna; where, according to the statement of Dr. Routh, by taking measures to prevent the possibility of inoculation in the manner referred to, the disease, which, previously, had prevailed to a fearful extent, has become of comparatively rare occurrence.

Dr. Pitcher having been led by numerous examples, most of which have occurred in his own practice, to adopt the belief "that puerperal fever is, in a majority of cases, essentially a traumatic erysipelas," the predisposition to, and approach of which may be foreseen and guarded against; consequently repudiates the idea of its propagation by the touch of the accoucheur, and he "holds it to be fallacious to resort to such extraordinary modes of explaining phenomena that may be accounted for, on the same principles that are applicable and daily appealed to, in speaking of the spread of other epidemic diseases, such as influenza, typhoid fever, and the dengue of the south, all admitted to be dependent upon a peculiar atmospheric constitution, and, in the opinion of many, spread by the influence of a special contagion."

It is true, that in numerous instances it is scarcely possible to determine with certainty whether the spread of puerperal fever is due to contagion, or is referable to a peculiar morbid condition of the atmosphere of a particular district, or town, or hospital; but, there are, not unfrequently, particular circumstances connected with the spread of the disease which cannot be accounted for upon, but are directly opposed to, the supposition that it is due, in every instance, to a general or local atmospheric cause; while, at the same time, facts are adduced which go absolutely to prove that its propagation is, in some instances at least, caused by contagion or inoculation. In this it resembles many of, if not all, the other known contagious affections, which may originate from epidemic or endemic causes, and be propagated by contagion, or communicated by inoculation.

D. F. C.

ART. XIV.—*A Treatise on the Practice of Medicine.* By GEORGE B. WOOD, M. D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania; President of the College of Physicians of Philadelphia; One of the Physicians of the Pennsylvania Hospital; One of the Authors of the Dispensatory of the United States of America, etc. etc. Fourth Edition. 2 vols. 8vo. pp. 852, 864. Philadelphia, 1855. Lippincott, Grambo and Co.

THE announcement of a new edition of Dr. Wood's treatise on the practice of medicine may be made in a very few words. We take it for granted that there are very few physicians in the United States who are not by this time perfectly familiar with the work, or, who have not formed for themselves a just estimate of its merits.

We know of no treatise which presents a more full and accurate synopsis of the present state of our knowledge, in regard to the pathology of the leading ailments incident to the human organism: no one that can, with greater safety,

be placed into the hands of the student as his guide to correct views in reference to the nature, causation, symptoms, and march of disease, and to sound practical directions for its treatment; no one that may be consulted, as occasion demands, by the practitioner, with greater profit and satisfaction.

Devoid of all pretension in his teachings, Dr. Wood's sole object in the preparation of the present treatise was evidently "to present, along with the results of his own personal observation, experience, and reflection, in reference to the practice of medicine, such a view of this important department of medical science, as fairly to represent its general condition at the time." And this object, all who have studied the work with candour and attention, will acknowledge, has been kept constantly in view by the author, and very successfully attained.

We meet, in its pages, with no straining after the questionable character of originality by the enunciation of novel and startling theories, or of attractive, but misleading generalities, nor any attempt to build up an exclusive plan for the treatment of disease, by rejecting with contempt, or ignoring the accumulated and concurrent experience of his predecessors or contemporaries. But, in a concise, perspicuous, unornate style, Dr. Wood delineates the broad outlines of the theory and practice of medicine, as recognized and illustrated by the most reliable and recent of the authorities in our profession.

In the present edition is incorporated a faithful digest of whatever may be esteemed true and useful, that has been developed since the appearance of the former one, in reference to either of the subjects embraced within the general scope of the treatise; it will be found, therefore, fully brought up to the present time.

D. F. C.

ART. XV.—*The Principles and Practice of Obstetric Medicine and Surgery in Reference to the Process of Parturition*. With sixty-four plates, and numerous wood-cuts. By FRANCIS H. RAMSBOTHAM, M. D., Fellow of the Royal College of London; Obstetric Physician to the London Hospital, and Lecturer on Obstetric and Forensic Medicine at the London Hospital Medical College, etc., etc. A new American Edition, revised by the Author. With Notes and Additions, by WILLIAM V. KEATING, M. D., A. M., Lecturer on Obstetrics and Diseases of Women in the Philadelphia Medical Association, Physician to the St. Joseph's Hospital, Member of the American Medical Association, Fellow of the College of Physicians, Member of the American Philosophical Society, etc. etc. Τα μὲν ἄλλα καὶ πόλεμος καὶ μεταβολὴ τύχης ἀνάλωσαν. ἡ Τέχνη δὲ σώζεται. 8vo. pp. 648. Philadelphia, 1855, Blanchard and Lea.

THE work of Dr. Ramsbotham has been now for many years before the medical public, and has received the almost unanimous approval of the professional periodical press for the clear and accurate demonstrations it imparts of the doctrines of obstetrical science, and the sound practical precepts it inculcates. It cannot fail always to command a high rank among the numerous treatises which have, of late years, been presented as guides to the student in the acquisition of the necessary elementary knowledge of the principles and practice of obstetrics, and as books of reference for the practitioner who desires to refresh his memory on the leading points connected with the rules and practical details of his profession as recognized by the best authorities of the day.

Sufficiently full in its details, while in its style it is clear and concise, it is, at the same time, plain and unpretending, but sound and explicit in the practical instructions it inculcates, and cautious in the exposition it presents of the particular conditions under which manual or instrumental interference is demanded in the conduct of labor, with reference to the safety of the mother or child, or of both. Every important point connected with the anatomy of the organs concerned in child-bearing, and with the changes they undergo during

gestation—the several positions of the fœtus in utero, the mechanism of labour, and every subject of practical importance connected with obstetrics, are illustrated by a series of accurate and well executed engravings, admirably calculated to render them clear to the comprehension, and, at the same time, to fix them upon the mind of the student.

As an exponent of British Midwifery, the work of Dr. Ramsbotham is among the best extant, and, for the admirable synopsis it presents of the several subjects embraced within its general plan; the good common sense which pervades its doctrines and its precepts, and the plain, unpretending practical knowledge it inculcates, it may be safely recommended as one admirably adapted for the use of the student and junior practitioner.

The present edition has received the especial revision of the author with the view to its publication in this country, while the Editor has endeavoured to “adapt the work more fully to the wants of the American Practitioner,” by embodying, as far as possible, the peculiar views of the most eminent of our own authorities in obstetrics.

The additions he has made, “refer chiefly to the inclined planes of the pelvis; generation; signs of pregnancy; fœtal circulation; mechanism of labour; employment of anæsthetics; use of ergot; American forceps and rules for their application; views of American Obstetricians on puerperal fever; Professor Clark’s treatment of puerperal fever; rupture of the uterus; causes of abortion, etc.”

These additions are, upon the whole, judicious, and enhance the value of the work to the American student and practitioner.

In regard to its mechanical execution, the present edition is worthy of all praise. The typography and illustrations are of the best style as regards accuracy as well as elegance. The volume will form a beautiful, as it unquestionably will a useful addition to the library of the American obstetrician.

D. F. C.

ART. XVI.—*On Criminal Abortion: a Lecture introductory to the Course on Obstetrics and Diseases of Women and Children in the University of Pennsylvania, Session of 1854-’5.* By HUGH L. HODGE, M. D. 8vo. pp. 20.

WE have read, with no little satisfaction, this very excellent introductory lecture. It is dignified in manner, high-toned in its sentiments, and inculcates, with becoming earnestness, the obligation of the practitioner of any and all the branches of the healing art, not only to perform with skill and faithfulness his merely professional offices, but to observe strictly towards his patients, and society generally, a code of the purest ethics. The lecture, throughout, is one well calculated to impress upon the minds of those who are preparing themselves to enter the ranks of our profession, to practise in either of its departments, a sense of their deep responsibilities and incumbent moral obligations.

It is, however, to the leading theme of the lecturer—criminal abortion—that we would particularly call attention on the present occasion.

In reference to this execrable practice, which, we have reason to believe, is pursued in our very midst to an extent of which many have no conception, Professor Hodge speaks with a plainness of language and warmth of indignation that does him much credit. If his language and feelings, in this respect, shall be responded to by the profession generally, throughout the breadth and length of our land, we shall, at least, absolve ourselves from all participation in the guilt incurred by those who, having gained admittance into our ranks, are willing, for a price, to prostitute themselves as panderers to vice by the criminal induction of abortion; while, at the same time, we may be the means of diminishing the frequency of the crime, if we shall be unable entirely to suppress it.

There can be no doubt that, in a proper ethical, if not in a legal sense, Judge King, of Philadelphia, was strictly correct when he declared “every act of abor-

tion to be murder, whether the individual perpetrating such act intended to kill the woman, or merely feloniously to destroy the fruit of her womb." It is certainly "a base and unmanly act"—"a crime against the natural feelings of man—against the welfare and safety of females—against the peace and prosperity of society—against the divine command, 'Thou shalt not kill.' It is murder.

"It is in vain to evade this conclusion. As far as human investigation has gone, or probably ever will go, in penetrating the mysterious functions of generation—as far as the light of reason or the torch of Revelation has elucidated the subject, there can be no reasonable doubt that human existence, corporeally and spiritually, commences, not with the birth of the foetus and the first inspiration, but at conception, when the germs furnished by both parents are quickened into life." By what strange perversion of the moral sense has mankind come so generally to view the intentional destruction of the life of the foetus prior to birth as a venial offence, and to render this form of murder so prevalent among the most intelligent, refined, moral, and Christian communities?

"We blush," says Dr. Hodge, "while we record the fact that, in this country, in our cities and towns, in this metropolis, where literature, science, morality, and Christianity are supposed to have so much influence—where all the domestic and social virtues are reported as being in full and delightful exercise—even here individuals, male and female, exist, who are continually imbruing their hands in the blood of unborn infants; yea, even *medical* men are to be found who, for some trifling pecuniary recompense, will poison the fountains of life, or forcibly induce labour, to the certain destruction of the foetus, and not unfrequently of its parent.

"So low is the moral sense of the community on this subject, so ignorant are the greater number of individuals, that even mothers, in many instances, shrink not from the commission of this crime, but will voluntarily destroy their own progeny, in violation of every natural sentiment, and in opposition to the laws of God and man. Perhaps there are few individuals in extensive practice as obstetricians, who have not had frequent applications made to them, by fathers and mothers of unborn children (respectable and polite in their general appearance and manners), to destroy the fruit of illicit pleasure, under the vain hope of preserving their reputation by this unnatural and guilty sacrifice.

"Married women, also, from the fear of labour—from indisposition to have the care, the expense, or the trouble of children—or some other motive, equally trifling and degrading, have solicited that the embryo should be destroyed by their medical attendant. And when such individuals are informed of the nature of the transaction, there is an expression of real or pretended surprise that any one should deem the act improper, much more guilty; yea, in spite even of the solemn warning of the physician, they will resort to the debased and murderous charlatan, who, for a piece of silver, will annihilate the life of a foetus, and endanger even that of its ignorant and guilty mother.

"This low estimate of the importance of foetal life is by no means restricted to the ignorant, or to the lower classes of society. Educated, refined, and fashionable women—yea, in many instances, women whose moral character is in other respects without reproach—mothers who are devoted, with an ardent and self-denying affection, to the children who already constitute their family—are perfectly indifferent respecting the foetus in utero. They seem not to realize that the being within them is indeed *animate*—that it is in verity a *human being*, body and spirit—that it is of importance—that its value is inestimable, having reference to this world and the next."

"These facts are horrible, but they are too frequent and too true. Often, very often, must all the eloquence and all the authority of the practitioner be employed; often he must, as it were, grasp the conscience of his weak and erring patient, and let her know, in language not to be misunderstood, that she is responsible to her Creator for the life of the being within her.

"After this exposition," concludes the lecturer, "and the details which have been given, and especially in view of the influence which medical science must exert on these questions, it seems hardly necessary to repeat that physicians—medical men—must be regarded as the guardians of the rights of infants. They alone can rectify public opinion; they alone can present the subject in such a

manner that legislators can exercise their powers aright in the preparation of suitable laws, that moralists and theologians can be furnished with facts to enforce the truth on this subject upon the moral sense of the community, so that not only may the crime of infanticide be abolished, but that criminal abortion be properly reprehended, and that women, in every rank and condition of life, may be made sensible of the value of the embryo and fœtus, and of the high responsibility which rests on the parents of every unborn infant."

We can indorse, to their fullest extent, the statements and sentiments comprised in the foregoing extracts from the lecture before us.

No right-minded person can view the induction of abortion, to subserve a selfish or wicked purpose, in any other light than as a crime tantamount to that of murder, demoralizing in its influence upon both sexes, as well as decidedly prejudicial to the interests and happiness of society. And they must be strangely inattentive to what is passing around them, who are not aware of the extent to which criminal abortion is practised in almost every community. Our newspapers teem with advertisements, couched in scarcely equivocal language, proffering to those who would avail themselves of them, the services of the professed abortionist.

It is, indeed, surprising with what impudence—with what an entire contempt of every moral principle and obligation, men who profess themselves to be practitioners of "the healing art," and who, perchance, are the unworthy possessors of a collegiate diploma, proclaim themselves to be ready, for a trifling compensation, to induce abortion. We were recently shown a letter containing an order for such medical books to be sent to the writer, as would prove useful to him in facilitating his practice as an abortionist; or, as he more hypocritically expressed it, would instruct him in the best and safest means for relieving females from the exposure and disgrace that an unfortunate pregnancy would necessarily entail upon them.

If it be a fact that the laws of Pennsylvania are insufficient to remedy the evil—if they consider it rather in the light of a misdemeanor than as a crime, it is high time that our Legislature should be called upon to make such enactments as will insure the conviction and adequate punishment of all who are concerned in the perpetration of this most demoralizing form of murder.

D. F. C.

ART. XVII.—*Nature in Disease, illustrated in various Discourses and Essays: to which are added Miscellaneous Writings, chiefly on Medical Subjects.* By JACOB BIGELOW, M. D. Boston, 1854: pp. 391, 12mo.

THE greater number of the essays contained in this work were published at various periods, between 1812 and the present time, and the more important of them have already been noticed in this Journal. We are glad to meet with them again in their collected form, because they are interesting in themselves, and because they are almost the only record of their author's medical opinions. His eminent position as a teacher, his large experience as a practitioner, his clearness of intellect, and his exemplary rectitude of character, give to these memorials of his professional life an absolute and exceptional value. They are peculiarly interesting to us, of a more southern sphere, as illustrating a mode of thought upon medical subjects which strongly contrasts with our own. We have more faith in the resources of our art, and cannot persuade ourselves that demonstrations of the efficacy of medicine are to be obtained by mathematical processes alone. We trust more to written tradition, and cannot easily be dissuaded from relying upon methods which bear the stamp of catholicity. It is well, perhaps, that the expansiveness of our faith should sometimes be checked by the chill breezes of northern logic. This work has such a tendency. If its style is cold, it is also clear, and occasionally coruscates with brilliant thoughts; if it is unadorned, it is charming in its simplicity; if it is somewhat rigid, it satisfies the reason by its precision and the completeness of its ideas. It is,

therefore, not an ordinary book, and it is one which we commend to all who have a relish for a pure and high tone in the discussion of medical questions.

Of the principal essay in the work, that on "Self-Limited Diseases," we might have much to say in criticism did our limits suffice; but we must restrict our remarks to a few points which appear especially to call for them. A self-limited disease is defined to be "one which receives limits from its own nature, and not from foreign influences;" and this definition agrees with the one generally given. But while in its ordinary acceptation the term is applied only to certain continued fevers, such as the eruptive fevers and typhoid fever, our author would seem to extend it to the whole nosological catalogue, except, perhaps, functional diseases of the nervous system. For instance, he remarks, "if we divide with a cutting instrument the cellular and muscular substance, we produce a self-limited disease." Surely this is equivalent to saying, that all diseases are self-limited, except those just alluded to, of the nervous system. In another place it is said: "We may suspect those complaints to be self-limited in which it is observed that the unwary and the sceptical, who neglect to resort to remedies, recover their health without them." Unless we greatly err, this passage means that all diseases which get well without treatment are 'self-limited,' and, consequently, all diseases except hydrophobia and one or two others deserve this epithet. Surely it were hardly worth while to invent a phrase which has so little in it that is distinctive.

In reviewing individual diseases, to show how slight an influence on their course and issue can be exerted by medicine, the author takes his illustrations almost exclusively from the class of continued fevers and of spasmodic affections, and intimates that art has little or no control over them. Such is not the verdict of universal experience, which asserts that the issue of these diseases is very often changed from fatal to propitious, and that the sufferings of those who endure them are greatly lessened by treatment. Are not whooping-cough, erysipelas, epilepsy, mania, &c., which he enumerates in the dark catalogue of diseases over which medical art has no certain control, are these not often, very often, cured?

"It may, perhaps, appear," says Dr. B., "that the views which have now been taken of the power of medicine in so large a class of diseases, are gloomy and discouraging, and that an unworthy tribute is paid to the labours of those physicians who have patiently studied, and arduously acted, for the benefit of humanity." Unquestionably they *are* calculated to chill all ardour in the pursuit of medicine, and make those who have consecrated themselves to it as to a crusade against suffering and death, feel that they have been the dupes of their own hearts and the victims of an imposture, an imposture which, marvellous to relate, has possessed vitality enough not only to have outlived every earthly thing contemporaneous with itself, but also to have grown to such strength and stature as insure its immortality.

The essay which presents so discouraging a view of medicine was written in 1835, and doubtless reflected in part the sceptical sentiment which had been fostered by the negative therapeutics of Broussais, and which the general attention, then turned to pathological anatomy, allowed to reach its full development. While a small portion of the profession, carried to the very extremity of scepticism, fell into the void of homœopathy, the great body retraced their steps and devoted themselves to the study of medicines and their relations to the animal economy in health and disease, so that the last fifteen years have done more to extend and define therapeutics than any equal period of medical history. That our author should have felt the latter, and salutary influence, as he also experienced the former and baleful impulse, is evident, from his essay "On the Treatment of Disease." It is true he states in his preface, that subsequent observation has not tended to shake his earlier opinions; but in the paper just referred to there is evidence enough of his occupying a more hopeful position than before. Thus he is careful to point out "many cases which are, for the most part, capable of being arrested or broken up by the interposition of remedies." And, "in other cases which cannot be thus arrested, but which, from their nature, must run a destined course, it is generally admitted that the safety of the patient may be promoted, or perhaps the duration of the case abridged,

by remedial treatment." And he goes on to indicate many occasions which illustrate the curative powers of medicines. He no longer includes in his definition of self-limited diseases all except the nervous, but establishes another category, that of the *curable*. In it we now find included those affections which "are under the control of remedies, so that they can be arrested, or abridged in duration." The powers of the antiphlogistic regimen in acute inflammatory diseases; of opiates, antispasmodics, and tonics, in spasmodic diseases; of specific remedies in gout, intermittent fever, scabies, syphilis, goitre, &c. &c., are fully dwelt upon and admitted, and it is only on approaching the class of malignant and organic diseases, that a feeling of regret is expressed on account of the finite powers of the healing art. Thus we have the satisfaction of seeing this eminent and influential teacher chasing away the despondency and discouragement inspired by his earlier instructions, and bidding us to be active and vigorous in the present, and hopeful for the future. His counsel comes to us with all the more emphasis and weight, because we perceive that it is the suggestion neither of habit nor of a dogmatizing spirit, but the conclusion of a logical mind, and that it is reached as a necessary consequence of rational experience.

The "Practical Views on Medical Education," are familiar to most of our readers in their present form, or in that which was originally presented to the American Medical Association. In this paper, it is said: "Custom in this country has generally fixed the limits of these things (lectures) at about four months. A comprehensive and judicious course, confined to the enforcing of necessary points, is far more profitable than a more discursive course to a wearied and diminishing audience." Sentiments like these, from so distinguished a source, excited surprise when they were first published. Their repetition can now only inspire regret. If it should be said that the plans of those who proposed a more extended plan of medical education have partially failed, let them enjoy the honour of having seen the necessity of rendering our collegiate instruction more extensive and more thorough, and let the profession at large bear the disgrace of being so far inferior to their guides as to desert them in their noble and self-sacrificing effort.

In the essay "On the Medical Profession," there is a very apt commentary, though certainly an unintentional one, upon this reckless haste in hurrying through the course of undergraduate studies. It is there said: "When, as it often happens in our community, narrow circumstances require that a young man should live by his own exertions, this state of things, instead of being a motive that he should crowd himself prematurely into the ranks of the profession, encumbered with debt, and bare of acquirements and of means, is rather an imperative reason that he should at once begin by resolving to devote twice the customary number of years, if necessary, to the double purpose of keeping himself in an independent position, and of placing himself at length, in point of maturity of knowledge, on a par with his more favoured competitors."

In dealing with "Quackery," the author strongly advises that it be let alone; and we most heartily subscribe to his opinion, which is thus expressed:—

"It exists in every walk and occupation of life, by the exercise of which men procure bread. The pettifogger in law, the Millerite lecturer in theology, the demagogue in politics, the system-monger in education, and the wonder-worker upon the brains and bowels of infatuated audiences—what are all these but quacks moving in their respective spheres, and fattening upon the credulity of dupes. A certain portion of mankind are so constituted that they require to be ridden by others, and if you should succeed in unhorsing a particular impostor, it is only to prepare the saddle for a fresh and more unflinching equestrian. It is not good policy to say or to write too much in regard to the pretensions of impostors. A celebrated author observes that 'many a popular error has flourished through the opposition of the learned.' By throwing the gauntlet at an insignificant man, you at once raise him to the dignity of being your competitor, and acknowledge him as a 'foeman worthy of your steel.'"

The article on the "Treatment of Injuries by Fire," is interesting as showing

¹ McIntosh.

how nearly alike are the results obtained by the stimulant and by the sedative method. The author remarks, "should any one come forth as the advocate for a *negative mode* of treating burns, which should consist in letting them alone, or in leaving the process to nature; there is no doubt that in due time he would be able to collect a sufficient number of apparently satisfactory cases to answer all his purposes." This prediction, made in 1812, has been amply verified by the results of using raw cotton in the treatment of burns.

The paper which treats of the causes of "metallic tinkling" in pneumothorax, is one of the most interesting contained in the volume, and fully demonstrates that "the immediate or exciting causes of metallic tinkling is a forcible or sudden disturbance of the liquid in a vibrating cavity." This may be produced by the passage of bubbles through it, or by the agitation of the whole mass of the liquid itself.

In his paper "On the *Mucuna Pruriens*," the author calls in question the anthelmintic powers of this medicine; for he found by experiment that when the spiculæ were soaked in warm water for about ten minutes they lost their irritating property. He also observed that, although these spiculæ produce violent irritation when applied to the skin, they may be rubbed on the inside of the lip or tongue, without exciting any sensation beyond the ordinary mechanical stimulus.

Dr. Bigelow has collected some very interesting cases of poisoning from eating the flesh of the American partridge or Ruffed Grouse. The symptoms he mentions are vertigo, loss of sight, tinnitus aurium, and, in bad cases, loss of the power of sensation and voluntary motion. Respiration is slow; the heart and pulse are slow, feeble, and irregular; the surface cold, and the complexion pale or livid; and sometimes there is syncope. Nausea, vomiting, and abdominal pain are the symptoms affecting the digestive organs. Dr. B. rejects the popular notion of the flesh of the bird having been rendered poisonous by eating laurel berries or leaves, because these do not constitute its food, and because in the quantity taken they would be incapable of producing the effects we witness. He is disposed rather to attribute these effects either to the bird being diseased at the time of its death, to some slow chemical change, not putrefactive, which its flesh undergoes, or to the idiosyncrasy of the patients. It must be confessed that these explanations are not very satisfactory.

In the chapter on "Coffee and Tea," and in that "On the History and Use of Tobacco," we are entertained with an account of the extravagant eulogies and preposterous vituperations which all of these substances met with when first they were introduced into use. The fanatical muftis of the East are the prototypes of the reverend brethren of the West, who would drive the juice of the grape from the sanctuary, and abolish one of the holiest of religious rites rather than submit their crooked minds to be straightened on the anvil of common sense. It seems that "Coffee was introduced into Mecca, Medina, and Cairo, about the middle of the fifteenth century, and two coffee-houses were opened at Constantinople in 1554. Both at Cairo and in Turkey it had to encounter political and religious opposition; the dervises affirmed that roasted coffee was nothing but a *coal*, and that the eating of *coals* was forbidden by the laws of the prophet. So that the coffee-houses were obliged to be shut up until 'a more sensible mufti' succeeded in convincing the people that roasted coffee was not a coal, upon which they were again opened."

In another paper, the author attempts to convince his townsmen that the Cochituate water, which supplies Boston, is not made poisonous by passing through leaden pipes. The extreme excitement which disturbed the citizens of that town, as well as of New York, a few years ago, has, we believe, died away, and we hear no more of those terrible cases of lead poisoning which frightened the public from its propriety, and gave occasion to the publication of several copious and learned treatises upon the subject. We quite agree with the author that "from the present state of our knowledge, we are authorized to conclude that the insurance on a citizen of Boston, New York, Philadelphia, or London, against lead-colic, is probably worth *much less* than his insurance would be on a voyage across the Atlantic, or on a railroad for twenty miles."

A. S.

ART. XVIII.—*A Monograph on the Pathology and the Rational Treatment of Infantile Laryngo-Tracheitis or Croup.* By E. R. PEASLEE, A. M., M. D., Prof. of Anatomy, etc. From the American Medical Monthly. 8vo. pp. 32. New York, 1854.

THIS is a very able monograph. The pathological views advanced in it, when carefully and candidly examined, will, we are persuaded, be found to be, in the main, correct. They coincide, certainly, with all the facts deduced from recent observations, and divest one of the most important diseases incident to childhood of the mystery in which its true character has so generally been supposed to be involved, by indicating its analogies with other morbid conditions occurring both in the infant and the adult. Croup is an affection which has, unquestionably, nothing specific in its nature. It is simply a laryngo-tracheitis, and, as such, stands in close relationship to, or, rather, is identical with, this form of inflammation, whether it occur in the adult or infant—whether it run its course, favourably or unfavourably, with or without the production of a false membrane. Croup consists essentially in an inflammation of the mucous membrane of the larynx, extending, in most cases, into the trachea, and often into the bronchi likewise. The false membrane, when it does occur, as well as the spasmodic symptoms—the laryngismus—are both merely accessory symptoms, and altogether dependent upon and secondary to the inflamed condition of the laryngeal and tracheal mucous tissue, which alone constitutes the disease. We saw, during the last year, a number of cases of acute laryngitis, occurring chiefly in young persons, in which no false membrane was formed; and yet, in their general symptoms, these cases could not be distinguished from attacks of what has been termed genuine croup.

The source from which the false membranes are in all cases derived, and the manner in which they are developed are thus, and we believe correctly, explained by Dr. Peaslee.

“A false membrane is, in all cases, formed from the *plasma of the blood*, and which consists principally of its fibrin. It was remarked, several years ago, by Dr. Alison, that there is, in all inflammations, a *tendency* to the exudation of plasma from the vessels of the inflamed part. We maintain that in all inflammations an exudation actually does occur. It is, however, sufficient for our present purpose, that the plasma is exuded from the engorged vessels of the part, and upon the surface, in all cases of inflammation of membranes, at least. Nobody doubts this assertion in regard to serous membranes; and even the purulent discharge (often called a secretion) from an inflamed mucous membrane, is known to be derived from the plasma exuded, by a degeneration of the exudation-corpuscles into pus cells. Moreover, if an actual false membrane forms in croup upon the mucous surface, it is equally derived from the same source.

“1. The first step towards the formation of a false membrane in croup, therefore, is the exudation of plasma upon the surface of the mucous membrane, as explained, in consequence of the laryngitis. The amount of the exudation will depend upon several circumstances; of which, the state of the blood, the extent of the inflammation, and the vascularity of the part, are the most important. In membranous croup, the exudation is most abundant, and, therefore, the resulting membrane is thickest, on the surface where the mucous follicles are most abundant; and this, in the trachea, is the case with the posterior wall. The follicles are merely an *inversion* of the mucous membrane; so that here is a great amount of free surface, and a vast number of small blood-vessels inclosed within a very small space. In other parts the surface is much more smooth, the vascularity less, and thus the exudation, as well as the resulting membrane, is thinner. It has already been asserted, that it is not the secretion of these follicles that becomes organized, nor is the exudation ‘albuminous.’

"2. The second step in the development of a false membrane is the organization of the exuded plasma. But it is very well known that, in the majority of instances, this never takes place, even on serous membranes; and, therefore, no false membrane is formed.

"In fact three entirely different results may ensue to the layer of plasma exuded upon the free surface of a membrane in consequence of inflammation; provided it is allowed to remain in contact with it.

"1st. It may be entirely reabsorbed. 2d. It may be converted into pus; exudation-corpuscles being formed in it, and these becoming, subsequently, degenerated into pus cells. 3d. It may become fibrillated—coagulated—and thus form a false membrane; which, subsequently, may become more highly organized, and, in case of serous membranes, even vascular, at last.

"Now, either of these results is possible, in case of inflammation of both serous and mucous membranes. In case of the latter, however, the part affected may be so situated as to be brought into contact with foreign substances—as is the case with the mouth, pharynx, and other parts of the alimentary canal—and the exudation may thus become at once detached; and in that case, neither of the three results above mentioned can ensue. Moreover, the opportunities for reabsorption on mucous membranes are very slight indeed, and often null.

"To illustrate these results in case of a serous membrane: In *pleurisy* the exudation being reabsorbed, perfect recovery takes place; if it becomes organized, false membranes and adhesions are the result; or if it is converted into pus, empyema is the result.

"Now, the circumstances favourable to the organization of the plasma are: 1st. Its perfect contact with the living tissues; 2d. Perfect rest of the part, or maintaining the required contact, and for the necessary time also; 3d. Smoothness of surface, as also subservient to the first condition.

"On the other hand, the following circumstances are unfavourable to organization: 1st. Motion of the part affected; 2d. Contact of foreign substances; 3d. Irregularity of surface. But there are certain other conditions of the exudation itself, also, which affect the result. The former is more liable to become organized, and the membrane is more perfectly formed, in proportion—1st. To the amount of fibrin contained in it; 2d. to the slowness—amount of time allowed—of the fibrillation.

"Here we should, however, remark, that all the formative processes are in much less time completed in the young than in the adult animal; and it is in accordance with this ultimate fact, that Jurine and other experimenters could artificially produce false membranes in the air-passages of young animals alone. In adult animals, the exudation would become detached before its organization was complete.

"Now, if we compare the probabilities that an exudation upon a serous membrane will become organized with those that the same result will occur upon a mucous membrane, we find the former allows—1st. Of a far more perfect contact; since it is smooth, and comparatively or entirely at rest—for example, remark the immobility of the ribs in respiration, in *pleurisy*. 2d. It excludes the exudation from the air and from foreign substances. 3d. Allows time for the perfection of the process of organization. 4. The fact, also, that contact of the exuded plasma with the living tissues occurs on both its surfaces at the same time—being between the two layers of the serous membrane—must not be omitted here. Nor, 5, must we fail to add, that in case of inflammations of serous membranes, the blood, and therefore, probably, the exudation also, contains a greater amount of fibrin than exists in it in inflammations of mucous membranes.

"Mucous membranes, on the other hand, are opposed to the serous, in all the respects just mentioned. 1. Having a conoidal instead of a delicate, scaly epithelium in most parts—for example, over the whole respiratory passages, and the alimentary canal, from the cardiac orifice of the stomach to the sigmoid flexure—they do not allow of so perfect a contact of the plasma with the living tissue of the membrane itself. 2. They afford contact only on one side of the plasma. 3. In most parts, the plasma is exposed to the air and the con-

tact of foreign substances. 4. There is more motion of the part. 5. All these circumstances afford less time for the organization to occur; so that it may fail of being completed if commenced. 6. And, finally, the plasma itself is less organizable than in the case of inflammation of serous membranes.

"We have no difficulty, then, in accounting for the fact that false membranes occur very much more frequently on serous than on mucous membranes. Nor is there any in accounting for the fact that, in certain cases, they are formed in the latter also, as well as in the former.

"False membranes have been known to form in cases of inflammation of the rectum, the vagina, the uterus, the nasal passages, the pharynx, larynx, trachea and bronchial tubes; and in all these parts certain peculiarities exist, more favourable to the organization of the plasma than the mucous membrane in other parts presents. The rectum and vagina have a scaly epithelium, and are comparatively at rest—the rectum, even, being void of fæces and of motion, except during the act of defecation. The uterus, also, allows of still greater rest of the exuded plasma, though it has a conoidal epithelium. The whole extent of the air-passages is provided with a conoidal and a ciliated epithelium; but it has no foreign body in contact with it, except the air; and in this respect, and in point of motion of the surface, has the advantage of the alimentary canal above the rectum; where false membranes do not become organized. In children, however, there is less motion still; and they also requiring less time, we should expect organization to occur more frequently in them than in adults."

Instead of the foregoing long extract from the monograph of Dr. Peaslee, we might have contented ourselves by giving, in a brief synopsis, the leading views embraced in it. But, as the presence of a false membrane in cases of croup appears to have been the chief stumbling-block in the way of arriving at a correct understanding of the true pathology of the disease—while upon it has been attempted to be based the evidence of the specific character of the malady, we were desirous of presenting at length, and in his own words, the author's exposition of the source and mode of development of false membranes as a result, under certain circumstances, of simple inflammation of the tissues upon which such membranes occur.

Returning to the subject more immediately under consideration—the pathology of croup—Dr. Peaslee remarks:—

"In cases of actual laryngitis with false membrane, or membranous croup in children, the exuded plasma becomes to some extent organized, *fibrillated* at least. But can we suppose that this result occurs in every case of laryngitis in the early years of life? If the plasma is often absorbed, and also frequently converted into pus, in case of inflammation of a serous membrane, as in pleurisy, can we doubt that either or both these results often occur in cases of inflammation of any and of all mucous membranes? And to say that croup is a peculiar or *specific* inflammation, in order to account for the formation of the false membrane, would, for the sake of consistency, compel us also to invoke a *special* inflammation in those cases of pleurisy in which adhesions occur, while a *common* inflammation will answer if recovery occurs by absorption, or if empyema ensues. Our ideas, then, of the pathology of croup, so far as the answer to the first inquiry has established them, are these:—

"1st. An inflammation of the larynx, extending into the trachea, occurs; offering in its essential nature nothing different from any other case of inflammation of the same parts, either in the infant or the adult. It is generally preceded, in both infants and adults, by congestion and irritation, and therefore by catarrh.

"2d. An exudation of plasma occurs on the inflamed surface, as in the adult, this being most abundant in the trachea on the posterior wall, for reasons already given.

"3d. This exudation may be disposed of, in at least two ways, provided it is not at once removed, as it generally is, in adults but not in infants, by coughing; reabsorption probably very seldom occurring in this disease, though it is not impossible. *a.* It may become degenerated into pus—purulent matter—and thus, of course, at once be detached, which is the most common result.

b. It may become organized into a false membrane. This is more probable if the blood is rich in fibrin, as in a plethoric child; if there is but little cough, an adult generally expelling it thus; and if time is allowed for its development, less being required in the child than in the adult.

"4th. Croup is, therefore, merely a laryngo-tracheitis in infants and children, and offers nothing essentially different from the same inflammation in adults. The exudation in case of adults is, however, usually at once ejected by coughing, or in the form of purulent matter; while the liability to its organization in infants is greater; though, after all, a comparatively rare result, considering the whole number of cases, for the reasons before mentioned.

"5th. Practically, therefore, as well as pathologically, we cannot say with Bouchat, '*without a false membrane, croup does not exist.*' This membrane never exists till the inflammation—the essential element of the disease, as we believe—has preceded, and has produced the exudation of plasma, as before shown. No sooner does the catarrhal irritation merge into inflammation, than the plastic lymph is thrown out; and this *inflammation and its accompanying exudation* are the elements always present in croup.

"We, therefore, need not, for any practical purpose, admit an 'inflammatory and membranous' croup, as some writers have done, any more than we should make the same distinction in regard to pleuritis or peritonitis. All *croup is inflammatory*, at any rate; and a few cases are also accompanied by the formation of a false membrane. But the latter should not affect the treatment of the disease as an inflammation, but merely from its mechanical effects, and cannot be predicated in any case till it is actually seen; and this is not possible in most cases in which it is developed at the very onset of the disease. Finally, we would drop the term croup entirely, and use the term laryngo-tracheitis instead. In a work on the diseases of children, we would call particular attention to the fact that a false membrane is formed in about one-sixth of all the cases of this disease; while in adults, this is of very rare occurrence. But we would not make an accident the distinguishing feature of this disease, any more than we do in the case of others, nor allow it to enter into either our name or our definition of it.

"As in all other inflammations, so in this; the distinction of 'sthenic' and 'asthenic' is important, both in a pathological and a therapeutical point of view. So far, also, as laryngismus enters into any particular case—and it does into all cases of true laryngitis to some extent—the case is, of course, *spasmodic*; but this term must not be applied to the exclusion of the idea of inflammation. *Genuine spasmodic croup*, we have already seen, is a mere laryngismus. There is more or less spasm in all cases of bronchitis, and still more in whooping-cough; in the latter case in the larynx also; so that infantile laryngitis does not present any peculiarity in this respect."

In regard to the question: Does laryngitis and tracheitis, with false membrane, occur as well in the adult, as in early life? Dr. Peaslee says:—

"We do not hesitate to answer this question in the affirmative, having seen such membranes ourselves, and having demonstrated their fibrillated structure under the microscope. Such cases are comparatively rare, for the reasons already assigned; and therefore occur more frequently in females who are not accustomed, or, as in some instances, not able, to expectorate at all. I received and still retain such a membrane, expelled in a tubular form by an adult female patient of Dr. G. W. Garland, now of Lawrence, Mass. Even '*membranous croup*,' then, if we retain the term, occurs, though rarely, in adults, it being, at all ages, essentially the same disease so far as its pathology is concerned."

To the question: Does the danger to the patient, in infantile laryngo-tracheitis with false membrane, result almost entirely from the presence of the membrane, as seems to be generally admitted, or is the inflammation alone often sufficient to produce death? Dr. Peaslee answers, that—

"Surely no reason can be assigned why a disease so fatal to adults as laryngitis and tracheitis should not be equally so in early life. And yet, since the disease of the 'Father of our country' to the present time, adults have died without the formation of a false membrane, in almost all instances, so far as can be ascertained.

"The fact cannot be overlooked, that an inflammation of the larynx is a serious and a dangerous matter, at any time of life, as a *mere inflammation*, aside from the final disposal of the exuded plasma. The treatment is, therefore, to be directed primarily to the disease as a mere inflammation. Of course, the formation of the membrane brings a new danger to the patient, since it partially, and, if it extends to the bronchial divisions in the lungs, in some parts completely, closes the air-passages, and thus produces a gradual asphyxia. Occurring, as it does, also, after the patient is reduced by the previous inflammation, its dangerous effects are increased. But we must, however, bear in mind the fact that the membrane is not organized till the *inflammation has subsided* in the part. The inflamed vessels relieve themselves by the exudation, and thus the inflammatory process is arrested. The treatment, therefore, to subdue inflammation, so proper at first, is *not* proper after the false membrane is formed in the larynx and trachea. We have here a *consequence* of inflammation to treat, in a part which the inflammation itself has left. The thicker the membrane, the greater the danger and amount of asphyxia; but the small trachea of an infant being closed in a greater ratio by it than the larger tube of the adult, the danger is comparatively slight in the latter from its formation. It is seldom more than one and a half lines thick at the thickest part, in the child; and usually one-half to one only.

"The progress of laryngo-tracheitis," Dr. P. remarks, "is invariably from above downwards; the observations of the best pathologists have established this point. Generally, or at least very frequently, a catarrh commences in the nasal passages, and extends backwards into the pharynx; then descending into the larynx, continues such for a time, or at once merges into an inflammation. Often the pharynx becomes inflamed, and a false membrane appears upon it before the inflammation descends into the larynx and trachea. In some cases the larynx may be the part first affected; but if so, such cases are to be regarded as exceptional, and the inflammation never extends upwards, but always downwards from that organ. The existence, therefore, of a catarrh for two or three days, and especially of a false membrane on the pharynx, before the peculiar ringing cough of infantile laryngitis occurs, are more important elements of diagnosis in this disease, though their absence is not *demonstrative* of its non-existence. It will be recollected, therefore, that we said the exudation of plasma relieves the inflammation of *the part*, though at the same time it may be extending downwards to other parts lower in the air-passages.

"In infantile laryngitis it has been stated that its progress is downwards into the trachea. In some cases, and not unfrequently, it extends into the bronchi, and even into their subdivisions into the lungs. The exudation is sometimes so copious in the smaller tubes in the lungs, that they become filled with a solid cylinder of plasma, instead of being lined with a false membrane. The same often occurs in cases of bronchitis in adults. We can have no warrant that this result will not occur in any case where a false membrane has been formed; nor that convalescence will commence at once, though large portions of it have been expelled from the trachea. Nor can we expect the convalescence to progress rapidly as soon as the laryngismus and ringing cough have ceased, since the bronchitis may still remain for a few days, or may even prove fatal at last.

"Some seem to think that, if the false membrane is removed from the larynx and trachea, all danger is over. Nothing, however, is gained by merely passing the air through the *air-passages*; it must enter the *air-cells* before the blood is aerated thereby. Whether, therefore, the trachea is diminished to one-half its capacity by a false membrane; or one-half of the smallest bronchial tubes are completely closed by solid cylinders of plasma, will not make the least difference so far as the production of asphyxia is concerned.

"The false membrane is often formed in a very short time after the exudation is poured out; but it is an important fact that a false membrane formed on a mucous surface never becomes vascular, and therefore permanent, as it may on a serous membrane; but it *soon becomes detached, and spontaneously falls off*. This is well-known to be the case in vaginitis, and the endo-metritis which sometimes accompanies dysmenorrhœa; and the disease now under consideration

is not at all different from these in this respect. Only a fibrillation existing in the false membrane, and, therefore, no vascular connection between it and the subjacent surface, it soon loses its slight vitality, and is cast off accordingly.

"The time elapsing before the spontaneous detachment of a false membrane varies in different cases, but seldom exceeds five or six days after its formation. But this result will surely occur if the patient's strength continues, and his life is sufficiently prolonged to afford the necessary time.

"It has been seen that laryngo-tracheitis occurs in the adult as well as in the infant; but is not the 'Diphtherite' of M. Bretonneau a different disease?

"We see no necessity," says Dr. P., "for any such admission. In diphtherite the false membrane almost always covers the pharynx and tonsils, and often extends into the posterior nares, covering both surfaces of the velum, and then descends into the larynx and trachea. The same often occurs also in connection with scarlatina and rubeola. But all this also often occurs in the young child, and therefore the terms 'Diphtherite,' or 'Croup of Adults,' we consider objectionable, as calculated to foster the idea of a radical difference as to the nature of inflammation of the larynx and trachea in the young child and the adult, while they are pathologically the same."

Although, from a careful perusal of the foregoing extracts from the monograph before us—for the copiousness of which we hardly think we need apologize, considering the importance of the subject to which they refer—our readers will acquire a very correct idea of the views of Dr. Peaslee in reference to the pathology of croup; nevertheless, the following recapitulation of the more prominent of those views will not, we are persuaded, be unacceptable:—

"1. Croup is always a laryngo-tracheitis, or inflammation, not of a specific kind, of the mucous membrane of the larynx, extending thence into the trachea also at least.

"2. Like other inflammations, it may be either sthenic or asthenic.

"3. Its progress is always downwards in the air-passages, whether it commences in the larynx, or, as is more frequently the case, higher up.

"4. It is always attended by an exudation of plasma upon the surface of the inflamed membrane.

"5. The inflammation subsides in the part on the occurrence of exudation.

"6. The plasma *remains* on the surface—it being usually detached and removed in case of the adult by coughing—it may, *a.* Become reabsorbed, which is a rare result; or, *b.* Become converted into purulent matter, and then excreted; or, *c.* Become organized into a false membrane. This last result is far more common in early life than in adults, though then occurring only in about one-sixth of all the cases. Consequently it can never be predicted, at the commencement of the disease, whether a false membrane will, or will not, be formed.

"7. If a false membrane is formed, it will become spontaneously detached, if the patient's life is sufficiently prolonged. Its removal, however, in whatever way effected, does not *insure* recovery.

"8. Laryngismus is associated, to some extent, with true laryngo-tracheitis, as a matter of course.

"9. Catarrh generally precedes laryngo-tracheitis. But the catarrhal discharge, *so called*, during the more advanced periods of this disease, is the purulent fluid formed by the degeneration of the exuded plasma."

"10. Diphtherite, or the 'croup of adults,' differs not *pathologically* from infantile laryngo-tracheitis. Generally, however, it is an asthenic form of laryngo-tracheitis, since it usually attacks persons already debilitated by other diseases."

It is impossible for us to follow the author of the monograph before us in his discussion of the rational treatment of infantile laryngo-tracheitis directly based, as he supposes, upon a correct pathology of the disease. The treatment he recommends, and, in general, the means upon which he would depend for carrying out his general indications, have our hearty approval. In some few particulars—in the comparative value of this or that remedy—and the stage of the disease to which it is best adapted—we may differ from him; but, in its general outlines, his therapeutical directions are sound and judicious. A general abstract of them would be of little value—to be properly appreciated they must be studied

in extenso, in connection with the comments with which the author has accompanied them.

We can only give here the following tabular view of the treatment recommended by Dr. Peaslee for the two forms of the disease, as drawn up by himself:—

INDICATIONS.	STHENIC LARYNGO-TRACHEITIS.	ASTHENIC LARYNGO-TRACHEITIS.
I. To arrest the inflammatory process, and control the laryngismus.	An emetic, followed by a full dose of calomel, if required.	Do. do.; or pill. hydrargyri, instead of calomel.
	Leeches to the throat, or over the sternum; followed by cold applied continuously.	Leeches doubtful. Cold continuously applied.
	Tartrate of antimony; sedative doses, cautiously administered.	Nitrate of potassa.
	Internal application of solution of nitrate of silver.	Do. do.
	Inhalation of aqueous and narcotic vapours.	Do. do.
II. To prevent organization of the exuded plasma.	Opiates, with caution. Hydrocyanic acid, do. do.	Do. do.
	Sponge-probang and caustic solution.	Do. do.
	Expectorants.	Stimulating expectorants.
	Calomel, in small doses.	Turpeth mineral.
	Alkalies.	Do. do.
III. For removing the false membrane when formed.	Counter-irritation.	Do. do.
	Tracheotomy?	Do. do.
	Continue caustic solution.	Do. do.
	Do. expectorants.	Do. do.
	Do. counter-irritation.	Do. do.
IV. To sustain the strength.	Do. alkalies.	Stimulant alkalies.
	No emetics, or calomel.	Do. do.
	Tracheotomy, <i>seasonably</i> , if at all.	Do. do.
	Milk-porridge, arrowroot, broth, &c.	Do. do.; also wine whey, milk punch, &c.
	Tonics, if required.	Tonics and stimulants.

The entire monograph we would very strongly recommend to the attention of the medical public as being among the very best that have appeared in reference to the disease of which it treats.

D. F. C.

ART. XIX.—*A Practical Treatise on the Diseases Peculiar to Women, Illustrated by cases derived from Hospital and Private Practice.* By SAMUEL ASHWELL, M. D., Member of the Royal College of Physicians, London; and late Obstetric Physician and Lecturer to Guy's Hospital. Third American, from the Third and Revised London Edition. 8vo. pp. 528. Philadelphia, 1855. Blanchard and Lea.

THE treatise of Dr. Ashwell has been already assigned a high rank among the leading authorities on the diseases of the female; and deservedly so. Although less full and comprehensive than one or two of the modern works on the subject by distinguished Continental writers—it being confined mainly to a consideration of the affections incident to the non-pregnant state—nevertheless, the clear, definite, common sense, and practical manner in which the subjects embraced in it are treated—more weight being accorded to the results of positive and repeated observations than to the deductions from specious hypotheses, and to *a priori* reasonings in favour of any special system of medi-

cation—renders the treatise of Dr. Ashwell a valuable and trustworthy one for the use of the student and young practitioner.

The account given by Dr. Ashwell of the pathology and therapeutics of the maladies of the female sex, of which he treats, are based chiefly on the results of his own clinical observations, made under the most favourable opportunities for the study of these diseases; at the same time, however, he has not been neglectful of the labours of others in the same field; in fact, nothing of importance appears to have been overlooked by him “in the progress of that department of medical science to which the work is devoted, that could either assist the practitioner or inform the student.”

Of the various diseases treated of by Dr. Ashwell, he has narrated in more or less detail, numerous cases, in order that the symptoms of these “may show whether the histories of the diseases are accurately given,” and “from their successful or unfavourable issue, the danger of the malady and the worth of the treatment may be demonstrated.”

We agree with the author that well delineated cases of the diseases treated of “add also greatly to the interest of an elementary work, as they relieve the dullness from which a constant repetition of principles, without such portraits, is nearly inseparable.”

D. F. C.

ART. XX.—*Anniversary Discourse before the New York Academy of Medicine. Delivered in Clinton Hall, November 22, 1854.* By JOHN H. GRISCOM, M. D. Published by order of the Academy. 8vo. pp. 58. New York, 1855.

WE have read with uncommon interest this address of Dr. Griscom, and could wish that it were possible to place a copy of it in the hands of every citizen of these United States, and induce him to reflect carefully and deliberately upon the important themes it discusses—themes in which it is not the physician alone who is interested, but which involve the comfort, and health, and life of each individual of the entire community.

The relation between the people and the science of medicine is the subject of Dr. G.'s discourse, and with a manly vigour of style, great plainness of speech, and clearness of demonstration, he shows the important and continued benefits which legitimate medicine, administered by a well-educated, skilful and devoted body of physicians, confers upon society, as well by its therapeutic or curative means as by its prophylactic or preventive powers. And this, with small return on the part of the public to its professors, in the form of gratitude or pecuniary compensation, or even of a just respect for their proper rights and privileges.

The immense amount of gratuitous labour performed by the medical profession, at the sacrifice of the time and comfort, and at the imminent risk of the health, and even life of its members; of which labour the community at large reap the reward in the promotion of their best interest, as well as in merely a pecuniary point of view, is very forcibly depicted by Dr. Griscom.

“Can there be named,” he remarks, after pointing to facts which cannot be disputed; “can there be named any profession, high or low, feeble or strong in numbers, that gives the proportion of a thousandth part of the amount of service to charity, or public weal, that is given by the medical profession annually, without fee or reward? The glittering enticements of wealth, apparently so easily attainable in the counting-house; the allurements of popularity held forth from the hustings or the forum; the ambition to command the applause of listening senates, or the bubble reputation at the cannon's mouth; these are all suffered to pass as the idle wind, by him who finds a luxury in doing good, be it in the gilded and satin-furnished chamber of the millionaire, or the stripped and impoverished cellar or attic of the child of want and misery.

“But the response is upon the lip of the layman, ‘you receive an equivalent in enlarged experience, and the hospital and dispensary appointment gives a

credit which is a passport to private success.' Without stopping to criticize this assertion, which is so amenable to criticism by those who best understand the subject, let me simply say that the same amount of experience would be afforded, and an equal influence upon private success be exerted, if the incumbents of these appointments were to receive a pecuniary remuneration for their services; in other words, if the members of the profession were to-morrow unanimously to decline these incumbencies, and the authorities were compelled to pay a pecuniary equivalent for the required medical and surgical aid, while, in either case, the experience alluded to is more, far more, for the advantage of the public than for that of the profession; and in the establishment of hospitals, dispensaries, and asylums, the public receives back, in this very experience, all it pays for their support, with decupled interest. In whatever light it may be viewed, therefore, the rendition of these services is simply the *contribution of the medical profession to the support of public charity to the full amount mentioned*" (nearly one million annually in New York alone); "it is so much saved to the tax-payers.

"I would not be understood as recommending, or even suggesting, any alteration in the time-enobled and heaven-born spirit which actuates our profession in this matter; and I am fearful that, even so far as I have gone, I have not only trespassed upon the sacred injunction of Christian charity: 'Let not thy left hand know what thy right hand doeth,' but that I have also violated one of the most honored rules of medical ethics. If I have erred herein, the blame is exclusively my own. I have but sought to place the profession of my choice '*rectus in curia*' in the great assembly of the people, which contains so many who affect to believe in its hard-hearted and exacting disposition, and who flippantly charge it with inhumanity, when it seeks the wherewithal to obtain some of the creature rest and comfort, which all value so highly, but which the physician's very duties, in a great degree, deny to him."

Dr. Griscom next points out the disregard shown by the public to medical science and its legitimate professors, by the liberal encouragement bestowed upon every form and phase of charlatanry, and refers to the baneful effect this produces upon the progress of science and the improvement of medicine.

"I have before," he remarks, "adverted to the fact, that it is necessarily to the scientifically educated members of the medical profession, and to them only, that the world can look for progress in medical and surgical art. To this duty the *attention*, and no inconsiderable portion of the *means* of the profession, are continually addressed. Its journals are supported by itself alone; except in one of these United States (Michigan), medical schools, unlike our ward schools and Free Academy, which are sustained by the public, derive their support almost wholly from the profession. The expenses incurred by the numerous State, county, and private medical societies, are defrayed, to the last cent, without aid from that public whose interests are deeply involved in their continued and energetic action; while the time and talent expended in gratuitous services in medical charities, as has already been stated, redound greatly to the advantage of the public."

"Under this state of facts, it appears certainly due to the profession that it should receive from the public, whose interest it serves, all the aid and support which is legitimately derivable from the practice of the healing art. To put money into the pockets of an empiric, although his treatment may occasionally *hit right*, is to support a class of men devoted exclusively to private gain, without the least public spirit in their calling, who have never yet given the wheel of medical science a single revolution forward, and from their ignorance could not, if they would.

"This position I am ready to abandon when there shall be shown one new idea in chemistry, a single ray of light from the profound depths of physiology, one anatomical discovery, or a reliable idea before unknown in pathology or therapeutics, that has ever emanated from the head of any medical charlatan that ever lived. In fact, a truly scientific man cannot be a quack.

"The loss to the medical profession, therefore, by the support of charlatanry, though it has been enormous, is far surpassed by the loss sustained by *medical*

science, and consequently by mankind, for whose use all sciences are studied and improved."

Three means are indicated by Dr. G. for promoting the prophylactic or preventive powers of medical science, and the adoption of the means it indicates for the removal of the causes of disease.

The first of these is, *the universal introduction of Human Mechanism and Physiology, as a subject of study in schools, especially the public schools.* The second is, *the establishment of a voluntary association*, composed jointly of laymen and physicians, whose objects of inquiry and action shall comprise the entire field of the preservation of public and private health, in all its varied and multitudinous relations.

"I believe," says Dr. G., "that on no subject would sound and authoritative information be sought for with more avidity. By no other means could more practical general good be bestowed; by none could more lives be saved and prolonged than by such a society, energetically conducted.

"The argument for the formation of such an association exists in the fact that there are numbers of able men who have devoted much time to the study of Hygiene, who have done much, and are prepared to do more, to enlighten the public mind, but whose labours are rendered comparatively useless for want of union and concentration. Their individual efforts, dispersed through ephemeral publications, are productive of but feeble effects, when, by combination and a common channel, through which to pour the results of their investigations and studies, the good would be effected for which they now vainly pray and labour.

"Such a society would study out the true method of city sewerage, generally heretofore committed to most unscientific hands, and from which, valuable as it is when rightly conducted, there is great reason to apprehend serious evils. It would infuse into the public mind, into our legislative bodies, and our magistracy, a more intelligent regard for the proper construction of private and public edifices of every description; it would point out where and why certain diseases prevail, and how they might be avoided; it would obtain an influence in the proper regulation of health measures and the enactment of sanitary laws which would be of incalculable benefit to the commercial prosperity of this and other cities, and the happiness and comfort of their every inhabitant. It could do much by exerting its influence in the right direction to secure that great desideratum, a sanitary survey of the State, and obtain through the census a vast amount of important medical statistics, now almost wholly lost; it could materially aid in perfecting and establishing a complete system of registration of births, deaths, and marriages, now entirely neglected for want of proper intelligence and energy in the right quarter. In a word, it would take under its keeping and guidance the sanitary interests of the whole people, in all their aspects and relations, and, as a medium of communication between the people and the profession, would exert a powerful influence for the good of both."

The third and last suggestion of Dr. G. is the establishment of a *health police*.

"Since," he observes, "it is manifest that the aid of law must be invoked for the suppression and prevention of disease-producing circumstances, it would appear at first blush that, for an intelligent and satisfactory performance of this important and very nice duty, some degree of medical knowledge were requisite. In many instances it requires no little scientific acumen to determine what really constitutes a nuisance. Even for the mere purposes of *inspection* of the domicils, workshops, manufactories, and places of congregation of every kind, it were but common sense to acknowledge the necessity of a certain degree of familiarity with the laws of health and the causes of disease, to say nothing of the many other more delicate and responsible duties implied in the name of a preventive sanitary police.

"No argument can be needed to enforce so self-evident a proposition as that a *health officer* should possess a medical education; and yet, will it be believed, that the commercial metropolis of the western world, with a population rapidly approaching a million, has a sanitary police which would not only fail to be recognized as such by its name, but among the twenty-nine persons constituting

it, there is—and that only by chance—but one medical man, and he with no concern in its practical prophylactic duties. With this exception, from head to foot, not a member of it probably would be able to distinguish incipient smallpox from mosquito bites; and, if told it were the former, would most probably give it a wide berth, rather than personally attack so formidable an adversary."

"It is not enough that the sexton's weekly returns of the causes of death are summed up and given to us in annual tables by the yard in length, which a little arithmetic will accomplish. We need to know the *causes of the diseases*; where they are, what they spring from, and how to avoid them; information which can only be obtained and properly disseminated by men of good medical education, and possessing the proper taste and energy for such work."

There is great truth in the concluding words of the discourse before us. Would that the public could be induced to give ear unto them, and profit by them.

The votaries of medical science, whatever impediment may rise up in their path, will, Dr. G. remarks—

"Still move forward on the great errand of human health and life, enlarging its borders, and strengthening its stakes; feeling that upon their energy in the prosecution of improvement, and the enlightenment of the public, depend in a great measure the health, the strength, the longevity, the mental, moral, and physical condition of communities, as well as of individuals, and upon them depend the welfare of the State, and even the stability of the government itself. A republic of imbeciles, of dwarfs, either of body or spirit, of a people wasted by disease and deprived of the natural provocatives of health, is as impracticable as a heaven of profligates and demons; and without sound bodies, sound minds are impossible."

D. F. C.

ART. XXI.—*Outlines of Chemical Analysis prepared for the Chemical Laboratory at Giessen.* By HEINRICH WILE, Professor of Experimental Chemistry in the University of Giessen. Translated from the third German edition, by DANIEL BREED, M. D., late of the Laboratory of Liebig, Löwig, and Wile, and LEWIS H. STEINER, M. D., Professor of Chemistry in the National Medical College. Boston and Cambridge, James Munroe & Co., 1855. 8vo. pp. 298.

THE Laboratory of Giessen has so long been the most prominent resort for the acquisition of a practical knowledge of chemistry, and its conductors have acquired so firm a reputation for successful teaching, that any work in relation to chemical analysis emanating from this source, and founded on the course pursued there, must necessarily command attention. Heinrich Wile, under whose supervision it has some time since passed, has, in this third edition of his *Outlines*, given the results of his increased experience, and added to the examples in quantitative analysis. The first edition was soon translated by Professor Hoffman, and adopted in British and American laboratories as a safe guide, but the advance of analytic chemistry has rendered improved methods of research necessary, which are here fully supplied. The most marked feature in the two late editions is the increase in the number of tables, by which the student is to be guided in his researches, and is more easily led to his results and determinations. The translators have given a very satisfactory transcript of the original, and have, no doubt, adhered with strict accuracy to the ideas of the author, having previously been personally the recipients of the instruction here conveyed in the printed form. They have also given an introductory chapter on apparatus and manipulation, very judiciously adapting the work to the wants of those to whom this part is not familiar.

The mechanical execution of the book is good, and the type clear and of good size, not at all calculated to fatigue the eye; even in the tables, where restricted space necessarily produces a reduction, and which, from their constant use as references, should be entirely free from any drawback of this kind.

R. B.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Experiments on the Innervation of the Heart.* By Prof. RUDOLPH WAGNER, of Gottingen.—I have been engaged in making experimental investigations on the innervation of the heart, during several winters, in the Physiological Institute here, especially in cold-blooded animals. The present method of experimenting on warm-blooded animals, by means of opening the thorax and conducting artificial respiration, or by means of the kymographion or auscultation, are open to objections on account of the inconveniences attending them, and on account of the injuries done to the animal, which occasion great disturbances in the action of the organ. While spending a short time last autumn in Breslau, where I found prevailing a very lively interest in physiological and pathological investigations, Dr. Kliddedorpf communicated to me some experiments he had made on apparent death, in which he had introduced into the heart a slender needle, the movements of which gave indications of the activity of this central organ of the circulation for some time after the cessation of respiration and of the pulse. I immediately resolved upon acupuncture as a means of a new series of experiments on the innervation of the heart—a means which, as far as I know, has never yet been applied to this subject. The very first experiment showed that the vibrations of a needle introduced into the heart are not only extremely favourable for determining the number of contractions, but also for ascertaining their intensity, and other phenomena, even to accurate measurement. Assisted by a number of my pupils, I have been occupied since last November in carrying on a great number of experiments on the innervation of the heart, and I should have postponed for the present the publication of the results obtained, had I not lately learned that experiments upon the same subject are being made at other points.¹ I therefore presume to hope that the improved method of experimenting in use here may be made use of and perfected by others.

The animals employed here have been generally rabbits, and sometimes cats and dogs. The needles used at first were of steel, about ninety millimetres in length, and with a head about six millimetres in thickness, which can be made hollow, that the weight may not be too great. The situation and anatomical relation of the heart of the animal to be experimented on having been accurately learned, the needle is to be introduced through the parietes of the chest into the left ventricle near the apex. The vibrations of the needle are the expression of the beats of the heart. As the needle has its point of support in the skin and intercostal muscles, it must be somewhat influenced by the

¹ For instance in Halle, by Heidenhain Fechner's *Centralblatt für Naturwissenschaften und Anthropologie*, 1854, No. 7. Also in Zurich by Ludwig and Weinmann.

movements of respiration: but this does not generally occur to a degree worthy of consideration. The number of vibrations gives the number of contractions or beats of the heart, the extent of the amplitudes (the point of support remaining the same), the intensity of the contractions, the so-called impulse. The head of the needle, as it rises and falls, can now be allowed to strike on a sonorous body, such as an inverted wineglass fixed to the movable arm of a retort-stand, so as to be placed in any situation. A few trials will be sufficient to convince any one that the innervation of these strokes is far easier and more certainly performed than counting the pulse of a bound and perhaps struggling animal. Besides, in this way the enumeration can be carried on by a great number of persons at once—a point of some importance if the number of contractions per minute be considerable, as for instance in the rabbit, where they amount to two hundred or more when the animal is at rest, and are much increased both in number and velocity when both *vagi* are divided, as shown by the increased force of the blow, and the greater amplitudes of the needle. This simple instrument is, therefore, sufficient for a great number of experiments on the innervation of the heart. The vibrations of the needle take place with great uniformity—even on its introduction, but little irregularity is occasioned—the number of contractions not being increased more than from two to three per cent., and this only lasting a short time. After the removal of the needle, the animals appear quite well. For experiments on dogs, sometimes even for rabbits, a trocar, not exceeding one millimetre in thickness may be made use of, without causing permanent injury to the animal. If it is wished to observe the contractions of the auricle, another and finer needle must be introduced, preferably into the right auricle.¹ If another glass is now placed for this needle to strike on, a pleasant play of bells is produced by the rapidly following blows, in which the ventricular needle leads. In this manner the systole of the auricles and ventricles can be made audible and visible to hundreds of persons at once, and if now another needle is introduced through the parietes of the thorax, above the diaphragm, and another glass placed for it, the number and strength of the respirations are made evident at the same time.²

If it is merely wished to observe the number and impulse of the contractions during irritation of the nerves which influence the heart's action, a single needle is sufficient introduced through the intercostal space, immediately above the left ventricle, as more simple pendulum movements are obtained here than by its introduction nearer the apex, where the well-known burning movement will give it more rotation.

It is evident that this simple apparatus can be used for accurate measurements. The movements of the needle are sufficient in extent and power to set in motion delicate levers, which can be used in the usual manner for the delineation of curves. In this manner, by means of the abscissa, the very slight intervals of time which here occur, and by means of the amplitudes, the strength of the contractions, which we name the intensity of the heart-movement, can be graphically delineated. Thus far, I have made but few experiments in this manner, as practical difficulties are to be overcome. For a great number of experiments, which I have already made, the simple needle is sufficient, and on account of its simplicity and the ease attending its use, does it appear especially worthy of recommendation.

The first question I applied myself to answer was: From what parts of the nervous system can the heart be affected in its movements, and what is the character of the symptoms produced?

¹ The introduction of the needle into the auricle has its difficulties, which must be overcome by dissection. Extravasation is frequently produced by piercing the vena cava: this may also occur by wounding a branch of the coronary vein on introducing the needle into the ventricle.

² The degree of interest maintained in his class by this celebrated physiologist is exceeding great. The combination with lectures of experiments, in which all the students take part, is in the highest degree interesting and instructive; and this mode of teaching physiology, so general in the German schools, presents a marked contrast with that prevalent among us.—[Translator.]

It is already a well-known fact that an interrupted electrical current, as produced by the common induction-apparatus, when applied to the roots and branches of the *vagi* of all the vertebrata, produces retardation of the movements of the heart, considerable pauses between the beats, and even an entire cessation in the diastole, during which only slight contractions of single bundles of fibres are observed. With a powerful stream, the inclosure of only a single *vagus* in the circuit is sufficient, even in mammalia, to produce cessation of the heart's action. But, nevertheless, I believe I have discovered that this cessation is never so complete and enduring when the poles are applied to the *vagi*, as when they are applied to the *medulla oblongata*. This is particularly the case in birds, the cessation being far more perfect when the medulla is included. In frogs and fishes, a total cessation of several minutes' duration can be caused, after previous removal of the encephalon had increased them.¹ I concluded, from this circumstance, that there may be other fibres proceeding from the medulla, not included in the *vagi*, which exert a retarding influence upon the heart's action. It was necessary, therefore, to examine the cervical portion of the *sympathetic*. Weber supposes that the sympathetic exerts an influence opposite to that of the *vagus*, but he has not succeeded in proving this by experiments.² More frequently, the ground is now taken, in physiological works, that increased contractions of the heart follow irritation of the cervical portion of the sympathetic, a doctrine which Liebig has lately opposed, without, however, adducing anything definite in regard to the influence which is really exerted by the sympathetic upon the movements of the heart.³ I found by a great number of experiments made on rabbits, with all precautions, and with various forms of apparatus, that when both sympathici were acted on there was constant diminution in the number of beats of the heart, nevertheless, not in the same degree as in the case of the *vagi*. The decrease amounts to one-fourth, one-third, or even one-half of the normal number, depending for the greater part on the power of the current. Generally the decrease is in a constant proportion, as the following experiments on a rabbit show:—

			Beats of the Heart in fifteen seconds.				Beats of the Heart in fifteen seconds.
Both sympathici included			17	After breaking the circuit			43
"	"	"	16	"	"	"	44
"	"	"	18	"	"	"	36
"	"	"	18	"	"	"	35

Constant increase of the beats of the heart is occasioned: 1. By including the sympathici in a constant stream. 2. By tying and dividing the cervical portion of the sympathetic. I prefer ligation and division to the use of the electrical current as producing no irritation of neighbouring nerves nor other disturbing symptoms. Under favourable circumstances, the following or similar results will be found:—

Experiment on a full-grown Male Rabbit; number of beats of Heart in fifteen seconds; five enumerations.

At rest.	After exposure of nerves.	After tying sympathici.	After dividing sympathici. ⁴	After tying right vagus.
61	59	64	67	77
60	61	67	68	76
62	60	68	68	75
60	60	68	67	78
60	62	68	64	79

¹ For example, in frogs from six to ten beats.

² Handwörterbuch der Physiologie, vol. iii. sec. ii. p. 47.

³ Lehrbuch der Physiologie, vol. i. p. 178: "As regards the rabbit, this statement is entirely wrong, as I have satisfied myself by thorough investigations conducted by Weinmann, under my own observation."

⁴ "Violence must be avoided, and after each ligation, particularly of the *vagi*, time enough must be allowed for the animal to become quiet, and the consequent disturbance of the heart's action to cease."

After fright, by a blow on the table	68
Immediately after	64

Experiment on a full-grown Female Rabbit; number of beats in fifteen seconds; five enumerations.

At rest.	After exposure of nerves.	Both sympathici tied.	Both sympathici divided.	Both vagi tied.	Both vagi divided.
58	54	59	68	74	77
57	56	59	65	73	76
57	56	58	64	74	75
57	56	60	64	75	78
57	56	60	65	74	79

I have obtained similar results by experimenting on the cat, in which animal the sympathetic and vagus run nearer together than in the rabbit, but yet are more widely separated than in the dog.

Experiment on a full grown Cat; number of beats in fifteen seconds; five enumerations.

At rest.	Sympathici tied.	Vagi tied.	Ten minutes after.	Twenty minutes after.
40	48	62	68	67
42	45	62	66	66
43	45	58	65	67
43	45	59	67	68
42	47	57	64	67

Many of these trials prove to me conclusively that the sympathici exert an influence on the heart similar to but less powerful than the vagi—retarding its actions, but never increasing them—since the movements of the organ are constantly increased when removed from their influence. That ligation does not exert so much influence as division, I conclude is from the fact that some of the fibres retain their conducting power after application of the ligature.

In using electrical irritation on the sympathetic the objection could be made that, however perfect the isolation, the vagus would also be affected, through the union of the fibres of both nerves in the cardiac plexus. This objection has no weight when ligation and division are resorted to.

In what manner do emotions, such as fright, affect the movements of the heart? This is beautifully shown by the vibrations of the needle. If a violent blow is given on the table, the animal, of whatever kind, draws itself together, the needle remains still for a momentary period (mostly less than a second), and then, in the following seconds, give a series of rapid vibrations, as the following trials show.

A full grown female rabbit had constantly, when at rest, 62 to 63 beats of the heart in 15 seconds; during the same periods of time it had—

	Beats.		Beats.
After a blow on the table	70	Immediately after	63
" " "	69	" "	63
" " "	69	" "	63
" " "	67	" "	63
" " "	67	" "	62
" " "	66	" "	63
" " "	68	" "	63 ¹

That this communication between the brain and the heart is exercised by the vagus has appeared probable; yet possibly it may take place through the spinal column, and from thence in the course of the sympathetic, or it might be the result of the general muscular movement attending the shrinking together of the body. The apparatus affords means of proving conclusively that it is the vagus; after the division of the sympathici, the momentary cessation of

¹ Thus in seven observations the most striking similarity.

the vibration of the needle is observed, and the consecutive increase; but after division of the vagi the animal draws itself together as before, but the movements of the needle are not checked, and the number of beats of the heart continues the same. If we observe the effects of emotions on ourselves, we experience the same results; in consequence of a sudden fright, we have the same momentary cessation of the heart's action, and followed by a number of rapid beats. It is a precisely similar occurrence.

Some experiments on the *rami descendens hypoglossi* in the dog, have given no decided results, yet these nerves do not appear to be entirely without influence on the movements of the heart.

The number and intensity of the movements of respiration, and their relation to the beats of the heart have also been taken into consideration in our experiments. They are not without influence; nevertheless, they do not affect the principal results, as given above.

The following is a synopsis of the results obtained:—

1. The needle introduced into the heart affords a more reliable means of enumerating the beats of the heart than auscultation or any other mode; after a little practice, a number of observers in counting from fifty to sixty beats in fifteen seconds vary only one or two.

2. Immediately after the introduction of the needle, the number of beats are increased from two to six, in fifteen seconds; but in a few minutes this subsides; often no increase at all takes place. This may, perhaps, be accounted for by the needle sometimes wounding a branch of a nerve.

3. The number of beats of the heart in fifteen seconds varies in different individuals of the rabbit kind; most are between fifty and sixty; sometimes above sixty; very seldom as low as forty.

4. By irritation of both vagi with a powerful current and consequent cessation of the movements of the heart, the full vibrations of the needle are stopped, but yet slight, rapid, and trembling vibrations occur, which are the indication of partial contraction of single bundles of fibres.

5. Cessation of the heart's action is most surely occasioned, and for the longest time, by irritation of the medulla oblongata, with an interrupted current; the least surely through irritation of one vagus.

6. An uninterrupted current applied to the vagi occasions increased movements, whether passed in a centrifugal or centripetal direction. But this result is not so certain as the opposite effect with an interrupted current.

7. By counting through periods of fifteen seconds, it is found that the movements are somewhat slower than natural immediately after the removal of the poles, but they gradually increase until in the third period the usual number is reached.

8. Irritation of one or both sympathici, with an interrupted current, produces constantly decrease of the heart's action, from one-fourth to one-third, or even one-half the usual number, and, as is generally the case, with an uninterrupted current passing through the vagi, the respiration is also retarded.

9. Ligation and division of the sympathici, and of the vagi, cause (with some exceptions) increase in the number of beats; often in a definite proportion. The increase, on tying both sympathici and vagi together, amounts to one-fourth or one-third the usual number.

10. Fright has a similar effect as galvanic irritation, but weaker, the channel of communication being the vagi, after the division of which the influence of the brain on the heart is lost.

[We are indebted for the translation of the above paper to Dr. JOHN C. REEVE, of Dayton, Ohio.]

2. *Osmotic Force*.—Prof. GRAHAM in a Bakerian lecture (*Med. Times & Gaz.*) delivered at the Royal Society, presents the following views respecting this power, which are highly interesting in a physiological point of view:—

"The term 'osmotic force' is applied to the power by which liquids are impelled through moist membrane and other porous septa, in experiments of endosmose and exosmose. Diffusion and capillarity are shown to be insufficient to account for it.

"The nature and *modus operandi* of the chemical action producing osmose remains still very obscure. Salts and other substances, capable of determining a large osmose, are all chemically active substances, while the great mass of neutral organic substances, and perfectly neutral monobasic salts of the metals, such as chloride of sodium, possess only a low degree of action, or are wholly inert. The active substances are also, relatively, most efficient in small proportions. The chemical action must be different on the substance of the membrane, at its inner and outer surfaces, to induce osmose; and according to the hypothetic view, which accords best with the phenomenon, the action on the two sides is not unequal in degree only, but also different in kind. It appears as an alkaline action on the albuminous substance of the membrane at the inner surface, and as an acid action on the albumen at the outer surface.

"The most general empirical conclusion that can be drawn is, that the water always accumulates on the alkaline or basic side of the membrane. Hence, with an alkaline salt—such as carbonate, or phosphate of soda—in the osmometer, and water outside, the flow is inwards; but with an acid in the osmometer, on the contrary, the flow is outwards, or there is negative osmose, the liquid then falling in the tube."

A table is given exhibiting the osmose of substances of all classes.

"It may appear to some, that the chemical character which has been assigned to osmose takes away from the physiological interest of the subject, in so far as the decomposition of the membrane may appear to be incompatible with vital conditions, and that osmotic movements must therefore be confined to dead matter. But such apprehensions are, it is believed, groundless, or, at all events, premature. All parts of living structures are allowed to be in a state of incessant change of decomposition and renewal.

"The decomposition occurring in a living membrane, while effecting osmotic propulsion, may possibly, therefore, be of a reparable kind.

"In other respects, chemical osmose appears to be an agency particularly adapted to take part in the animal economy. It is seen that osmose is peculiarly excited by dilute saline solutions, such as the animal juices really are, and that the alkaline or acid property which these juices always possess is another most favourable condition for their action on membrane. The natural excitation of osmose in the substance of the membranes or cell-walls dividing such solutions seems, therefore, almost inevitable. In osmose there is, farther, a remarkably direct substitution of one of the great forces of nature, by its equivalent in another force—the conversion, as it may be said, of chemical affinity into mechanical power. Now, what is more wanted in the theory of animal functions than a mechanism for obtaining motive power from chemical decomposition as it occurs in the tissues? In minute microscopic cells, the osmotic movements, being entirely dependent upon extent of surface, may attain the highest conceivable velocity.

"May it not be hoped, therefore, to find in the osmotic injection of fluids the deficient link which certainly intervenes between muscular movement and chemical decomposition?"

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3. *Ammonia Evolved in Respiration.* By Professors VIALE and LATINI, of Rome.—After a series of delicate experiments, conducted with the greatest care and minuteness, these eminent chemists have arrived at the following conclusions:—

1st. That ammonia is evolved in each act of expiration. They consider that a healthy man expires, in 24 hours, 0.763 grammes of ammonia; and in a year a quantity equal to 278.504 grammes of the gas. They calculate that in a city containing 160,000 inhabitants, 44.56 kilogrammes of ammonia are evolved by respiration in the course of a year.

2d. That the ammonia is given off in the form of a subcarbonate.

3d. That the lungs cannot expire *pure* carbonic acid gas.

4th. That the *azote*, pronounced by chemists to be a product of respiration, is merely ammonia.

5th. That this ammonia keeps up the supply of the gas existing in the atmosphere, which, dissolved in rain, falls upon and enriches the earth.

6th. That the active principle of contagion is probably merely an ammoniacal salt. Their reasoning on this point is rather curious: The lungs, skin, and excretions all give off a considerable quantity of ammonia, and contagious epidemics are generally developed where a large number of people are crowded together within a small space, or where the emanations of putrefying animal substances are rife. The latter emanations are always ammoniacal. Fumigations of chlorine, chloride of lime, nitric and acetic acids, are the antiseptics which decompose the ammoniacal salts, and probably also the contagious principle. Heavy rains, and tempestuous winds have been known to arrest the course of epidemics of contagious diseases. Viale and Latini believe that the ammoniacal salts which constitute the principle of contagion may be united either to carbonic acid, or to the metalloids (as sulphur, phosphorus, iodine, etc.), or to animal products, generating cyanogen, or to fatty bodies, fixed and volatile oils, or even to albumen, so as to act as a ferment.

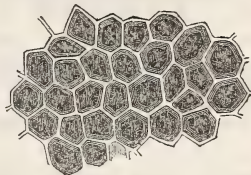
7th. That during respiration a certain quantity of albumen is eliminated.

Professor F. Liebig, of Berlin, has published a letter in which he attaches great weight to the foregoing deductions, and considers the demonstration of ammonia in respired air as a fact of great scientific importance.—*Monthly Journal of Medicine*, January, 1855, from *L'Union Médicale*.

4. *Reticular Mucous Membrane of the Alimentary Canal*.—ERASMUS WILSON, Esq., has recently made some investigations relative to the structure of the mucous membrane of the alimentary canal, and has published the following minute account of the reticular mucous membrane, illustrated by two very beautiful figures.

"In the *stomach*," he observes, "the mucous membrane is reticular; the raised portion of the membrane forming a fine network (reticulum) over its whole

Fig. 1.



"A portion of the mucous membrane of the stomach magnified seventy-five times. The alveoli measured 1-200ths of an inch in length, by 1-250ths in breadth; the width of the septa being 1-1000ths of an inch. The smaller alveoli measured 1-250ths of an inch in length, and 1-300ths in breadth. The trifid or quadrifid division of a small artery is seen at the bottom of each alveolus, and in the depressions between the divisions of the artery, the apertures of the gastric follicles; two, three, or four in each depression.

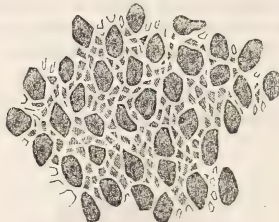
surface, and constituting the septa, the areolæ of the network being the alveoli. The amount of projection of the reticular frame, in other words, the depth of the septa is about $\frac{1}{800}$ of an inch; and their breadth, that is, the thickness of the septa, divested of epithelium, about $\frac{1}{1000}$ of an inch. In structure the reticulum is composed of a fold of the limitary membrane, containing the transparent granular substance of the surface of the corium and a plexus of capillary vessels. The capillary plexus of the reticulum is single, a disposition which probably determines the thickness of the septa; its meshes are large and open, and the vessels flexuous and serpentine in course. The border of the septum is formed by a capillary vessel which sometimes runs in a straight direction, and forms an even rim to the mouths of the alveoli, like that of a honey-comb; but more frequently makes abrupt curves and loops, which project before them the limitary membrane, and give rise to flat papillæ. These flat papillæ are chiefly found at the angles of the reticulum, and their existence in numbers gives a fringed appearance to the rims of the alveoli.

"The areolæ of the reticulum or alveoli are polyhedral in figure, for the most part hexagonal, and slightly oblong; about $\frac{1}{800}$ of an inch in depth at the sides, and somewhat deeper in the middle, and larger at the brim than at the

fundus. Divested of epithelium, they measure in diameter at the brim between $\frac{1}{200}$ and $\frac{1}{250}$ of an inch in length by $\frac{1}{250}$ and $\frac{1}{300}$ in breadth, and at the fundus something less. The floor is uneven, and presents in the centre the axis of a minute artery, from which two, three, or four, commonly three, capillary vessels pass off to make their way to the base of the reticulum, and constitute its plexus. This little capillary axis naturally forms as many ridges in the floor of the alveolus, leaving shallow depressions or foveolæ between them; and at the bottom of these foveolæ, generally three in number, are seen the openings of the gastric follicles. The openings of the gastric follicles are oval in shape, about $\frac{1}{200}$ of an inch in long diameter, and disposed irregularly in the foveolæ, two or three in each; so that the entire number of gastric follicles opening into each alveolus would amount to from six to ten or twelve.

“Large Intestine.”—The surface of the mucous membrane of the large intestine presents this most obvious difference from that of the stomach, namely, smoothness—a difference which is as apparent to the naked eye as to the eye armed with the microscope. With the microscope, moreover, we are struck with the symmetry of the reticulum and alveoli, by the greater breadth, lesser degree of prominence, by the flatness and evenness of the septa, and by the elliptical form and shallowness of the alveolar spaces.

Fig. 2.



“A portion of the mucous membrane of the large intestine, magnified seventy-five times. The alveoli measured 1-250ths of an inch in length, by 1-450ths in breadth; the septa between the alveoli measuring 1-600ths of an inch in width. The alveoli are less regular in form and shallower than those of the stomach; and in the bottom of each is a gland with a central excretory aperture. In some of the larger alveoli there are two glands.

“The septa measure about $\frac{1}{600}$ of an inch in breadth; and the alveoli $\frac{1}{250}$ of an inch in length by $\frac{1}{450}$ in greatest breadth. The septa contain a plexus of capillaries, smaller in size than those of the septa of the stomach, and having minute areolar spaces. The brim of the septum exhibits a small, chain-like plexus, composed of two or three of these capillaries lying parallel with each other; the meshes of the plexus are oblong, and about equal in breadth to that of the capillaries. The alveoli are shallow, the floor being slightly convex, and occupied by a mucous gland having a central excretory aperture; in some of the longer alveoli there are two glands and two excretory openings.

“Immediately around the solitary glands, the alveoli and reticulum undergo some modification; the former are larger, measuring $\frac{1}{150}$ of an inch in length by $\frac{1}{250}$ in breadth; and deeper—so deep, in fact, that their floor is no longer visible. The septa are thinner, measuring $\frac{1}{800}$ of an inch in breadth, and furnished with a single capillary, instead of the triple chain of the rest of the membrane.”

5. *Single Kidney.*—M. LABE found the left kidney absent in an old man who died at Bicêtre. There was neither a renal artery nor vein on the left side, and the bladder showed no trace of a left ureter having existed. The right kidney was considerably hypertrophied, and occupied its normal position.—*Comptes Rendus de la Société de Biologie.*

6. *Congenital Absence of Pericardium.*—Dr. BRISTOWE read to the Pathological Society of London, Jan. 16th, 1855, an account of a case of this kind.

The left pleura was continuous with the pericardial sac, the heart lying in the cavity of the former, and in parts adhering to the lung itself. A small rudimentary pericardium was connected with the right pleura, and opened by a small orifice into the left. The patient had died of disease of the heart.

7. *Direction of the Uterus in Little Girls.*—M. A. PORCHAT, Interne to the Foundling Hospital, in an interesting note read to the "*Société de Biologie*" in August last states "That the great number of autopsies daily made in the Foundling Hospital has given me the opportunity of investigating the direction of the uterus in very young female children. The greater number of those examined were about two years of age: some were only a few days old. In the majority the uterus was antiflexed, and I am inclined to believe with M. Boulard, that this direction most usually exists. Nevertheless, I have several times seen retroflexion. In some instances the retroflexion was very evident. I have presented to the Society a uterus so retroflected, that the superior portion had fallen back so as to become nearly parallel to the inferior portion. But retroflexion is not always so marked; the specimen which I presented, was pronounced by high authority to be simply retroverted, it being considered that there was no twisting of the neck of the organ. I believe still that the body and the neck formed an obtuse angle, and that the uterus was therefore retroflected. The direction of the uterus depends in some degree upon the situation of the rectum; when the rectum deviates much to the right, the uterus falls directly backwards; but it may, under other circumstances, undergo a lateral deviation. The appendages of the uterus occupy very different positions. The flexion of the uterus is not in my opinion the result of cadaveric alterations, and the degree of flexion has no relation to the distension of the intestine by gas. I have seen the uterus flexed in subjects where the intestine was not distended at all; and it, therefore, is probable that the cause of its direction is to be sought in the particular disposition of the tissue of the organ. The uterus in infants is soft, and falls naturally upon itself; and we have no right to regard this circumstance otherwise than as normal, and we cannot attribute to the organ any one fixed position. In the greater number of instances, however, it was antiflexed.—*Gazette Médicale de Paris*, Nov. 25, No. 47.

ORGANIC CHEMISTRY.

8. *Gastric Juice.*—GRUNEWALD and SCHROEDER have made some observations on this fluid obtained from a peasant woman affected with fistula of the stomach. The woman was otherwise healthy, 35 years of age, weighed 53 kilogrammes, and was, at the time of observation, nursing a child; the fistula had existed for two or three years, and was caused probably by a perforating ulcer of the stomach. The quantity of the gastric juice, after abstraction of the saliva (65 grammes per hour), was computed at 584 grammes (=19 oz. nearly) for every hour, or 14,016 kilogrammes (=22½ imperial pints nearly) for the day, which is about the fourth part of the weight of the whole body, a proportion much larger than that given by Bidder and Schmidt (6·4 kilogrammes for an individual weighing 64 kilogrammes);¹ 1 kilogramme of weight of a man would secrete, according to this calculation, 264 grammes (=8½ ounces nearly) of gastric juice in twenty-four hours. The smallest proportion was obtained in the morning (state of fasting), being, however, even then not less than between 40 and 400 grammes per hour. The fluid collected at that time was, in general, thin, serous, colourless, and clear; at other periods it was more viscid, containing sometimes bile, without any accompanying signs of functional derangement; not rarely sarcina was detected in it by aid of the microscope. As regards the *chemical constitution*, according to the examination performed

¹ Die Verdauungssaeft u. der Stoffwechsel. Mitau and Leipzig, 1852, p. 36.

by C. Schmidt, the fluid obtained at an early hour of the morning (state of fasting) was either neutral or slightly alkaline; after the ingestion of food it was always acid. *Hydrochloric acid* was not found in any of the analyses of the various portions of the gastric juice secreted at different times of the day, but *butyric acid* and the presence of *lactic acid* became highly probable. The following table shows the numerical proportion of the single constituents; for the sake of comparison we add the figures given by Bidder and Schmidt for the gastric juice of the *dog* and *sheep*.

	MAN. Gastric juice mixed with saliva.	DOG. Gastric juice without saliva.	DOG. Gastric juice mixed with saliva.	SHEEP. Gastric juice mixed with saliva.
Water	956.595	973.062	971.171	986.147
Total solids	43.405	26.938	28.829	13.853
Organic substances . . .	36.603	17.127	17.336	4.055
Inorganic substances . .	6.802	9.811	11.493	9.798
Single substances :				
Hydrochloric acid . . .	—	3.050	2.337	1.234
Chloride of potassium . .	—	1.125	1.073	1.518
Chloride of sodium . . .	4.633	2.507	3.147	4.369
Chloride of calcium . . .	—	0.624	1.661	0.114
Chloride of ammonia . .	—	0.468	0.537	0.473
Phosphate of lime . . .	0.961	1.729	2.294	1.182
Phosphate of magnesia . .	0.260	0.226	0.323	0.577
Phosphate of iron . . .	0.006	0.082	0.121	0.331
Potash, belonging to the } organic substances }	0.363	—	—	—

The organic substances consisted of coagulable albuminous substance (pepsin), sugar, butyric acid, uncoagulable proteinaceous substances, and lactic acid. The organic acids are not considered as originally present in the secretion of the stomach, but as products from the ingested aliments; they are changeable in quantity according to the quality of food. The hydrochloric acid, on the contrary, was considered as an essential component, although it is not exhibited by the analyses, probably on account of its being so easily bound by the alkalis of the saliva. In an analysis performed at a period, the results of which are communicated in the appendix, Schmidt actually found free hydrochloric acid, but only in the proportion of 0.2 in 1000 parts—a proportion ten times smaller than in the dog. Concerning the question, whether the gastric juice prevents the saliva from transforming starch into sugar (as asserted by Bidder and Schmidt) the authors found that the action of the saliva is not annihilated; they confirm, however, the observation of Bidder and Schmidt, that no sugar is detected in the stomach of the dog even after the ingestion of boiled starch.

The digestion of *proteinaceous food* was examined by introducing weighed pieces of coagulated albumen, meat, &c., in thin linen bags through the fistula into the stomach, and observing both the loss of substance within a certain space of time, and the changes in the microscopic structure of the elements. Thus it was found that, for proteinaceous substances, the dissolving power of the gastric juice of man is far inferior to that of the dog; their solution is completed in the stomach of the dog within two to four hours, in that of man it requires nineteen to twenty hours. Raw meat is better digested by the stomach of man than boiled meat, and veal better than beef.

Concerning the *microscopic alterations*, they found after one hour and a half the primitive fasciculi easily separable from each other, without change in themselves, the sarcolemma destroyed; after two hours and three quarters, the primitive fasciculi commenced to exhibit transverse fissures; after three hours, only transversely striated lamellæ were visible; after three hours and a half, besides the quadratic lamellæ, many primitive fasciculi, fissured in longitudinal and transverse direction, frequently dentated at the extremities; after three hours and three quarters, the primitive fasciculi twice or three times divided in the longitudinal direction; after four hours and a half, scarcely any

solid remains left in the stomach, except some few primitive fasciculi much fissured in the longitudinal and transverse direction, still, however, exhibiting the transverse striae. After three and a half to four hours the stomach is, in general, empty; the proteinaceous substances become therefore subject to the influence of the enteric secretion, &c. As regards the digestion of *fat*, the membrane of the cells was dissolved in the stomach; but the fat itself did not undergo any alteration. *Milk*, after three quarters of an hour, formed a thick coagulum, inclosing a large quantity of milk globules and free fat; after two and a half hours, the casein was seen partly as amorphous substance, partly in transparent, membrane-like pieces, with some unchanged milk globuli; after three hours and three-quarters scarcely any remains were left in the stomach.

These alterations, which required in the stomach of this woman three hours and three-quarters to four hours and a half, were completed in that of a dog within two hours.

Finally, the authors allude to the great importance of the secretion of the gastric juice for the intermediate circulation of fluids and the intermediate change of matter, the principal object of which is to economise in the animal household, by prolonging the series of changes to which animal matter is submitted within the organism, and thus to render it for a longer time efficient in the discharge of vital processes. We find ourselves more compelled to adopt this view, as the quantity of the secretion amounted, in this instance, to one-fourth of the weight of the whole body.—*B. & F. Med.-Chirurg. Rev.*, Jan. 1855.

9. *Pancreatic Fluid*.—KROEGER gives the results of some experiments performed by Prof. BIDDER on this fluid. Through a fistula in the pancreatic duct they collected the pancreatic juice during certain spaces of time at various periods of the day and under various influences. Concerning the quantitative relations, 1 kilogramme of dog secretes, on the average, within twenty-four hours, about 89.3 grammes of pancreatic juice. According to this calculation, an adult man (weighing 64 kilogrammes) would secrete, in twenty-four hours, 5.715 kilogrammes, *i. e.*, nearly the eleventh part of the weight of the whole body. The ingestion of food exercises great influence over the secretion, the latter becoming much increased in quantity almost immediately after meals, reaching its maximum within about half to three-quarters of an hour after the meal, when it is about six or ten times larger than it had been just before the ingestion of food. Water has not the same effect: on the contrary, when taken simultaneously with solid food, it prevents the latter from causing so evident an increase. The concentration of the pancreatic juice appears frequently diminished in the same measure as the quantity is increased, but this phenomenon is not a constant one; and, at all events, the absolute quantity of solid substances is greater after meals than before.

As regards the physical and chemical character of the secretion the author does in general, agree with Bidder and Schmidt;¹ some differences, however, must be noticed. The specific gravity is, according to Kroeger, 1.01065, while Bidder and Schmidt had assumed 1.0306, and Frerichs 1.0082.² Further differences become apparent by comparing the following figures relating to 1000 parts of fresh juice.

	Kroeger. (Analysis by C. Schmidt.)	Frerichs.	Bidder & Schmidt.
Water	981.52	986.40	900.76
Solid matter	18.48	13.60	99.24
Organic substance . .	12.68	3.50	90.38
Inorganic substance .	5.80	10.12	8.86

Concerning the *physiological action* of the juice, its power of transforming starch into sugar is not doubted. According to the author's experiments, 1 gramme of the fresh juice transforms within half an hour, under the influence

¹ l. c. p. 244.

² Wagner's Handwoerterbuch der Physiologie, iii. p. 844.

of a temperature of 35° C., 4.672 grammes of dry starch into sugar; as 1 gramme of fresh juice contains 0.014 grammes of pancreatic ferment, one gramme of this ferment would transform 333.7 grammes of dry starch. If we assume, with Frerichs, that an adult man requires daily about 490 grammes (=15 ounces nearly) of starch to compensate the daily loss of carbon, the quantity of pancreatic juice necessary for the transformation of this starch into sugar would be less than 105 grammes, while the quantity actually secreted amounts to more than 5000 grammes. Kroeger is therefore of the same opinion with Bidder and Schmidt—viz: that the pancreatic juice cannot have as its *principal* function the transformation of starch into sugar. As one of the functions, he considers the promotion of the constant interchange of fluids within the body, in the same manner as Bidder and Schmidt have made it so probable concerning the saliva, and with Grunewald and Schroeder concerning the gastric juice. It further appears to him that an intimate connection exists between the secretion of the stomach and that of the pancreas—namely, that the hydrochloric acid secreted by the former is, after having performed its part, neutralized by the soda of the latter, thus again forming the chloride of sodium previously disunited by the process of secretion. In favour of this theory Kroeger observes that the hydrochloric acid secreted by 1 kilogramme of dog through the gastric juice in twenty-four hours amounts to 0.305 grammes, while that of soda contained in the pancreatic juice of twenty-four hours is calculated at 0.237 grammes, *i. e.*, very nearly the equivalent (0.259) corresponding to 0.305 grammes of hydrochloric acid.—*Ibid.*

MATERIA MEDICA AND PHARMACY.

10. *On Digitaline.*—Prof. CHRISTISON states (*Monthly Journal of Medicine*, January, 1855) that his experience of the effects of digitaline has been highly satisfactory. I have, he says, used it expressly as a diuretic only; but its sedative virtues have also come incidentally under observation; and it is quite evident that digitaline is a most energetic remedy in both ways.

In the first two trials made with it, which were both of them in cases of extensive, protracted, obstinate oedema in connection with disease of the kidneys, diuresis commenced, in one towards the close of the second day, and in the other a day later; in both the flow of urine was profuse: and in both the oedema entirely disappeared, but with the slowness not uncommonly observed in this form of dropsy.

Here I cannot help observing in passing, that a striking illustration was presented, on both these occasions, of the unsoundness of the objection which has been brought by many practitioners, since it was first propounded by Dr. Osborne, against the use of diuretics in the renal forms of dropsy. I have taken every opportunity, in my clinical lectures, and in occasional publications, to protest equally against the unsoundness of the theory and the looseness of the facts, which have led to the prohibition of diuretics in renal dropsy, and which have deprived many persons of the most immediate and most efficacious means of relief from the principal secondary affection occurring in Bright's disease of the kidneys. And I may here repeat, in the strongest terms, that I have never, except in a single instance, seen the albuminosity of the urine, or any other indications—which either the other conditions of the urine or any other local symptoms can furnish—increased under the employment of such diuretics as I have given in this disease. These are digitalis, squill, and bitartrate of potash, sometimes singly, more commonly combined. What may be the case with other diuretics I shall not pretend to decide. I strongly suspect that they are all in the same category, and that they have been shunned on grounds purely theoretical and baseless. But at any rate I can answer for the theory not holding in the case of the three diuretics I have mentioned. So, too, in the case of digitaline. It is not a little remarkable that in the first two patients to whom

I administered it, the albumen, instead of being increased, was quickly and greatly diminished. In one it disappeared entirely in a few days, and did not return so long as the patient remained under observation in convalescence. In the other it also disappeared: but after some days the albumen reappeared, though in a greatly diminished proportion. Digitaline, therefore, while it stimulates the kidneys to increased secretion, has not, in so doing, any effect, as some have thought of digitalis and diuretics at large, in increasing the peculiar renal irritation which constitutes or occasions Bright's disease.

Digitaline has proved equally, or even more serviceable in dropsy connected with disease of the heart. It has, in my hands, accomplished complete discharge of the dropsical effusions, and thus effected such relief as to enable the patient to return to his occupations, though given in circumstances apparently very desperate. In this respect digitaline has done no more than is often accomplished by foxglove itself. But it has appeared to me to act with more speed, and with greater force after the action did begin. In one instance great depression of the heart's action was brought on instead of a flow of urine.

I have also given it in some local dropsies, more especially ascites, either simple or combined with anasarca of the lower part of the body only. It has not proved more useful, however, as a diuretic in these cases than digitalis and other diuretics; which in general fail to influence the urine or remove the dropsical effusion, when given internally. I have not yet tried it externally, according to the method recommended by me with infusion of foxglove.¹

There can be no doubt that digitaline possesses the action of foxglove itself upon the heart and circulation. I have not made express trial of this action. But while using it to excite diuresis in a case of dropsy with diseased heart, the pulse began to flag, soon fell to 44, and became very soft and somewhat irregular. At the same time languor, nausea in a slight degree, and faintness ensued. All these symptoms disappeared in a few days after the remedy was discontinued. On this occasion I observed that the two actions of digitaline, its diuretic and sedative actions, do not concur. It is very likely that they are even incompatible. This I have formerly stated to be a probable fact in regard to the actions of foxglove.² The disregard of it is, I apprehend, the reason why some underrate the efficacy of foxglove as a diuretic. Attention should therefore be paid to the fact in using digitaline.

The dose to be administered is stated by MM. Homolle and Quevenne at a seventy-fifth part of a grain of digitaline, three times a day. I have never given any other dose. It is a very small one certainly. But a tenth of a grain will kill a little dog; so that the dose is only in proportion to its exceeding energy. The form I have used is that of a pill about a grain in weight. It is to be given with the conditions and precautions observed in using foxglove.

11. *Note on the Therapeutic action of Cerium.*—By J. Y. SIMPSON.—The salts of few metals were at present in our pharmacopœia, and Dr. S. saw no reason why many more might not be added to their number. He had recently drawn the attention of the profession to the salts of nickel, which as far as his observation went, presented much similiarity in action to those of iron and quinine, and seemed to be of use in cases of sick headache. He now proposed to read to the Society some very imperfect observations on the therapeutic action of some other metals; and first, as to cerium, which, given in the form of nitrate, and in one grain doses twice or thrice a day, appeared to act as a sedative tonic of considerable value, strongly resembling bismuth, and the salts of silver. He had employed it in the first instance in cases of general chronic intestinal eruption—a peculiar and intractable form of disease, for which arsenic and nitrate of silver were generally prescribed, and where these remedies had failed, cerium had been tried with marked advantage. In irritable dyspepsia, with gastrodynia and pyrosis, and in chronic vomiting, its exhibition was attended with satisfactory results; and in the vomiting which occurs during pregnancy prompt relief was afforded. It was a good tonic, and a useful substitute for the salts

¹ Edin. Monthly Journal of Med. Science, 1850, xi. p. 310.

² Dispensatory, 1842, p. 401.

of silver, bismuth, and hydrocyanic acid. Dr. S. had not employed it much in convulsive diseases, as chorea and epilepsy, in which nitrate of silver was used, but the exhibition of the salts of cerium was certainly attended with this advantage, that it could be persevered in without any fear of discoloration of the skin. As far as his experiments with cadmium went, it bore much resemblance to the preparations of antimony, and excited diaphoresis and vomiting. Tellurium, besides its expense, was precluded from being used in practice by its disagreeable effects. Dr. S. mentioned the particulars of a case where a dose had been inadvertently given to a student of divinity, and had been followed by the evolution of such a persistent odour, that for the remainder of the session the patient had to sit apart from his fellow students.—*Monthly Journ. Med. Sci.* Dec. 1854.

12. *New method of introducing medicines into the system, more especially applicable to painful local nervous affections.*—Dr. ALEXANDER WOOD has been led to introduce solutions of morphia and Batley's sedative solution into the cellular tissue, as near as possible to the affected nerve, by means of the small perforating syringe, constructed by Mr. Ferguson of Giltspur Street, for injecting aneurisms with perchloride of iron. Dr. Wood narrated nine cases in which he had employed this method of treatment, in all with perfect safety, in some with complete, in others with partial success. As to the *modus operandi* of this method of treatment, he endeavoured to show, from the experiments of Muller and others, that the effect of the local application of opium to a nerve was to destroy its sensibility at the part, and that from this action of the drug the immediate cessation of the pain arose. He then pointed out the rapidity with which absorption appeared to take place from the cellular tissue, which seemed to account for the rapidity of the narcotic effect which a small dose of opium so introduced was found to produce. He also pointed out, that other medicines might be introduced in the same way.

Dr. W. T. Gairdner mentioned, that a patient in his wards in the hospital, had been injected the other day in the way recommended by Dr. Wood. The result was not decisive, as the complaint for which the man was under treatment, viz: lumbago, had been undergoing rapid amendment, and, indeed, the day after the operation, was nearly gone. The experiment, however, was attended with little suffering, and it was noted that some degree of giddiness was almost immediately produced.—*Monthly Journ. Med. Sci.*, Feb., 1855.

13. *The So-called Chloric Ether.*—M. DONOVAN makes some interesting remarks on this article in the *Dublin Med. Press* (Feb. 28, 1855).

"Within a few years," he remarks, "medical practitioners have been in the habit of prescribing what is commonly designated spirit of *chloric ether*. To the compounder of the prescription this name causes some embarrassment: accordingly, we find that sometimes Dutch-oil, sometimes perchloride of carbon, but more generally chloroform, is employed, under the impression that these are the representatives of chloric ether. What is the apothecary to do? He is called on to supply an article which absolutely does not exist—there is no such thing as chloric ether: much confusion, therefore, results; the patient receives no benefit; and the physician administers a medicine which he never intended, and of which he has no medical knowledge.

"It is true that in some treatises on chemistry we find the term chloric ether applied to a liquid which does not deserve that name, it not being an ether at all. Chemists, no doubt, are acquainted with a liquid properly called perchloric ether; but this were rather a hazardous article of *materia medica*, inasmuch as it detonates violently, either by heating or shaking, and often explodes spontaneously.

"When chlorine and alcohol, in certain proportions, are presented to each other's action, aldehyde is produced. With an increased ratio of chlorine, an ether in very minute quantity is certainly formed, but it is hydrochloric ether, formerly called muriatic ether, not chloric ether."

This ether is difficult to obtain, and is of no use in pharmacy, for at a lower

temperature than that of summer, it assumes the form of vapour unless confined by strong pressure.

Perceiving that spirit of hydrochloric ether is becoming a favourite with medical practitioners, M. Donovan tried several simple methods of producing it, and succeeded by the following:—

“Equal measures of alcohol and liquid muriatic acid, both in as high a state of concentration as possible, were distilled by a cautiously regulated heat through a stoneware worm surrounded by snow, the liquor being received into a balloon refrigerated with a mixture of snow and salt. All the junctures were well secured as soon as the contents of the retort began to boil. Two-thirds of the original measure of the alcohol were drawn off. This liquid was poured into a retort containing a much larger quantity of dried carbonate of potash than was necessary for saturation, and the tubulature instantly closed; for if not instantly, the extricated carbonic acid would have carried off much ether. When one-fourth of the original measure of the alcohol had passed over, the distillation was discontinued. The excess of carbonate of potash retained the chief portion of the water present.

“In this case, when the acid liquor and carbonate of potash were presented to each other, an effervescence took place, because there was water in the former; but for this none would have occurred; and the acid would not have saturated the hydrochloric acid which the ethereal liquor contained.

“Another process, which afforded not only spirit of hydrochloric ether, but hydrochloric ether itself, was the following: A mixture of thirty-two ounces of strong alcohol, and sixteen of sulphuric acid, each by measure, was poured into a retort containing thirty-six ounces of well-dried common salt. The apparatus was arranged as before, and the distillation proceeded with. The first ten ounces of distilled liquor were highly acid. The retort and its contents being removed, a new one, almost filled with well-dried pearl ash, was luted to the same apparatus: into this was poured the acid ethereal liquid, and the distillation was resumed. The first two ounces of the distilled liquor consisted of ether strongly impregnated with hydrochloric acid, notwithstanding the presence of so great an excess of alkali, the effect of the latter being merely to detain the water present. Scheele had observed an analogous phenomenon in the case of sulphuric ether (*Essays*, 301).

“This ether was immiscible with water, and burned with a white fuliginous flame—a characteristic of hydrochloric ether. Several modes of detaching the hydrochloric acid were resorted to, care being taken to avoid such means as would produce effervescence. The best mode seemed to be to add a small quantity of water, equal to about one quarter of the ethereal liquor; to agitate the mixture; and when the strata separated, to draw off the upper one with a suction tube. The ether was now entirely free from acidity; but its quantity was much reduced.

“The distillation of what remained in the retort being resumed, the ethereal liquor produced was very acid.

“I found that saturation with magnesia (not carbonate) answered every purpose: but as, on each addition of this, a slight elevation of temperature took place, which caused the expulsion of a little ether in vapour, it was necessary to immerse the bottle which contained the mixture in snow after each successive quantity. This neutralized liquor being redistilled, afforded a spirit of hydrochloric ether of a sweetish penetrating taste. To this was added the ether previously obtained.

“No more magnesia must be used than is necessary for saturation; for if more be present, violent concussions will occur towards the end of the distillation.

“The liquor thus produced contains a small quantity of alcohol; it is therefore *spirit of hydrochloric ether*, and is in a fit state for medical use. It is permanent at all ordinary temperatures. It mixes readily, and in all proportions with water, which pure hydrochloric ether does not. Its taste is pungent and penetrating, but much less disagreeable than sulphuric ether. Of its medicinal properties it is not my province to speak; but from its sensible qualities, I should suppose it to possess energy. I have some reason to suspect that it

would act as an anæsthetic, and perhaps might have its advantages. The season was favourable for these experiments, they having been made during the late fall of snow."

14. *Chloroform and Ether combined as an anæsthetic.*—M. ROBERT, in his report on chloroform to the *Société de Chirurgie*, recommended a mixture of equal parts of chloroform and ether as the best anæsthetic agent. M. CELLARIES has subsequently published (*Gaz. Méd. de Montpellier*, 1853), the results of experiments made by him with the compound upon rabbits, etc.; and he reports very favourably as to its efficiency as an anæsthetic. The two fluids combine together, without the occurrence of any precipitate, and the odour of the compound is said to be far from disagreeable.—*Gaz. Méd. de Paris*, No. 96, 1854.

[This combination was first employed, we believe, in this country, and is preferred to either article alone by several of our practitioners.]

15. *New Hæmostatic Agent Invented by* M. MONSEL.—M. ARMAND, Aide-Major of the 36th Regiment, in an article in the *Gazette Médicale de Paris* (December 30, 1854), states that M. Monsel was induced to undertake the analysis of the eau Pagliari. From his first trials, the reactions made evident the presence of the sulphate of alumina and potash, associated with an organic matter the odour of which resembled that of vanilla. M. Monsel then thought that, on treating the balsam of Peru—which, as is generally known, possesses a strong odour of vanilla—with the subcarbonate of soda, and by throwing the residue of the treatment into a solution of cubic alum, he could obtain an alum charged with a principle identical with that of the analyzed water. This having been verified, he determined to test by the same treatment the resin of the gum benzoin, and the balsam of tolu. The results being the same, he came to the conclusion that the unknown fluid consisted of benzoic acid dissolved in alum. Having formed an hæmostatic agent, in his opinion similar to the eau Pagliari, he proceeded to test its coagulating power experimentally.

The results of these experiments were satisfactory.

M. Monsel has also made another very efficacious hæmostatic, by adding ten grains of tannic acid, and a scruple of alum, deprived of its iron, to an ounce and a half of rose-water.

16. *New Broth for the Sick.*—By JUSTUS LIEBIG. To prepare this broth, half a pound of the flesh of a recently killed animal (beef, or the flesh of a fowl) is chopped fine, and well mixed with a pound and an eighth of distilled water, to which four drops of pure muriatic acid, and from half to a drachm of common salt, have been added. After an hour, the whole is thrown on a common hair sieve, and the fluid is allowed to run off without pressure. The first portion, which is turbid, is poured back, until the fluid runs off quite clear. On to the fleshy residue in the sieve half a pound of distilled water is thrown in small portions. In this way a pound of fluid (cold extract of meat) is obtained, of a red colour, and an agreeable taste of broth. The sick are allowed to drink a cupful cold at pleasure. It must not be heated, as it then becomes turbid, and deposits a thick coagulum of animal albumen and hæmatin.

The sickness of a young female servant from typhus, in my house, gave occasion to this preparation. It was called forth by a remark of my medical attendant, that, in certain conditions of this disease, the greatest difficulty which presented itself to the physician lies in an imperfect digestion—a consequence of the condition of the intestines, and the difficulty of obtaining food suitable for digestion and the formation of blood. Generally, broth prepared by boiling is deficient in all those ingredients of meat which are necessary for the formation of the albumen of the blood, and the yolk of egg which is added is very poor in this substance, as it contains in the whole 82½ per cent. of water, and 17½ per cent. of egg-albumen, or a substance analogous to it, and whether this substance, in its nutritive qualities is equal to the albumen of flesh is, according to the investigations of Magendie, at least doubtful. Besides the flesh albumen, the new broth contains a certain quantity of hæmatin, and therein a large quantity of iron necessary for formation of blood

corpuscles, and, lastly, the muriatic acid for its digestion. A great hinderance to the employment of this broth in summer is its changeableness in hot weather. It undergoes fermentation, as sugar with yeast, without giving a disagreeable odour. What substance causes this change it is very desirable to ascertain. On that account the flesh must be treated with very cold water in a cool place. Icewater and cooling with ice removes this difficulty. But, above all things, care must be observed that the flesh is used fresh, and not several days old. In the hospitals of Munich and in private practice this broth has been employed with great advantage.—*Annalen der Chemie* and *Annals of Pharmacy*.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

17. *Plastic Bronchitis*.—DR. T. B. PEACOCK, Assistant Physician to St. Thomas's Hospital, and Physician to the City of London Hospital for Diseases of the Chest, relates (*Med. Times & Gaz.*, Dec. 30, 1854) a case of plastic bronchitis, in a boy 11 years of age, an out-patient of St. Thomas's Hospital, and presents the following interesting remarks on this disease, which, though rare, is of sufficiently frequent occurrence to render a knowledge of its chief characteristics a matter of practical importance.

"During the last session of the Pathological Society, I prepared a report, since published in the *Transactions*, in which I collected the particulars of thirty-four cases of fibrinous expectoration, and deduced from them certain general inferences as to the circumstances under which it occurs. Since that report was drawn up, a memoir on the subject has appeared, by Dr. Thierfelder, of Leipsic, which contains the account of a case which had occurred to the author, and notices of five others, three of which are new to me. I have also seen the reports of cases by Gendrin and Caseaux; and my attention has been drawn, by Dr. O'Connor, to two interesting communications, containing several well-observed cases, by Dr. Banks and Dr. Gordon, published in the *Dublin Journal*, which, from the unavoidably hasty way in which the report for the Pathological Society was drawn up, had escaped my notice.

"The cases to which I have now alluded, together with those referred to in the report, amount to forty-eight in number; and it would probably not be difficult still further to increase the series. I find, however, that the inferences previously drawn so closely correspond with those which an analysis of the more numerous data establish, that there would be little advantage in endeavouring to obtain a wider basis for generalization.

"1. The *plastic bronchitis*, *bronchite pseudo-membraneuse*, or *bronchitis crouposa*, as the disease has been termed by different modern writers, is of more frequent occurrence in males than in females.

"2. While the affection is not limited to any period of life, but occurs both in the young, in persons of middle age, and in the old, it is most frequent between the ages of 20 and 50.

"3. It may attack persons who have enjoyed robust health, but generally occurs in those who have been previously suffering from some chronic pulmonary disease, or who have been exhausted by other debilitating causes.

"4. The affection is characterized by the ordinary phenomena of bronchitis, aggravated, however, by the difficulty in expectorating the solid material; and it may either be acute, rapidly terminating in recovery or in death; or chronic, the peculiar sputa continuing to be expectorated, at longer or shorter intervals, for weeks, months, or even years; or it may repeatedly recur on any exacerbation of the bronchitic symptoms.

"5. Hæmoptysis, at least to any considerable extent, is by no means a frequent or even a common symptom; but, on the contrary, the membranous material is most generally expectorated, either almost alone, or mixed with the

ordinary bronchitic sputa, usually with the white, glairy, adhesive mucus which first occurs during a bronchitic attack.

"6. The membranous material is most generally of a dull white colour; more rarely of a brownish hue, or slightly discoloured by blood. Chemically and microscopically, it presents the usual characters of fibrin. In form, it may either be moulded into the shape of a bronchial tube and its ramifications, or it may constitute a thin shred of membrane. When assuming the branched or denticritic form, it may be hollow or solid, and is found to be comprised of a number of delicate laminæ, arranged concentrically. The trunk of the branched cast generally has a diameter of from one line to two or three lines, or varies from the size of a small crow-quill to that of a large goose-quill. The branches subdivide till they become very minute. It would thus appear that the casts are generally moulded in bronchial tubes of the third or fourth size; but cases are recorded in which the masses expectorated had a much larger diameter, and in which, after death, the main bronchi have been found obstructed; and others in which the false membranes extended from the smallest tubes to the trachea.

"Michælis supposed that the *bronchial polypi*, as the casts used formerly to be termed, were of two kinds: one consisting of fibrinous exudations from an inflamed mucous membrane; the other being decolorized clots, from blood retained in the tubes after hæmoptysis; and this view was supported by Dr. Cheyne. While, however, the expectoration of solid masses, assuming the form of the bronchial ramifications, occasionally follows profuse hæmoptysis, and so affords support to the idea of Michælis, I believe Dr. Watson to be correct in supposing that the fibrinous material is always the product of inflammation; and the hemorrhage, when it occurs, an accidental complication. In the Report, I have suggested that cases of plastic expectoration may, in some cases, when accompanied by hemorrhage, be analogous to the hemorrhagic pleuritis or pericarditis, the effusion not being limited to the escape of the liquor sanguinis only, but being combined with a discharge of blood-globules; while, in the ordinary form of fibrinous expectoration, the plasma only escapes. In some cases, also, the false membrane may adhere closely to the mucous membrane, and be with difficulty separated from it; in others it may have little or no connection with the exuding surface, and hence another cause of the presence or absence of hemorrhage.

"7. The ultimate result of cases of fibrinous expectoration depends upon the circumstances under which the formation of the solid matter takes place. When, as is not unfrequently the case, the membranous effusion occurs as a complication of phthisis, or of some other fatal disease, it necessarily follows the course, and may but little affect the result, of that disease. When, on the contrary, it is an idiopathic and acute affection, though the symptoms by which it is attended are ordinarily urgent and alarming, it usually soon subsides, under appropriate treatment, and the patient quickly regains his usual health. In other cases, however, the acute symptoms may subside, but the false membrane may continue to be formed, at intervals, for a long period.

"The opinion which I have just expressed of the generally remediable character of the acute plastic bronchitis, is, I am aware, opposed to the conclusion of Dr. Thierfelder, who states, that, in the acute form, the disease is generally rapidly fatal; and refers, in support of his assertion, to the fact, that of thirteen cases which he has collected, and which he very erroneously supposes to be all that have been placed on record within the last 120 years, eleven proved fatal. It is true, that the affection is occasionally attended by fever of a low typhoid type, and proves very rapidly fatal; but most generally the inflammation appears to be by no means intense; the accompanying constitutional disturbance has a sthenic character, and the urgent symptoms subside when the fibrinous material is expectorated. The unfavourable opinion as to the general result which is entertained by Dr. Thierfelder, is owing to the small number of the cases which he has analyzed, and to his having confounded with them cases of acute capillary bronchitis, in which false membranes were found in the tubes after death.

"The case which I have related will be observed to belong to the chronic

form of the disease, the patient having had several attacks before that in which he fell under my notice. Like many of the similar cases, he was a very delicate person, and inherited a predisposition to pulmonary disease. The inflammatory symptoms had probably never been very severe, and had already nearly subsided. The casts appear to have been at first moulded in the smaller bronchial tubes of the upper lobe of the left lung, but, as the disease advanced, the fibrin was either effused from the original seat of disease, in larger quantity, or the disease itself extended, so that the masses expectorated had the form of the main bronchus of that lobe. When first expelled, the solid matter formed oblong or rounded bodies, about the size of a filbert, and these, when macerated for a short time in water, gradually unfolded themselves, till their peculiar branched form became apparent; but for the complete separation and exposure of the smaller divisions much careful manipulation was required. The largest pieces expectorated had an extreme length of about three inches, and at the trunk were three to four lines in diameter. In some I was able to trace ten distinct subdivisions, and the terminal portions were most minute. Two or three masses were generally expectorated on the same day, or within two or three days, and he then usually did not bring up any more for a period of a week or ten days; but, occasionally, they were expelled more frequently, and in a day he once expectorated not less than seventeen pieces. When first observed, the membranous material was only expelled after a severe fit of coughing, but latterly they were brought up with little effort. The physical signs which were observed in the case were those of slight capillary bronchitis, with obstruction in the tubes of the upper part of the left lung, and consolidation of the corresponding pulmonary substance. There was slight sibilant rhonchus, with more or less crepitation, heard in different parts of the chest; some dulness on percussion at the upper part of the left side, especially posteriorly; and slight bronchial respiration, with mucous sounds, occasionally having the clicking or valvular character, more frequently the usual subcrepitation, in the same regions.

"These signs correspond with those generally heard in cases of a similar description. In the early stages of the disease there are the usual signs of bronchitis; and, in the later, these pass into those of obstruction of the smaller tubes and consolidation, probably collapse of the lung.

"M. Valleix states that if, when the voice is unaffected, there exist the signs of capillary bronchitis, but, instead of their being a general and abundant subcrepitant rhonchus, the *petite bruit de soupape* is heard, with sonorous rhonchus, we may suspect the presence of false membrane in the tubes; and he refers, in support of this opinion, to cases observed by Caseaux and Barth; the latter I have not been able to find; but M. Caseaux, in the report of his case, describes a sound, *de soulèvement et d'abaissement*; and it is most probable that, under the term of *bruit de soupape*, M. Valleix means the clicking or valvular sound, which was once or oftener heard in the case here related. Dr. Cane mentions having detected a similar sign in a case which he has reported; and Dr. Gordon speaks of a 'peculiar flapping valvular sound,' and 'double flapping sound,' as heard in some of his cases, and evidently regards the sign as that referred to by the French writers. This subcrepitant or valvular sound cannot, however, be regarded as very characteristic of the presence of false membrane in the tubes. Such a sound may be caused by any viscid material obstructing a large bronchial tube; but the precise nature of the effusion can only be ascertained by inspecting the sputum.

While, however, the diagnosis of the plastic bronchitis can thus not be effected by physical signs alone, there are certain general characteristics which sufficiently distinguish the disease from other forms of affection to which it bears a very close resemblance. It has been confounded with the acute capillary bronchitis; but, though in the latter affection, as in cases reported by Nonat and Fauvel, false membranes do sometimes form in the tubes, and the secretion is always very viscid, this cannot be regarded as an essential feature of the disease. Its most striking character is the tendency of the inflammation quickly to extend itself over the bronchial membrane of both lungs, and so to lead to rapid asphyxia; whereas the plastic bronchitis is generally limited

to a portion only of one lung, and the physical signs of the two diseases are very dissimilar. The false membrane, in cases of acute capillary bronchitis is also of comparatively rare occurrence; I did not once meet with it in any of the cases which I examined during the last epidemic of influenza, and when it does occur, it is rarely more than in small shreds, which gradually pass into the more usual viscid secretion.

"The acute plastic bronchitis may also be mistaken for acute pneumonia, or for a circumscribed empyema; and when, as in cases mentioned by Dr. Gordon, in addition to dulness on percussion, and absence of respiration, the heart also is dislocated, it would be difficult, if not impossible, to distinguish it from the latter disease.

"The plastic bronchitis is analogous to the form of inflammation of the mucous lining of the air-passages, which is known under the names of diphtherite, angina membranacea, and croup, in the adult, and which, commencing in the fauces, is occasionally continued through the larynx, to the terminal portions of the bronchial tubes.

"It may naturally be expected that, in this communication, some allusion should be made to the cause of the chief peculiarity of this form of disease, and that some reason should be assigned why the mucous membrane, when inflamed, instead of pouring out the usual secretion, exudes a fibrinous material. I regret, however, I have no new views to advance on this point. It is easy to state objections to the various explanations which have at different times been proposed, to show that it cannot depend on the age of the individuals attacked, nor on the intensity of the inflammatory action; that there is no proof that it is owing to the excess of fibrin in the blood, as supposed by some pathologists, or to the particular tissue affected, as imagined by others; and that the views of Reynard and Stokes, that the lining membrane of the bronchial tubes loses the mucous, and partakes more of the serous character as it approaches the periphery of the lungs, is not borne out by recent investigations. I am, therefore, compelled to resort to the unsatisfactory mode of solving the difficulty, by supposing that the exudation of fibrin depends on the specific character of the inflammation, resulting from some peculiarity in the constitution of the individuals affected, though I cannot hazard a conjecture as to the nature of this peculiarity.

"The only other remarks which I have to offer upon the case which I have read, refer to the treatment which was adopted. Before the patient fell under my notice, the period for active interference had passed, and the indications to be fulfilled, consisted in the relief of the slight bronchitic symptoms still existing, and the prevention of the further formation of the fibrinous material. For the first purpose, expectorants, anodynes, and counter-irritants were employed; and for the second, tonics, and especially the cod-liver oil; and I have, within the last few days, seen the patient, and have found him tolerably well, and free from cough or expectoration. How far, however, his recovery will be permanent it is impossible to say.

"In the cases of acute plastic bronchitis which come under the care of the medical practitioner at the commencement of the disease, the aim should be—1st, to relieve the inflammatory symptoms; 2d, to assist the expulsion of the membranes when formed; and 3d, to prevent their further formation. These indications should be accomplished by a judicious employment of antiphlogistic measures, regulated by the urgency of the symptoms and the power of the patient. It would be necessary to cup or apply leeches over the affected part of the chest, to exhibit antimony, and to affect the system mildly with mercury. The expulsion of the membrane might be assisted by the use of emetics, and by inhalations of the vapour of water, or of ethereal or other fluids; and the further formation of the membrane would probably be best controlled by mercury. In the later stage, the use of anodynes and tonics would be required.

"The use of mercury in conjunction with other antiphlogistic means is strongly recommended by Drs. Gordon and Banks; and Dr. Thierfelder, while he says little benefit results in the acute disease from a purely antiphlogistic course, approves of the employment of depletion in combination with mercurials. He also regards mercurials, the hydrochlorate of ammonia, and the

iodide of potassium, as beneficial in the chronic form of the disease. The latter remedy he employed in the treatment of the case which he has related, in very large doses. Dr. Watson, in alluding to a case of plastic sputum, which was attended with hemorrhage to an alarming extent, remarks that, though the case at first seemed to call for active interference, and was in consequence energetically treated, on looking back upon it, he regarded the treatment as having been unnecessarily active; and the remark seems applicable to a large proportion of the cases of this description. Though alarming in their first appearance, they often do not seem to require very active measures for their relief. In those cases in which the disease becomes chronic, or recurs at intervals, on any exacerbation of bronchitic symptoms, protection against the weather, and, if possible, residence in a warm climate, should be enjoined."

18. *Case of arrested Pulmonary Tuberculosis.*—By Dr. STOKES. Some years ago I saw a gentleman, who came to town labouring under all the symptoms of well-marked phthisis. The disease had been of some months' standing, and the patient was a perfect picture of consumption. He had a rapid pulse, hectic, sweating, purulent expectoration, and all the usual *physical signs* of tubercular deposit, and of a cavity under the right clavicle. I may also state, that the history of the disease was in accordance, in all particulars, with this opinion. I saw this patient in consultation with a gentleman of the highest station in the profession, and we both agreed that there was nothing to be done. This opinion was communicated to the patient's friends, and he was advised to return to the country. In about eighteen months afterwards a tall and healthy-looking man, weighing at least twelve stone, entered my study, with a very comical expression of countenance: "You don't know me, doctor," he said; I apologized, pleading an inaptitude that belongs to me for recollecting faces. "I am," he said, "the person whom you and Dr. — sent home to die last year. I am quite well, and I thought I would come and show myself to you." I examined him with great interest, and found every sign of disease had disappeared, except that there was a slight flattening under the clavicle. "Tell me," said I, "what you have been doing?" "Oh!" he replied, "I found out from the mistress what your opinion was, and I thought as I was to die I might as well enjoy myself while I lasted, and so I just went back to my old ways." "What was your system of living?" said I. "Nothing particular," he said; "I just took what was going." "Did you take wine?" "Not a drop," he replied; "but I had my glass of punch, as usual." "Did you ever take more than one tumbler?" "Indeed, I often did." "How many? three or four?" "Aye, and more than that, I seldom went to bed under seven!" "What was your exercise?" "Shooting," he said, "every day that I could go out." "And what kind of shooting?" "Oh, I would not give a farthing for any shooting but the one!" "What is that?" "Duck shooting." "But you must have often wetted your feet?" "I was not very particular about the feet," says he, "for I had to stand up to my hips in the Shannon for four or five hours of a winter's day, following the birds."

So, gentlemen, this patient spent his day standing in the river, and went to bed after drinking seven tumblers of punch every night; and if ever a man recovered from phthisis he had done so when I saw him on that occasion. Suppose, now, that he had been confined to an equable temperature, and a regulated diet, and had been treated in all respects *secundum artem*, what would have been the result? Any of you can answer the question. In point of fact, this very treatment had been adopted during the first three months of his illness, and his recovery may be fairly attributed not so much to the duck shooting and whiskey-punch, but to the tonic and undepressing treatment which he adopted for himself, and which his system so much required to enable him to throw off the disease.—*Med. Times and Gaz.*

19. *Tuberculosis limited to the Base of the Right Lung.*—The following case related by Dr. J. T. BANKS, one of the physicians to the Whitworth Hospital, is interesting, as presenting a very unusual exception to the general rule as to the primary seat of tubercle:—

"A man, aged 40, was admitted into the Whitworth Hospital; he had been ill for some time previously, but had not been incapacitated from following the arduous occupation of a labourer. The symptoms under which he suffered before his admission were cough, hurried respiration, and pain in the right side.

"On being questioned, it appeared that he had never had hæmoptysis, nor, in short, any ailment until his present, and that there did not exist any hereditary taint. A few days before entering the hospital, he perceived that his expectoration had acquired a most offensive odour.

"His appearance was that of a man who had long laboured under disease. His skin was sallow and greasy; his pulse accelerated, never under a hundred; respiration rapid; he complained of debility, loss of appetite and of night-sweats. On examination of the chest the upper regions were found, both on auscultation and percussion, to be perfectly free from any morbid sound. The base of the right lung was dull, and a coarse mucoc-crepitating rale was audible; there was also an augmentation of vocal resonance. He complained of cough, followed occasionally by the expectoration of matter which was distinctly purulent and of an abominable fetor. After a violent paroxysm of cough he often got up a very large quantity of the purulent fluid, which was always followed by relief.

"He gradually lost flesh and strength; he suffered at times from diarrhœa. The auscultatory signs of cavity at the base of the right lung became manifest, cavernous breathing was audible, with gurgling and pectoriloquy, which was so marked as to be almost painful to the ear of the auscultator. The purulent fetid fluid continued to be expectorated in great quantity, and after a residence of seven weeks in the hospital, he sank under the influence of his wasting malady. The apices of the lungs anteriorly and posteriorly, up to the close of life, were free from any sign of diseased action.

"The autopsy revealed the existence of tubercular infiltration of the base of the right lung, with numerous cavities. In no other part of the lung was there a trace of tubercular deposition.—*Dublin Hospital Gazette*, 15th Feb., 1855.

20. *The cracked pot sound (bruit de pot filé) is not always indicative of a cavern.*—Prof. BENNETT states that, "On examining a man called John McKay, on the 2d of November, 1854, it was determined on percussion of the apex of the left lung anteriorly, that it was unusually resonant, and accompanied by distinct cracked pot sound, of a clanging character. On auscultation over the part, the inspiration was harsh and exaggerated, the expiration prolonged, and the vocal resonance very loud, amounting to pectoriloquy. This unusual combination of physical signs attracted my attention. The rest of the lung, as well as the condition of the opposite one, together with the symptoms, could leave little doubt that this man was in the last stage of phthisis pulmonalis. The disease followed its usual progress, both lungs being ultimately affected, and the apex of the left one always presenting on percussion the same physical characters as have just been described, without moist rattle of any kind. He died January 6th, 1855, and on the following day his body was examined. Both lungs were scattered throughout with miliary and infiltrated tubercle, and the intervening pulmonary tissue was loaded here and there with simple exudation. At the apex of the left lung was a mass of bluish-gray miliary tubercles, about the size of a hen's egg, closely condensed together, very hard to the feel, and cutting under the knife with considerable resistance. This mass was solid throughout, tolerably limited, the pulmonary tissue around being light and spongy, containing a few very small isolated tubercles. There was no cavity or softened tubercle in this left lung, although in the opposite one, a few small anfractuous excavations existed here and there in the upper lobe.

According to Skoda,¹ "the cracked pot sound is heard in the thorax, over tolerably large and superficially situated cavities which contain air, and communicate with the bronchial tubes. When the percussion is forcible, or the thoracic walls flexible, the cavity is compressed at each stroke, and a portion

¹ Markham's translation, p. 21.

of air suddenly driven out of it into the bronchial tubes; this hissing murmur, caused by the escaping air, is mixed up with the ordinary percussion sound of cavities, and this compound represents the cracked pot sound." But the observation made in the case of McKay, has satisfied me that occasionally distinct cracked pot sound may be elicited over condensed lung, without any cavity whatever. In referring to an excellent paper on this subject by Dr. Markham,¹ I find that on one occasion, both he and Dr. Sibson have noticed this phenomenon, over the upper portion of a lung, which was afterwards shown to be gorged with blood and serum, though still retaining some portion of air. It would seem, from what has been said by Skoda, Stokes, Walshe, as well as by Dr. Markham, that a peculiar tympanitic sound may be heard over collapsed or condensed lungs, when covered or mingled with a certain amount of air. For instance, when in cases of pleuritic exudation, air is effused into the pleura, a few hours before death—when in certain cases of pneumonia there is also emphysema, etc. In the case I have alluded to, these conditions were so far fulfilled, that the tubercular mass described was surrounded by spongy lung full of air. At all events, it must be evident, that the physical conditions on which this peculiar sound depends, require more careful study, and that our ideas as to its necessary connection with a cavity, must undergo modification. —*Monthly Journ. Med. Sci.*, Feb., 1855.

21. *Absence of Chlorides from the Urine, Diagnostic of the Onward Progress of Pneumonia.*—In the April number of the *Monthly Journal of Medical Sciences*, Prof. BENNETT gave an account of certain facts which confirmed the statements of Simon, Redtenbacher, Beale, and others, as to the absence of chlorides in the urine during the onward progress of pneumonia. The professor resumed these inquiries on returning to the clinical wards of the Edinburgh Hospital last summer, and in the number of the above mentioned Journal for December last, he relates three cases of the disease there treated which confirm the diagnostic value of this change in the urine. In all these cases the absence of chlorides marked precisely the onward march of the pneumonia, whilst their presence indicated its cessation and was generally accompanied by the returning crepitation and commencing absorption of the exudation.

22. *Pathology of Saccharine Assimilation.*—Dr. GIBB read a paper on this subject before the Medical Society of London (Jan. 27, 1855), of which the following is an abstract:—

Before entering upon the pathology, he gave a brief summary of the physiology of the subject to the present time, and referred to the discoveries of Bernard, Pavy, and himself. He considered that the relation subsisting between fat and sugar, more especially in the liver, and as likely to help in the working out of the pathology of fatty degeneration, was a discovery, the importance of which could not be over-estimated. He gave a table of the pathological conditions of saccharine assimilation, and considered some of the more important. In diabetes, he supported Prout's view of the formation of quantities of sugar in the stomach, which, owing to paralysis of the converting function was absorbed into the circulation. This condition he looked upon as the result of the paralysis of the saccharine functions of the liver, which he believes to be arrested in this disease, and not over-exerted, as Bernard believes. The principal evidence he brought forward in support of this view was the invariable absence of sugar in the liver after death, and the "starved" state of the liver, from its poorness in fat. In fatty liver only did he consider the saccharine function abnormally active. The gorged condition of the portal venous system, found after death, he looked upon as owing to hepatic obstruction, due to the paralysis of the special function of the liver, from taking too much gastric sugar to that organ which it could not assimilate. If thoracic disease was a frequent termination of diabetes, it was due, he believed, to the want of the naturally formed hepatic sugar to undergo combustion, and not from excessive combustion of the sugar, as Bernard maintains. He admitted, however, that

¹ Monthly Journal for June 1853, p. 495.

diabetes and allied affections were under the governing power of the medulla oblongata and its branches, and sympathetic communications. One of the uses of healthily-formed sugar in the liver, independent of combustion in the lungs, he said, was to form fat; and he believed it to be one of the great sources of the fat throughout the system; and that, if sugar was convertible into fat, fat could not be reconverted into sugar; and he supported this view by reference to experiments. He next considered oxaluria, in connection with sugar, and believed there is a mutual relationship existing between the two, but not constant. In dwelling upon diseases of the liver, he mentioned that fatty liver was the only abnormal condition which did not interfere with its healthy function; but here it was increased, and, as the lungs did not consume it proportionate to its secretion, it was converted into fat in the liver. He drew an analogy between this condition in the marine mammalia, marine birds, etc., and the same condition in man; in the former it was a normal process, while in the latter it constitutes disease. He related some cases of disease in the body, accompanied with fatty liver, to prove that the severity of a disease elsewhere does not necessarily destroy the saccharine function of the liver. In one recent instance he found sugar in fatty liver, coincident with cancer of the same organ. In other diseases of the liver sugar is absent; in cholera, sugar is more easily detected in this organ than in health. In relation to nervous affections and saccharine assimilation, he believed there was a very great sympathy between the two, perhaps more so than in other classes of disease. He confirmed the observations of Dr. Goolden, having found the urine saccharine in many nervous affections; he had found this fluid saccharine in epileptics, even when convulsions were threatened, but which passed off; also in pertussis, both in its simple and complicated forms. He briefly went over the special conditions of the base of the brain, as likely to influence saccharine assimilation. From the length of his paper, the author was only able merely to allude to the influence of dyspepsia, gout, boils, and carbuncles, Bright's disease, tuberculosis, abscesses, diseases of the respiratory system, cholera, milk, and the effects of certain medicines, upon saccharine assimilation. The peculiar effects of each were shown in a large diagram.

Dr. Goolden expressed his concurrence in the views entertained by the author, and mentioned that in an analysis of the urine of a large number of patients suffering from nervous disease, sugar was discovered in many of the cases; but very recently, the analysis having been continued by the same person (Dr. Dundas Thompson), sugar was not found to anything like the same extent. The cholera in 1832 was preceded by a greater amount of saccharine matter in the urine of patients. He (Dr. Goolden) had traced many cases of diabetes to injury of the brain. One resulted from the kick of a horse, and another from the falling of a piece of timber on the patient. In both these cases the quantity of urine was diminished from twelve or fourteen to about five pints, below which it was difficult to reduce it when the quantity was at first considerable, even after the saccharine principle had disappeared. The kidneys, once accustomed to secrete large quantities, continued to do so. His treatment had not been so successful as he at first had anticipated, but in many cases he had materially relieved the patients. In his experience animal diet did not diminish the quantity of urine, and he always allowed his patients milk and farinaceous food *ad libitum*. He saw no benefit to be derived from prohibiting fluids. Opium influenced the amount of sugar in some cases, but not in all.—*Med. Times and Gaz.*, Feb. 3, 1855.

23. *Nature and Treatment of Diabetes Mellitus*.—Dr. F. HEADLAND read, February 10, last, before the Medical Society of London, a paper on this subject. After a slight general sketch of the symptoms, he proceeded to the inquiry, What is the physiological cause of this abnormal and excessive secretion of glucose, or grape sugar? Three chief theories had been brought forward to account for this.

1. *Theory of Renal Disorder*.—By Dr. M. Good and others of his time it was supposed that the glucose was formed by the kidney in the act of secretion. The author discussed the various alleged morbid conditions of the kidney in

diabetes, none of which are known to be constant. The discovery of sugar in the blood and other secretions of diabetics is sufficient to overthrow this theory.

2. *Theory of Saccharine Assimilation*; held by Bouchardat in France, and by the majority of physicians in England. It supposes that the formation of glucose is due to a deranged digestion or assimilation. Most consider that it is formed in the stomach; others blame the liver. This notion, also, the author disclaimed—arguing that, after a meal on starchy matters, grape sugar may be found in the blood of a healthy man; that it is part of the function of the liver to form sugar and fat out of albuminous compounds; and that this explanation does not account for the excretion of sugar, for grape sugar given to a healthy man does not pass out in the urine.

3. *Theory of Saccharine non-Assimilation*.—Supported by Mialhe, Liebig, B. Jones, and others. To this the author gave his own adhesion. It derives confirmations from the experiments of Lehmann, Dumas, and D. Thomson. The starch of the food is the chief supporter of the respiratory process. Starch cannot be absorbed without being first changed into dextrine. This is a sort of transition to grape sugar, into which it is all formed in the blood. This grape sugar is not yet in a condition to be oxidized; it is therefore again changed into two atoms of lactic acid (or some very similar material). This then combines with oxygen in the blood, supporting the animal heat by its combustion, and forming carbonic acid. These changes require certain agents, probably ferments, to effect them. Supposing they have proceeded as far as glucose, and the agency be wanting which should change this into lactic acid—then the glucose, not being available to the system, is excreted in the urine. The liver attempts to supply the want by forming glucose and fat out of albuminous food. This glucose passes also into the urine. In addition to all this waste, the very tissues are preyed upon to supply fuel for the respiration. The author then discussed at length the subject of treatment, under the following heads:—

A. *Erroneous Plans of Treatment.*

1. Attempts to prevent the formation of glucose.
 2. Attempts to hinder the excretive function of the kidney.
- The first is a natural process; the second is a healthy provision.

B. *Doubtful Plans of Treatment.*

1. The use of diuretics.
2. Stimulation of the nervous centres, as by strychnia.
3. Treatment directed to the liver.
4. The use of oxidizing agents.

C. *Correct Plans of Treatment.*

Dietetic Rule.—To supply, if possible, such articles of food as shall be able, at the same time, to nourish the patient, and to maintain the respiratory combustion without passing through the stage of glucose. (Among other things, fat and oils, dry wines, and milk, were recommended.)

Therapeutic Indications—

1. To give some remedy that shall seem to be capable of causing the glucose to undergo its normal transformations. Yeast, rennet, pepsin, etc., were discussed. The author particularly recommended *milk just turned sour*, as containing a decomposing casein, which transmutes milk sugar into lactic acid. He had advised the use of this remedy in his *Essay on the Action of Medicines*. It should be used as an article of diet; or it may also be given in enemata, and in warm footbaths.

2. To replace the urinary secretions by means of diaphoretics and purges. (The author recommended copious sweating, for a physiological reason.)

3. To attend to the general health, and to treat complications.—*Med. Times and Gaz.*, Feb. 17, 1855.

24. *The Juvantia and Lædientia in Diabetes*.—JOHN M. CAMPLIN, Esq., in a letter to Dr. BRIGHT, read before the Royal Medical and Chirurgical Society January 9, 1855, gives some particulars of his own case, believing that the means which proved beneficial to himself may afford hints useful in the treat-

ment of others. It was nearly ten years since the author was attacked with diabetes. The symptoms appeared to have been of unusual severity, and many of his professional friends viewed his case as all but hopeless. The principal interest of the paper consists in some modifications of diet, which, the author thinks, contributed most essentially to his recovery. In addition to the usual nitrogenous diet recommended to diabetic patients, and, as a substitute for bread, he first tried a cake or biscuit made of washed flour and lard; but this article soon disagreed. He then tried gluten bread; but, after a year or two, it became so unpleasant as to be insupportable, and he abandoned it, and never resumed it. Dr. Prout recommended him a bran cake, but the coarse particles of the half-ground bran acted on the bowels, and could not be continued in that form. The author devised a hand-mill, by which he succeeded in grinding the bran into a very fine powder, and in making a kind of cake, the use of which he was enabled to continue for some years. The author gave a formula by which this cake may be made, and is the same in substance as the directions contained in a foot-note in Dr. Pereira's last edition of the *Materia Medica*. The author believes that the bran cake greatly ministered to the prolongation of his life, and is desirous of stating his opinion, that some modification of this bran cake might be made on a large scale, and be used with equal success in our hospitals. The use of fat meat and eggs the author found productive of biliary derangement, which, although they might have exercised some unfavourable effect on the constitution, did not appear to augment the diabetic symptoms. The author spoke of the propriety of using curds instead of butter, and the advantage he derived from this preparation from fresh cow's milk. With regard to vegetables, he limited himself at one period exclusively to the cruciferæ. In respect of tea and coffee, the author gave the preference to the former; and, as a beverage at dinner, very dilute brandy and water; or, if wine was employed, it should be claret, as he found other wines injurious. He alluded to the remedial influence of cold and tepid sponging of the surface of the body, and the effects of warm bathing, not omitting the benefit to be derived from efficient clothing. The author's experience of other cases, as well as from his own, led him to the conclusion that there was no specific for this disease; for the cases he had witnessed had improved under the use of various remedies, keeping in view the great principle of building up the system, promoting the action of the skin, regulating that of the liver, and, as much as possible, removing the causes which aggravate the disease. Among remedies, the citrate of ammonia in the effervescing form, combined with the citrate of iron, afforded him at one period of the attack very sensible relief. The author was inclined to class the disease among the neuroses, believing that exhaustion of nervous power existed in all cases. In his own person, two causes had been long at work; the wear and tear of a laborious profession, and an ill-regulated diet; but he was not desirous of raising any discussion on this point, as his aim in the present communication had been simply to relate the facts of his own case, and the results of actual experience.

25. *Diagnosis of Typhous and Typhoid Fevers.*—By Dr. PARKES. You are brought to see this young woman, we will say, for the first time; the specific rose-spots are gone; she is labouring to all intents and purposes under severe bronchitic and chest symptoms (a chemist, or practitioner with a druggist's shop, has prescribed, and given cough mixtures, perhaps, without seeing her); you find her respiration 30 in a minute, cough incessant, with some expectoration, nervous symptoms also well-marked; vertigo complained of, torpor, the eyes closed; she is delirious at night; she has also diarrhœa, pain over the abdomen, pulse quick, tongue furrowed and somewhat coated. Suppose, I say, you were called to such a patient, and moreover she is unable to give any account of the previous illness, how are you to make the diagnosis? There are only two ways—one the positive method, the other the method as it is called by "exclusion." The first is obvious enough, and will of course be more valuable to the practised eye of the experienced physician, who seizes the nature of the case at the first glance by a sort of intuitive knowledge of what typhoid really is. Now the method of diagnosis by exclusion—the plan of logic-writers, *per viam exclusionis*, in this and other

diseases, is one, though not without disadvantage, one yet of no mean importance. The first question you resolve in your mind will be—is she or he, as the case may be, labouring under any of the idiopathic fevers? any of the exanthemata? No. Is it typhous? You make the same answer, as the eruption in *ty-phus* is as different from *ty-phoid* as scarlatina from measles. The eruption is absent in patients under 22 or 21 (this patient's age is about this). Is it relapsing fever, so common some years, as 1828–29? No. You ask yourself, then, is it typhoid? Yes. Nervous symptoms are marked, chest symptoms and diarrhœa also; the latter loose, granular, yellow, so peculiar to typhoid. You have soreness of the right iliac fossa; but then you say we have no rose-spots, and then you remember in at least 20 per cent. these rose-spots are not found. You must weigh and balance all these circumstances in your mind.—*Med. Times and Gaz.*

26. *Non-identity of typhous and typhoid fever.*—Although typhous and typhoid fevers resemble each other very much in their external appearances, Professor Forget of Strasburg believes that they are clinically and essentially different in their nature. They are anatomically different; they are not equally frequent in their occurrence; and typhoid fever may arise from many causes, quite irrespective of infection. In follicular enteritis the typhoid condition is not constantly present, and it is usually secondary; while in typhous, the prostration and stupor, attending its invasion, indicate a powerful intoxicating principle, acting primarily as a poison, which is not usual at the commencement of the typhoid state. Gastro-intestinal symptoms are more constant and primitive in typhoid than in typhous fever; for in it they are occasionally found existing alone; and we very rarely see cases of follicular enteritis which do not present the rough (*saburral*), punctulated, and rosy tongue; also *gargouillement* and pain in the right iliac region, and either diarrhœa or constipation. Though other symptoms may coexist with them, they are never wholly obscured. In typhous again, especially at its outset, the tongue is frequently moist, smooth, and white; meteorism and abdominal pain are absent, and there is no derangement of the process of defecation. If we except the rosy, lenticular spots, peculiar to typhoid fever, and which constitute, according to M. Forget, its most constant differential sign, there are no symptoms in general connected with either malady which are not purely accessory and inconstant. Typhoid fever is usually slow, and gradual in its course, and in simple uncomplicated cases its evolution is in exact ratio to the development of the intestinal lesion. In typhous, on the other hand, the disease is very violent at first, and afterwards becomes suddenly or alternately better or worse, and often in those cases which appear the most unfavourable, it advances rapidly and spontaneously to a favourable termination. Ataxia characterizes it more than it does typhoid fever.

As regards the treatment of the two diseases, Professor Forget says that while, in typhoid fever, this must always be adapted to the peculiarities of each particular case, special attention must be paid to the intestinal ulcerations which are present during its course, and that all those medicines should be avoided which are calculated to aggravate these lesions of the alimentary canal. There is no need for this caution in cases of pure typhous.—*Monthly Journ. Med. Sci.*, Feb., 1855, from *Gaz. Med. de Paris*, Nos. 42–48. 1854.

27. *Some Points in the Treatment of Gout, Rheumatism and Rheumatic Gout.*—Dr. FULLER read a paper on this subject before the Western Med. and Surg. Soc. of London. He commenced by stating, that in all diseases, but especially in those which, like gout, rheumatism, and rheumatic gout, are attributable to faulty assimilation and the consequent formation of a morbid matter in the blood, the success of treatment must necessarily depend upon the care and judgment bestowed on four several points—first, the history of the case; secondly, the appearance and general condition of the patient; thirdly, the character of the excretions; and, fourthly, the selection and adaptation of proper remedies to meet the various exigencies of the case. He then proceeded to point out the bearing of each of these considerations upon the course of treatment to be pursued, and after drawing attention to the close relationship

subsisting between the amount and character of these various excretions and the condition of the system generally, he insisted on the necessity of providing for the full and perfect action of all the excretory organs. He instanced cases which had fallen under his care at St. George's Hospital, in which the long continuance of the disease was evidently referable to the sluggishness of some one particular organ; and others in which the obstinacy of the symptoms was attributable to the imperfect action of all the excretory organs, rather than to extreme sluggishness in any one of them. These latter cases, when not attended by fever, and when characterized by affection of the muscular structures, are often benefited by bark and hydrochlorate of ammonia, which, under such circumstances, has the effect of promoting an increased flow of all the excretions, and so of eliminating the *materies morbi* and checking the disease. Dr. Fuller, however, dwelt especially on the fact, that under different conditions of the general health, very different remedies are needed to regulate the various excretions. He showed, that when the system is low and deficient in vital energy, tonics, such as bark, quina, sarsaparilla, steel, and cod-liver oil, together with change of air, change of scene, freedom from anxiety, and the use of proper baths, and a due amount of exercise, form the most effectual means of checking the mal-assimilation on which the formation of the poison depends, and of increasing the action of the excretory organs, whereby its elimination is brought about; and that, unless such remedies are exhibited, and the tone of the system is improved before colchicum, guaiacum, and other so-called "specifics" are administered, no good results can be produced. He attributed the failure of treatment, in many instances, to want of care in tracing out the history of the case, whereby it happens that purely neuralgic pains, as well as the pains which follow the incautious use of mercury, and the pains and swellings resulting from a gonorrhœal taint, are confounded with gout and rheumatism, and are wrongly treated accordingly. He regards the too frequent habit of treating disease in accordance with its name, rather than according to the condition of the patient and the character of the secretions, as another cause of failure, inasmuch as it leads to the practice of prescribing each of the so-called specifics separately, and apart from other remedies. He pointed out, that, in most cases, the action of all the excretory organs is disordered; and that, whereas no single remedy will excite them all equally to action, the judicious practitioner should employ such a combination of remedies as will prove efficacious for the purpose. Dr. Fuller combated the opinion advanced by Dr. Garrod as to the non-existence of such a disorder as rheumatic gout; and avowed his belief that the three forms of disease which form the subject of his paper are referable to different forms of mal-assimilation and the consequent formation of different poisons in the blood; and he urged the remarkable differences observable in the age, sex, and physical condition of the sufferers from rheumatic gout, in the ascertained causes of the disease, the mode in which it makes its attacks, its symptoms, its behaviour under treatment, and the pathological changes to which it gives rise, as evidence in favour of its being a disease essentially distinct both from gout and rheumatism. He admitted the value of Dr. Garrod's uric acid test as a means of distinguishing, in most instances, between gout and rheumatism, as well as between true gout and rheumatic gout; but he did not admit the absence of uric acid from the blood as proof of the disease being pure rheumatism. Moreover, admitting the value of the test theoretically, he denied its practical utility. Its only indicational value is in regard to the administration of colchicum, and even in this respect it cannot be trusted. In no cases is lithic acid more abundant in the blood than in those old cachectic cases of gout, accompanied by the formation of chalk-stones in the joints and by granular degeneration of the kidneys, yet in none is colchicum of less avail or more ill borne by the system. On the other hand, when the blood is free from lithic acid, as it usually is in true rheumatism, colchicum is often of great service, by acting as an evacuant and influencing the secretions of the kidneys, liver, and bowels. Colchicum, therefore, should not be relied upon in all cases of gout, neither should it be discarded in all cases of rheumatism; and in the one as in the other, the history of the case, the condition of the patient, the stage of the disease, and the cha-

racter of the excretions, must be our guides to the administration of that or of other remedies.—*Med. Times and Gaz.*, Feb. 3, 1855.

28. *New remedy for Pruritus Vulvæ*.—Dr. SCHOLZ recommends an Indian plant, the *Caladium seguinum*, which is used by the natives of India as an anaphrodisiac, for the treatment of those cases of this most distressing malady, which are due rather to a hyperæsthetic condition of the parts, than to any lesions of the mucous membrane of the vulva. The plant belongs to the *Aroideæ*. Scholz has used it with great success in two cases which had previously resisted all remedies; and he administered it in the form of an alcoholic tincture in doses of six drops.—*Arch. Gén. de Méd.*, Sep., 1854.

29. *Anaphrodisiac properties of Bromide of Potassium*.—THIELMANN recommends this remedy as an excellent anaphrodisiac in satyriasis, in the frequent and painful erections during gonorrhœa, in spermatorrhœa, and in nymphomania. He administers it in doses of from 2 to 3 grs. every two or three hours; and, at the same time, enjoins a mixed vegetable and milk diet, and forbids all acid substances.—*Med. Zeit. Russl.*, 1, 1854.

30. *Sulphate of Quinine in Phthisis*.—D. MUNTENDAM, a Dutch physician, has just published a paper to show that, from his experience in twenty-two cases, sulphate of quinine, given along with acetate of morphia, or even alone, is capable of prolonging the life of the patient in many cases of phthisis, and that it may even effect a cure in those cases in which a tubercular deposition has just commenced, especially in married women and children. He alleges that, when given continuously in small doses, it does not cause dyspnœa, diarrhœa, headache, or any disagreeable effects; and he believes that in very many, but not in all cases of phthisis, it should be ranked as one of the best remedies for the disease.—*Monthly Journ. Med. Sci.*, Feb., 1855, from *Nederlandsch Lancet*.

31. *Borax injections in Infantile Diarrhœa*.—M. BOUCHUT considers infantile diarrhœa to be of two kinds; the one, symptomatic of lesions of the intestinal mucous membrane; the other, idiopathic, a nervous or catarrhal flux from the great intestine, which may occasion death, without leaving any material morbid appearances. In the latter variety, M. Bouchut recommends clysters containing the bi-borate of soda, as peculiarly efficacious. This remedy proves as beneficial as it does in aphthæ of the mouth, it acts as a weak astringent on the intestinal mucous membrane, and as an alkali in neutralizing the acid secretions poured out by it, which lead secondarily to ulceration of the bowels, and especially of the anus.

He uses clysters containing from 10 to 20 grammes of the salt, in 125 grammes of water.

In cases of infantile diarrhœa symptomatic of intestinal ulcerations, M. Bouchut has found benefit from the use of borax, administered internally in the dose of 2 grammes, in 80 grammes of a mucilaginous emulsion.—*Gaz. des Hôpitaux*, 16th Sept., 1854.

32. *Trial of Phosphorus in Paraplegia*.—Prof. BENNETT has given phosphorus in seven cases of paraplegia from disease of the spinal cord, in the form of phosphuretted oil (4 gr. of phosphorus dissolved in 3j of olive oil.) In none of these cases has he been able to satisfy himself that any improvement was occasioned. He commenced with three drops a day, which were afterwards cautiously increased to ten, and in one case to fifteen drops. But these large doses soon induced violent nausea and vomiting, and, after a short suspension of the remedy, he continued it in doses of three drops.

In the case of chronic myelitis, which took ten and fifteen drops, the phosphorus was excreted by the lungs, as the breath smelt strongly of the drug, but was not phosphorescent at night—a phenomenon which has been seen by some physicians who have employed it. In another case, that of a delicate woman, with probably congestion of the cord, a large amount of phosphate was passed in the urine presenting, under the microscope, beautiful feathery crystals.

The dose of the oil, therapeutically, ought never to exceed five drops, and even this amount cannot be administered for any length of time without deranging the stomach.—*Monthly Journ. Med. Sci.*, Feb., 1855.

33. *Therapeutic Action of Lupulin*.—Debout's observation on the favourable influence of lupulin in quieting painful erections induced Dr. ZAMBUCO to institute some experiments with the medicine, which, to prevent mistakes, he administered to the patients with his own hands. The form was threefold; pure lupulin, the tincture, or mixed with an equal part of sugar. The doses were different: of pure lupulin, 1—16 scruples (these high doses caused no disturbance of the nervous system); the tincture, 2—16; the mixture with sugar, 2—3. From eight well-marked cases of painful erection, consequent on gonorrhœa, out of a much larger number, the author was enabled to draw the following conclusions: Lupulin possesses a remarkable effect upon the genital organs; the erections were quieted in four-fifths of the cases. It is of therapeutic utility when there exists the necessity for keeping the penis in a state of rest. Camphor given in large doses, with a similar object, both irritates the alimentary canal, and often fails in its end. The sedative and anti-blennorrhagic influence of lupulin depends upon a resinous, æthero-oily principle; while the bitter principle yields a real tonic. The author has seen lupulin given with good effect in scrofula.—*Med. Times and Gaz.*, Feb. 3, 1855, from *Schmidt's Jahrbuch.*, 1854, No. XII.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

34. *Curability of Cancer, and its Diagnosis by means of the Microscope*.—[In our preceding No., pp. 224—231, we gave the first portion of the discussion at the Academy of Medicine on this subject, and we shall now present the continuance of it.]

M. JULES CLOQUET thought, with M. VELPEAU, that the use of the microscope in the diagnosis of cancer, threatened to divert professional attention from clinical observation. The essential nature of cancer—not only of the encephaloid, but of all other varieties—was to reproduce itself with all its primitive characters. He had operated on a great many epithelial cancers of the lips, almost all of which had recurred; and he had removed many fibro-plastic tumours, several of which had returned. Cancers varied from one another only in their degrees of malignancy and fatality. He regarded the melanotic as the most formidable species, and could remember no instance in which he had removed a cancer of this kind without its having recurred. Next in order to it in malignancy he ranked the encephaloid, fibro-cartilaginous, fibrous, colloid, cartilaginous, epithelial, and fibro-plastic varieties. The danger of reproduction varied according as the cancer was encysted or ramified, ulcerated or not so, complicated or not with glandular engorgements, &c. We were constantly finding our prognosis to be fallacious in cases of cancer. In illustration of this he cited several cases in which malignant cancers of the breast of unfavourable prognosis had been removed, without any recurrence of the disease.

He thought the following conclusions were warranted: 1. That microscopic examinations should never be allowed to supersede clinical observation; but that both should, in every case, be resorted to. 2. That tumours, having all the characters of cancer, could occasionally be removed without recurring, although their prognosis was most unfavourable. 3. That other tumours might return, the circumstances of which were apparently the most favourable. 4. That although there were cases in which an operation seemed clearly indicated, and others in which it was as clearly counter-indicated, there were many others in which the most experienced practitioner could not decide the question with any degree of certainty or satisfaction.

October 24.—M. ROBERT.—My remarks on this occasion will still continue to be strictly practical in their nature, and I shall endeavour to demonstrate clinically the utility of the distinctions pointed out by the microscope—in the use of which I confess myself to be by no means perfect, but the employment of which I have several times found to be highly useful for the elucidation of doubtful cases.

First of all, let me allude to M. Velpeau's address. In it he stated that there are statistics on record which are directly opposed to those of Munro, Mayer Boyer, Macfarlane, &c., which I quoted. He alludes to those of Hill and Flajani; but these, in my opinion, are not worthy of being placed in opposition to those which I cited. But even granting that they do counterbalance the statistics I quoted, this does not in the least affect the evidence borne by the experience of Lebert and Broca. The conviction which I have expressed, therefore, seems to be supported by the opinions both of ancient and of modern surgeons.

The cases brought forward by M. Velpeau from his own private practice are divisible into two classes: 1. Those of a remote date (of the nature of which I have expressed some doubts), and 2d. Those of recent times; while the cures were not of more than six, five, three, and two years, at the publication of his *Maladies du Sein*. Those of the second class I frankly accept as true cases of cancer, having not only been pronounced such by an authority in whom I have every confidence, but having also been demonstrated to be of this nature by means of the microscope. The cases included in the former and more numerous class have not been defended with any great warmth by M. Velpeau. I am still of opinion that M. Velpeau has not yet waited long enough to be able to announce the radical cure of those rare cases of true cancer in which he has operated. We know that recurrence may take place after a long space of time, and M. Velpeau has himself given two instances of distant recurrence in his work, in one of which it happened at the end of fourteen, and in the other at the end of twelve years. But further, M. Velpeau has detailed four cases, one of which occurred in 1847, the others in 1848 and 1850, in which tumours containing cancer cells were removed, and the patients recorded as cured. Chance has thrown in my way intelligence respecting one of these cases; a lady, who was seen by one of the first surgeons in Paris, and who died last year of a relapse of her malady.

As to the clinical infallibility of M. Velpeau, he grants that there are difficult cases, and, when he says that he is never deceived, he means in the most marked cases of cancerous or other tumours. In this respect, however, he is no farther advanced than the rest of us. But in the two extreme phases of the evolution of mammary tumours, for example, our diagnosis is surrounded by almost insuperable difficulties. How can we always distinguish, with certainty, a small encysted mass of an encephaloid nature, which is mobile, unadherent, and which rolls below the fingers, from an incipient hypertrophic tumour? Who has not met with such cases? But let us, on the other hand, suppose the case of an enormous ulcerated tumour, with everted and fungous edges, with profuse fetid suppuration, the hemorrhages from which have undermined the general health—and all this occurring simultaneously with general cachexia. M. Velpeau has said that he would declare such a tumour to be cancerous, suppose all the microscopes in the world were to testify against him. I deprecate this singular mode of demonstration. Less confident or more inquisitive than M. Velpeau, I appealed to the microscope in a case similar to this which I met in 1849, and it showed me that this horrible tumour was composed only of the hypertrophied mammary tissues. I operated, and the patient has ever since enjoyed perfect health. How precious to us, then, is a method by which we may rectify our first impressions! No one has a higher estimate of the sagacity of our eminent colleague than myself, but after clinical examination of a morbid tumour with the naked eye, he is often, like ourselves, left in doubt as to its true nature. This I can prove. At the last meeting of the Academy, the members were deeply interested by M. Velpeau exhibiting several tumours which he had removed, and the nature of which he had diagnosed before operating. In these cases examinations with the naked eye and by means of the microscope, testi-

fied once more to the ability of our colleague. But one of the specimens puzzled M. Velpeau; and the diagnosis of it then, as well as before the operation, was very obscure. The tumour resembled neither encephaloma, nor schirrus, nor colloid cancer—in spite of the gelatinous aspect of its tissues—nor had it much likeness to an adenoid tumour. M. Velpeau regarded this doubtful tumour as of benign nature, but added that he would never be surprised to hear of the ultimate appearance of a similar tumour elsewhere. Thus, out of six specimens, taken at random, he is unable to determine the nature of one, by ordinary means. In his embarrassment he consults two microscopists, one of whom finds cancer cells, and the other discovers none; and M. Velpeau, as much in doubt as ever, doubts the efficacy of the microscope. Had I been present on that occasion, I confess that my convictions would have been strongly shaken. But I have since then been fortunate enough to obtain accurate and precise information respecting this tumour, from the most competent authorities.

(Here M. Robert read notes, which he had received from MM. Robin, Verneuil, and Houel, which showed that these three able microscopists, after having severally demonstrated identically the same structures in this tumour, had unanimously concluded that it contained no cancerous elements, and that it was merely a particular variety of mammary hypertrophy.)

Since receiving this information, said M. Robert, I confess that I feel much more convinced of the value of the microscope.

The question before the Academy as to the *curability of cancer*, lacks a proper basis. For, to know whether or not a disease is curable, we must know exactly what this malady is; we must carefully distinguish it from those affections which are liable to be confounded with it; in a word, we *must define it*. Hence, on beginning this discussion, we ought to have set out with a clear definition of what cancer really is—which task all the orators have evaded. Owing to the difficulty which has hitherto existed in assigning to this malady precise and pathognomonic characters, we have merely recorded its most striking and constant phenomena; that is to say, as a substitute for a dangerous definition, we have given a summary description of the evils which attend the evolution of cancer. This hiatus is unfortunate, inasmuch as it has led, and must always lead, to the greatest confusion. And hence it happens that the word *cancer*, instead of designating a pathological condition, and its simple varieties, is the qualifying attribute of an immense class of very different maladies. This family is, by common consent, composed of elements so diverse, that in looking to their clinical characters, and keeping their structure altogether in the background, we are obliged to admit (as has been done by MM. Leblanc, Velpeau, and Cloquet) the existence of a great many genera, which are themselves subdivisible into species—thus rendering the classification of limits quite indeterminate and arbitrary.

But, gentlemen, our venerable instructor, Bichat, long ago laid down the following law, which has remained immutable amid all the revolutions of science: When two diseases differ in their symptoms, their course, and their termination, they also differ in their seat, and in the nature of their accompanying lesions; and reciprocally, when the anatomical lesions vary, and when the structure of the pathological products is dissimilar, then the functional disorders which cause their evolution, are distinct in their nature.

Taking this immutable principle, then, for our groundwork in the study of cancerous disorders, if we find that clinical observation does not afford us the elements of satisfactory distinctions, and that their physiological characters are not sufficiently general—admitting of too many exceptions, and containing too many varieties—then it behoves us to study their structure. But are the means afforded us by pathological anatomy, in the present day, sufficient for the prosecution of this study? I unhesitatingly say, No. Persevering and laborious investigations of tumours have doubtless produced very useful documents, but they yielded very insignificant results. To these researches we owe that useless, but copious, phraseology, which at present encumbers pathology; such words as colloid, tuberos, ligneous, napiform, chondroid, pustulous, encephaloid, schirrous, lardaceous, medullary, fungus hæmatodes, cancerous and

phagedenic ulcers, *noli me tangere*, &c. What signifies to us the hardness or softness of a tumour, or the sound it gives when cut, if not decisive?

There can be no doubt that we still need to be possessed of more delicate characteristics of cancer. But whence are we to get them? Chemical analysis has been unable to give them. It was once hoped that we might gain our end by scraping the surface off a section of a tumour, so as to demonstrate the presence or absence of a particular creamy or lactescent juice. M. Barth attached great importance to this juice, and considered it almost pathognomonic of cancer. But an analogous fluid is found in some tumours which are not at all cancerous in their nature, and these exceptions prove that this test is not by any means conclusive.

Such was the state of science when the microscope came upon the field. It proposed a precise and brief definition for cancer, and declared it to be a malady constituted by the presence of a special anatomical element, which had no analogue in the economy, and the characters of which were easily recognizable by the practised eye. The microscope informs us that *that* only is a cancer which contains the cancerous element; that this element combines with the various simple and composite tissues of the economy, and that it is from the occurrence of this mixture in different proportions that the innumerable varieties of cancer result. It further declares that there are other morbid tissues which, at first sight, are apt to be confounded with cancer, and that their development in the economy often indicates great danger, and may even occasion death. But instead of being constituted, like cancer, by the presence of an anatomical element, without any analogue in the economy, *i. e.*, heteromorphous in its nature—these tissues are merely the result of an exaggerated yet circumscribed growth of one or more of the elementary tissues which enter into the normal constitution of the organism.

Finally, I would have you remember that the laborious microscopical investigators have made their clinical observations keep pace with their histological researches. Their practised eyes having discovered, in these morbid tissues, structural peculiarities which had escaped the earlier observers, they proposed an entire revision of this part of our nosology, inasmuch as they found that those diseases structurally resembled others totally distinct from them in their seat, origin, prognosis, &c.

What ought the partisans of the clinical school to do in the case of so notable a deviation from the great precept of Bichat which we have enunciated? We ought either to expose firmly the errors of the new doctrine, if such exist, or to confess frankly and honestly the progress which the microscope has achieved.

I shall now proceed to state to you what I have learned concerning this question, both from my personal observations, and from the attentive perusal of modern works; and if I succeed in demonstrating to you that cancerous, epithelial, and fibro-plastic tumours are not identical affections, I think I will have sufficiently proved the utility of the microscope, which alone can reveal to us precise and minute distinctions.

You are aware that, according to the microscopists, the cancer-cell is the specific characteristic of cancer. I will not pause to discuss, as M. Velpeau has done, the relative value of the cells, the nuclei, and the nucleoli; for, although I have certain convictions of my own on the subject, I think it is better to leave questions like this to those men who have studied them *especially*, and not to meddle with topics about which we know little practically. You will perhaps say that I am adopting the definition of cancer given by the microscopists, totally forgetting that my opponent refuses to accept their opinions. M. Velpeau has said that the cancer-cell cannot be the specific anatomical character of cancer, because it has been found in non-cancerous tumours. I would ask my learned colleague by what authority he declares the tumours which contained the cells in question, not to have been cancerous? Because, he will reply, they did not return after extirpation. From this it would follow that every tumour which does not recur after removal, although it contains myriads of so-called cancer-cells, is not cancer; and from this I would also be correct in concluding that the so-called cases recorded in M. Velpeau's work, at p. 585, were not cancers at all. And, by carrying this mode of reasoning still further,

I could prove that my colleague has not, any more than Munro and Macfarlan, cured a single case of cancer.

The physiological are not less marked than the anatomical characters of cancer. The disease has a constant tendency to become generalized, and manifested elsewhere, either in the neighbouring lymphatics, in some other organ, or in the viscera themselves, especially in the lungs and the liver. Before cancerous tumours ulcerate, and injure the constitution by profuse suppuration and hemorrhages, we often see the signs of general cachexia become manifested. The malady constantly returns after extirpation, and the progress of cancer becomes much more rapidly fatal in exact proportion to the frequency of its recurrences, and the operations for its removal. I have often noticed this fact, but was never more struck by it than in the case of a woman, aged 38, who, after four successive ablations of a small tumour of the mamma, had three recurrences of the malady in less than two years, and who returned to the hospital three months after the fourth operation, where she died from the recurrence of a multitude of similar tumours within the chest.

Fibro-plastic tumours are always circumscribed, and have not the irregular ramified (*rameuse*) form which characterizes certain cancers, especially schirrus. On their first appearance they possess a firm, slightly elastic consistence; they are homogeneous in structure, and contain no juice. Their tissue is termed by Lebert *fibro-plastic*, and it is composed of cells, and of fibres which exactly resemble the elements of cellular tissue during their development. These tumours advance but slowly, and they are, for a long time, quite compatible with all the appearances of good health. Nevertheless they may become softened, ulcerated, and even generalized, and they may occasion death by being developed in the viscera. Of these facts M. Velpeau has given us several examples. But even among those rare cases in which the malignity of the fibro-plastic tissue rivals that of true cancer, we find consoling facts, which M. Velpeau ought not to have passed over in silence. Of these facts, which are now very numerous, I shall mention a few.

Paget (in his *Lectures on Tumours*, 1853) mentions a case quoted from Mac-lagan, of a girl, who, in 1832, was operated on for a firm, elastic, fibrous-looking tumour, in the left lumbar region. It returned after a year, and was again removed in 1834. A second recurrence happened after a period of eighteen months, and the third operation was completely successful; for in 1850—fifteen years after it—she was, and had continued until then, in perfect health.

In 1839 I removed, from the right axilla of a young man, a large tumour, the size of both fists, which was circumscribed, lobulated, but firm and elastic, and appeared to be composed of fibrous tissue intermixed with fat. In five years thereafter it returned, and I operated again in October, 1844, removing a tumour, the size of my fist, possessing the very same appearance as the first. Again it returned after the lapse of other four years, and for the third time it was extirpated in January, 1849. M. Lebert examined the tumour, and pronounced it to be fibro-plastic tissue mixed with fat. Since this time the patient has enjoyed perfect health.

Again, Mr. Syme, in 1839, removed a tumour, situated over the first rib, from a man aged 48. A second operation was necessary in 1843, a third tumour was removed in 1847, and a fourth in 1849, and a fifth in 1851. Professor Bennett examined these tumours microscopically, and found them to be composed of fibro-plastic tissue. Shortly after this fifth operation another tumour, of a considerable size, was formed under the clavicle. During all this time the patient's general health was never impaired. Ultimately this tumour ulcerated; hemorrhage took place; and, worn out by it, the patient died in July, 1852. *There was not a trace of the malady to be found in any of the internal organs.*

In September, 1849, M. Chassaignac removed a tumour from beneath the left clavicle. In a few months it reappeared, and a second operation was performed in December, 1850, a third in July, 1851, a fourth in January, 1852, a fifth in July, the same year, and, finally, a sixth in October, 1853. Since that date, M. Chassaignac has not seen the patient. During the course of these operations the general health continued excellent, and there was no implication of any of the lymphatics. The tumours were examined each time by M. Lebert, who

pronounced their structure to be purely fibro-plastic, mixed with a little gelatinous matter.

When we reflect on the course of all these fibro-plastic tumours, on the obstinacy of their recurrences, and on the fatal termination of many of them, we certainly cannot regard them as of a benign nature. But, on the other hand, are we correct if we class them with cancerous tumours? Has an encephaloid or a schirrous tumour ever been extirpated for four, five, or six recurrences, without the disease becoming generalized, and the constitution cachectic? And can the instances of fibro-plastic recurrence in the viscera be compared, for frequency of manifestation, to the more constant return of cancer in the glands, and within the cavities of the chest and abdomen?

I would next speak of *epithelial affections*, which are composed anatomically of epithelium cells or of the epidermis which normally covers the skin and mucous membranes. I cannot conceive how M. Velpeau can say that the structure of such tumours (if we are to believe the microscope) renders them akin to benign growths, such as warts, corns, &c. What does it signify, that two tumours have in common a normal anatomical element, if this element be not disposed and combined in the same manner in each? In warts and corns, the epidermis, though more abundant, stands in exactly the same relations to the other parts of the skin as it does in the normal condition of the parts. We will find, on examination, that this is not the case in an epithelial tumour. M. Velpeau's assimilation, then, is more specious than real.

Epithelial tumours appear principally on tegumentary surfaces, and ought to be studied separately according as they are situated on the skin or the mucous membranes. On the skin they begin by small crusts (*boutons croûteux*), warts, or fissures, and they form ulcers called *canceroid*, on account of their differing in appearance from cancer, progressing more slowly, and having less tendency to infect the system. The utmost evil that they do is to affect the neighbouring glands. When treated by energetic appliances, calculated rapidly to destroy the indurated parts, they often become healed definitely and radically. The epithelial tumours of the mucous surfaces (as types of which I shall specify those of the tongue and cervix uteri) are different in their progress; their course is more rapid; they reappear almost invariably after extirpation, and they terminate by causing death. Their tendency to generalization, however, is very far inferior to that of true cancer. If we examine the bodies of those women who have died of these deep ulcers of the cervix, we may often find the uterus partly destroyed; and the bladder, peritoneum, and great blood-vessels may be perforated by the disease. But, in such cases, we never find epithelial masses deposited in the viscera; the utmost traces of the disease which we can discover are the slightly enlarged glands in the neighbourhood of the uterus. It is the same with epithelial ulcers of the tongue; the glands of the neck alone become affected; and, when the patients die, it is rather from being exhausted by hemorrhages, and from the long-continued deglutition of putrid matters, than from any generalization of the disease having occurred.

At the orifices of the body, where the skin and mucous membrane become blended together, epithelial tumours possess, what I call, *mixed* characters, which they communicate to the complexed structure of the tissues affected. But there, as on the skin, the tumours continue for a long while in the condition of local maladies; and although it may be proved that their extirpation is less frequently curative than of those on the skin, it can also be demonstrated that the radical cures effected by the operation are very highly encouraging. I am quite aware that epithelial tumours *may* become propagated or reproduced in the lymphatic glands, and even in the viscera; but such instances of generalization are very rare.

These, then, are the differences, both of the minute structure and the mode of progress, of the three lesions which are generally confounded together under the term—cancer. We cannot consider epithelial and fibro-plastic tumours as benign in their nature, and we certainly cannot, in point of malignity, rank them with cancerous maladies properly so-called; M. Velpeau does not admit these distinctions, for to him all these lesions are cancers, and he does not recognize the speciality of any of their elements. If the patient *must* sink, "what

does it matter," he asks, "in *what manner* he dies—whether by the disorganization of the diseased parts or from the results of general infection?" I would answer, in the first place, that all these maladies are not alike in their progress and severity; but even supposing that they *were* all equally fatal, is it not important to study them, in order to establish the differences between them, and to endeavour, sooner or later, to apply the knowledge thus gained to their scientific treatment? I feel convinced that, at present, the only question concerning cancer that is at all settled is that of prognosis. Is it nothing for us to know that although the removal of a cancer leaves the patient exposed to the danger of an almost certain recurrence, sooner or later followed by death—that the extirpation of an epithelial or fibro-plastic tumour is much more hopeful and satisfactory in its results? If, in the latter case, a return of the disease should take place, the surgeon may operate again with greater hope and courage, knowing well that a radical cure is not impossible.

Let us, then, study these diseases more and more, and let us endeavour to distinguish them from those maladies with which they are often confounded. Let us strive to learn still more accurately their clinical differences; and, in doubtful cases, let us always avail ourselves of the aid afforded us by the microscope.

M. LEBLANC said that he did not consider, with the microscopists, that cancer did not exist unless the heteromorphous elements, called cancer-cells, were found in the morbid growth. He gave a more extended signification to the term, and included under it all those grave lesions which never healed spontaneously, which became generalized, and which possessed physical characters peculiar to each species of tissue. To determine these characters with certainty, the microscope should, in all cases, be used. Among the lesions which he considered cancerous, were a large number which consisted of the substitution of a homeomorphous, but abnormally placed element, for some other homeomorphous element which was absent at the diseased spot. This was what occurred in epithelial and fibro-plastic growths. They were anomalous accumulations of normal elements occurring, in different proportions, in regions where they ought not to be found in a state of health. The union or simultaneous occurrence of fibro-plastic, epithelial, and melanotic tumours, was by no means uncommon. He cited an interesting case, which proved that fibro-plastic tumours may return, either at the same or at a different place; that these tumours may coexist in the same individual with other growths demonstrated by the microscope to be true cancers; and that each of these varieties possesses a different degree of importance. In every case it was futile to attempt to form a correct diagnosis or prognosis by clinical observation alone, without also appealing to the microscope.

Veterinary pathology presented many good cases illustrating the curability of cancer; and he could advance many instances in which a radical cure was obtained by operating *early* on animals, while the disease was as yet local and not fully developed. It was the same in human as in veterinary medicine—the chances of recurrence were materially lessened by an early operation.

M. BARTH exhibited to the Academy a tumour which had been removed, by M. Amussat, jun., from the testis of a boy, æt. 11, several of whose relatives had died of cancerous affections. The tumour had all the appearances of encephaloid cancer, save that no juice could be obtained from it by scraping or compression. One of the most distinguished microscopists had been unable to find any cancer-cells in it; while another gentleman, equally expert in histological investigations, had demonstrated their presence. M. Barth himself had seen none; and he therefore regarded the growth as a fibro-plastic tumour of the worst species. It seemed to him probable that the hereditary cancerous taint had, in this boy's case, given a malignant character to what was, naturally, a simple and benign tumour. M. Barth discussed the relative value of the microscopical characteristics of cancer, and remarked, in conclusion, that although the microscope had not revealed cancer-cells in this tumour, he did not the less consider it to be a malignant growth. He was quite convinced of the utility of the microscope, but thought we should guard against over-enthusiasm for it,

which would lead us to exaggerate its value, and be deceived by its applications.

M. VELPEAU exhibited several tumours which he had recently removed, the nature of which he had diagnosed before their extirpation. "I only exhibit these specimens," said he, "to show that clinical diagnosis is not uncertain, and that, when I affirm having seen a tumour, I wish my word to be accepted."

M. GERDY—(with great warmth).—I cannot allow this. I protest strongly against such a pretension (*une semblable pretention*).—(Great excitement).

October 31.—M. LARREY did not intend to have taken part in the discussion, but he was surprised to hear so much said, both for and against the microscope, without the cause of the instrument being defended by any of those who had made its use a special study. The microscopists had hitherto abstained from speaking, and it had required all M. Robert's talents to defend the instrument from the imputations cast upon it by M. Velpeau. He possessed no claim to constitute him an efficient partisan of the microscope, but he thought we might as well deny the utility of pathological anatomy as of microscopical observation.

If the cancer-cell appeared, to microscopists, the distinctive cancerous element, we ought not to deny that it was so, in spite of certain exceptional cases, and those differences of opinion which were more apparent than real. One objection urged against the use of the microscope was that its employment required an amount of patient investigation which was incompatible with the exigencies of the profession, and with the acquirements of most medical practitioners. But it was a pity that we should neglect a thing which was really useful, merely because all of us were unable to use it properly. He himself, in doubtful cases, always referred the matter to his microscopical *confidés*. It had been said by others that we had presumed too much upon the value of microscopical appearances. The discrepancies of opinion among microscopists were to be explained in two ways: 1st, They certainly were not infallible in their dicta; and 2d, the mistakes of the inexperienced histologists had brought discredit on the more able observers. Another reason of their diversities of opinion was, that observers were seldom furnished with *entire* tumours for examination, but merely with fragments of them. Now, if these tumours were of a mixed character, they might easily contain cancerous elements in certain parts, and none in others; a circumstance which would occasion contradictory results, if they were examined piecemeal. Moreover, it was not fair for surgeons to try to puzzle microscopists by submitting to them morbid structures for examination, without previously informing them anything about their history, course, diagnosis, or other elucidatory circumstances.

As to the curability of cancer, the microscope—although it was not an infallible test—had not been consulted in the majority of the so-called cases of true cancer, said to be radically cured; hence he entertained strong doubts as to whether cancer was ever curable. One of the most renowned practitioners they had ever known, Marjolin, did not believe in its absolute curability; he looked upon every truly cancerous affection as sooner or later fatal by relapse, and believed a radical cure only to be possible in those tumours which were benign, or had been erroneously diagnosed. Could not this distinction, which Marjolin established from practical observations only, be decided with habitual precision by the aid of the microscope? The real value of this instrument consisted in the aid it afforded us in elucidating cases of doubtful, and rectifying those of erroneous diagnosis. M. Larrey cited the details of several interesting cases in which benign fibrous tumours had been mistaken for cancerous, and showed the advantages of microscopical examination in such cases.

In conclusion, he considered that the Academy was not as yet in a position to decide definitely either for or against the utility of the microscope in forming a diagnosis and prognosis of cancer. He proposed that the Academy should hear the cause of the instrument defended before it, by one or more of the microscopists.

M. MALGAIGNE considered that the microscope had been too much depreciated by M. Velpeau, and too much praised by M. Robert. He thought that the microscope had been of little real service to surgery. He went over, in a rambling manner, the clinical views of cancer which had been held by the older

surgeons, as Boyer, Scarpa, Bayle, Laënnec, Astley Cooper, and Abernethy. He said that the microscope had made a complete revolution in surgery; he did not admire the instrument, and still less those who used it. If the microscopists had not been kept in check by surgeons, they would have destroyed science altogether.

He thought that there were three kinds of cancer; namely, that containing the characteristic cells; epidermic cancer, and fibro-plastic tumours; and all of these were malignant. M. Velpeau had been very severe on the microscope, yet but for it his work on *Diseases of the Breast* could never have been written, for we there find tumours divided into malignant and benign—true cancers and fibro-plastic growths. M. Velpeau had said that he could dispense with the microscope. He might be able to do so, but M. Malgaigne affirmed that, for the bulk of practitioners, a scientific diagnosis was impossible without the employment of the microscope. Those who obstinately refused to employ the microscope appeared to him to resemble a person with imperfect sight, who would not use spectacles, affirming that he could see just as well without them. The question of the curability of cancer was entirely one of correct diagnosis, which could never be made without the microscope.—*Monthly Journ. Med. Sci.*, Dec. 1854.

[We shall give the continuation of this protracted discussion in our next No.]

35. *Cancer*.—Mr. I. Z. LAURENCE read (Jan. 10, 1855), before the North London Medical Society, a paper on cancer, of which the following is a short abstract:—

According to Mr. L., cancer is “a growth which is obviously but the local manifestation of a diathesis, the tendency of which is to destroy life, either in virtue of its own local effects, or of that diathesis, or of both conjoined.” Canceroid differs from cancer but anatomically, in the comparative rarity of secondary deposits, and in its minute structure. Semi-malignant forms of disease are morbid products, as it were, locally malignant, but constitutionally innocent. The diagnostic data of a surgical tumour were divided into two groups: 1. The clinical; 2. The anatomical. The former were considered by the author *seriatim* at some length. In reviewing the physical properties of tumours, Mr. Laurence insisted strongly on the advisability of employing the grooved needle previous to any operative procedure in all cases that were at all of a dubious character. He related a case of submaxillary chronic abscess, in which the utility of this practice was forcibly illustrated. Mr. Laurence considers the so-called cancerous “cachexia” of indisputable occurrence, but of little diagnostic value. The anatomical properties of tumours were now passed in review. Special allusion was made to the anomalous characters cancerous tumours often assume when they originate in the osseous tissues, and illustrated by several cases that had been examined by the author. The relations, the clinical phenomena attending the development of morbid growths in the organism were now discussed in considerable detail. Mr. Laurence gave as the *resumé* of his researches on the minute structure of tumours the following conclusions: “1. That, in the greater number of cancerous tumours, the ‘cancer-cell’ will be found to exist. 2. That this cell-form occurs now and then in growths clinically innocent. 3. That, *vice versâ* (what is, however, more rare), tumours anatomically innocent prove clinically malignant. That the cancer-cell is not the *sine qua non* character of cancer.—(Velpeau). 4. That the results afforded by the microscope must take an important but not exclusive position in the series of diagnostic data.”—*Med. Times and Gaz.*, Feb. 10, 1855.

36. *Cancer successfully Treated by External Application of Caustic*.—Dr. J. D. GILLESPIE related the following case to the Medico-Chirurgical Society of Edinburgh:—

A gentleman, aged seventy-nine years, of a large robust frame, in 1847 received a slight abrasion of the heel, which speedily cicatrized, but the cicatrix soon after gave way and ulcerated. The ulcer dried up and left a hard warty substance in its place, which continued to increase. In 1851, it had assumed the appearance of a hard cancerous growth, fully half an inch in depth. By the advice

of Mr. Syme, caustic was applied. Erythema of the leg soon after set in and reached as far as the groin, and the tumour increased in size. A second attack of erythema occurred a few weeks after. The tumour continued to increase in size; the glands in the groin became enlarged; the general health began to be impaired, and the mental faculties were affected. It was in October, 1851, that caustic had been first applied; the tumour was then the size of a walnut, spongy, and occasionally bleeding. Chloride of zinc was had recourse to, and the tumour came away, but after cicatrization had commenced, the tumour reappeared. The application was continued for a year with similar results, repeated hemorrhages took place, and the glands in the groin were permanently enlarged. Potassa fusa was applied on three different occasions, but the chloride of zinc was decidedly more efficacious. On September 18, a year after the first application, chloride of zinc was tried for the twentieth time, and the tumour to the extent of three inches was destroyed; a part of the os calcis exfoliated. Two years have now elapsed, and there had been no tendency to reproduction. Dr. Gillespie considered the case interesting, as it was just such a one which was occasionally vaunted by some quack as a victory gained over modern surgery, and this one certainly explained their so-called cures, and at the same time it was fraught with caution to the surgeon not to lose an opportunity of giving similar cases a trial with the caustic before dooming them irrevocably to the knife.

Professor Miller referred to cases of soft cancer of the breast where a cure was sometimes obtained by the diseased product sloughing *en masse*. He could corroborate the statement of Dr. Gillespie with reference to the efficacy of the chloride of zinc; as a caustic it was thorough, and easily kept under command. — *Monthly Journal of Medicine*, January, 1855.

37. *Malignant Disease of the Ilium*.—Mr. FERGUSSON, in a clinical lecture delivered at King's College Hospital, gave the following account of a case of this kind.

"T. B., aged 56, a bookseller at Birmingham, states that he has always been very healthy, although subject latterly to great debility. About last Christmas—that is, nearly four months ago—he began to notice a pain in the posterior part of the left haunch bone, which was at first trifling, though increased by the slightest exercise, and always relieved by rest. The pain gradually increased in severity until six weeks since, when it came on rather more sharply than usual, and then it was that he noticed a swelling in the situation of the pain. This swelling was then as large as the top of his thumb; and the pain, when it commenced, was, according to his feelings, situated in it. He moreover states that, when he first discovered the tumour, he found it pulsate distinctly. From its appearance to the present time the tumour has been steadily and slowly increasing. Present condition: The patient is in a state of great debility, which has been slowly coming on for the last two years. Bowels much confined; appetite good; sleeps well; pulse 90 and weak. There is a tumour situated on the posterior part of the left dorsum ilii, above and slightly over the great sacro-sciatic foramen. The crest of the ilium can be felt distinctly above it. The dimensions are, five inches in one direction, and four in the opposite; its circumference is nearly circular, and it stands out from the surface of the body about one inch. When the fingers are placed upon the tumour, it may be distinctly felt to pulsate, each pulsation being synchronous with one at the wrist. If the stethoscope be applied to it, a blowing sound in successive puffs is plainly audible. It is not tender to the touch. The skin over it is freely movable, and not discoloured. A bladder filled with ice is kept for two or three consecutive hours over the tumour, till the skin becomes quite numb. This has a very decided effect on the tumour, causing it to become hard, with the pulsation diminished."

From the history, I think you would naturally suppose the case to be one of aneurism of the gluteal artery; and for such it was taken by the surgeons who sent the patient to me from Birmingham; indeed, so certain were they, that it was considered a proper instance for ligature of the internal iliac artery. I

have never seen an aneurism of the gluteal artery, and I believe spontaneous aneurism of that artery to be extremely rare.

After the admittance of the patient into the Hospital, I made a careful examination; and this is what struck me: Here was a man lying in bed, with a large pulsating tumour, resembling in many ways an ordinary aneurism; pallid, thin in the limbs, hardly able to walk, and experiencing great pain in and about the region of the tumour. Now, contrast this with a case of spontaneous aneurism of the popliteal or other large artery of the extremities, but especially with one situated within one of the large cavities. The patient labouring under the disease is much annoyed at being kept to his bed; he imagines that little is the matter with him, for he may be capable of undergoing great fatigue, perchance walking great distances to seek opinions regarding his malady. In fact, it is not a feature in ordinary aneurismal disease for the patient to be out of health. I mentioned that great pain was very early complained of in the tumour, whereas it is rare for those labouring under an aneurism of a large artery to make such a complaint at so early a period.

Regarding the character of the tumour: Where I made pressure in the site of the gluteal artery, as it leaves the sciatic notch, and turns upon the outside of the pelvis, all pulsation in the tumour ceased, which fact certainly seemed very strong in favour of the supposition of a gluteal aneurism, and an indication which, taken by itself, was not unlikely greatly to bias the surgeon in his diagnosis; but I confess this had little or no weight with me, for I have met with malignant tumours of bone in various parts of the body possessing great pulsation, which could be arrested by pressure on the main artery supposed to lead to the disease.

The size of the tumour and its situation were very much against it being considered an aneurism of any one branch of the gluteal; indeed, I could not fancy such constitutional disturbance to be dependent upon such a source. It is seldom that the surgeon is consulted about an aneurism at so early a period as this case came under notice; but, if he should meet with such a disease, at what is termed its first stage, it is generally possible to empty the blood out of the tumour; but, as you saw during my visits to the patient, I frequently tried so to do, but was always unsuccessful. In the earlier stage of the disease, the crest of the ilium could be distinctly traced above the upper border of the tumour; but, after a short time, the part became entirely involved in the disease. There was one feature in which commonplace anatomy greatly assisted me. I noticed that the insertions of the long extensor muscles of the back, as the disease increased in its dimensions, became elevated above the surface of the tumour; whereas, if the swelling had been an ordinary aneurism of the gluteal artery, they would doubtless have been beneath. These were, then, the principal points from which I framed my diagnosis, in the early stage of the disease; and I came then to the conclusion, that the man was not the subject of a gluteal aneurism, as was at first supposed, but that he was labouring under malignant disease of the ilium, and that the tumour was what is commonly called a pulsating tumour of bone, possessing more vascularity than was usual in such cases; and hence the well-marked pulsation.

This so-called aneurism of bone is, in my opinion, nothing more than aneurism by anastomosis in hard structure, somewhat modified in its condition and symptoms, and which might in many instances be called medullary sarcoma, or fungus hæmatodes. There is generally a very large expansion of bone, seeming as if the bone itself had been blown up, the interior of which may contain a quantity of medullary or melanotic matter, or resemble a sarcomatous mass, containing cysts with fluid blood, and possessing great vascularity. Though thoroughly satisfied in my own mind as to the nature of the disease, I was anxious, nevertheless, to avail myself of the advantage of other opinions; and, accordingly, after requesting the opinions of my colleagues, I invited surgeons of the highest eminence in London to see the case, the majority coming to the same conclusion that I have already expressed.

It was quite evident that an operation could in no way benefit the patient, and that the only treatment that could avail was rest and kind attention to his wants. He daily grew weaker, until at length, becoming delirious, he ultimately

died from pure exhaustion. Here let me impress upon you how important it is to avoid all surgical interference in those instances in which such a proceeding is unlikely in any way to better the patient, but which, on the other hand, is calculated to bring discredit on the surgeon, and the worst possible result to the sufferer. This disease so nearly resembles gluteal aneurism that surgeons of experience have actually mistaken its true nature, and treated the case as if it really had been an ordinary aneurism. Among such instances I may mention two cases which occurred to Mr. Guthrie and Mr. Stanley, in which the main artery was ligatured; but subsequent results showed the error that had been committed. These gentlemen have candidly laid the whole proceedings before the profession, and therefore I have no hesitation in mentioning their names.

By studying these unfortunate cases, which are justly held out to us as landmarks, we are enabled to avoid falling into similar errors; and I often think that in our profession as much is learned from mistakes as from the performance of good surgery.

Some time before the patient's death, while paying my visit one day, I found that the left thigh-bone had spontaneously broken just below the trochanter, no cause being assigned for the accident. Now, in malignant affections, or certain forms of cancer, it is not by any means uncommon for bones to become friable, and break with the least possible exertion. This was, therefore, another very conclusive proof in support of the malignant nature of the disease.

These were, then, the principal points of interest in this remarkable case; and the *post-mortem* examination proved the correctness of the leading opinions that had been formed during life. Most of you saw that examination; but I will briefly detail the morbid appearances which were presented. When the body was put on the table, you saw the extreme emaciation to which the patient had been reduced by the malignant tendency of the disease. On reflecting the skin from the left buttock, and dissecting back the gluteus maximus, which was greatly wasted, a large tumour was found, reaching from the sciatic notch to the brim of the pelvis—in fact, covering the whole of the haunch bone, and resembling more the substance of brain than any I remember to have seen. The tumour, I think, might with propriety be called by half-a-dozen different names, each being equally applicable, as medullary sarcoma, encephaloid disease, soft cancer, etc.

The disease had evidently commenced in the cancellated structure of the ilium, and found its way into the cavity of the pelvis, as well as outwardly. That part projecting into the pelvis had elevated the iliac vessels, and put upon the stretch the large nerves of the sacral plexus, which fully accounted for the constant pain complained of in the thigh, extending to the foot. That portion of the ilium in which the disease commenced was entirely destroyed, the free margin of the bone being left rugged and bare.

The gluteal artery, much enlarged, could be traced, shortly after leaving the sciatic notch, winding over the lower part of the tumour, and distributing large branches, which ramified freely over the surface, and fully explained the reason of the well-marked pulsation during life. The large sacro-sciatic nerve was pressed upon by the lower part of the tumour, which doubtless caused the excessive and frequent pain to the patient. The disease had extended to the left side of the two lower lumbar vertebræ, sacrum, crest of the ilium, and upper part of the femur. The remaining bones of the body were not found similarly affected, but the kidneys and lungs presented well-marked specimens of cancerous deposits.—*Med. Times and Gaz.*, Dec. 9th, 1854.

38. *Treatment of Aneurism by Compression.*—Mr. FERGUSSON, in a recent clinical lecture, delivered at King's College Hospital, made the following interesting remarks on this subject:—

“From the time of the celebrated John Hunter down to the present day, aneurism, especially of the popliteal artery, has been deeply studied, and, in attracting such an amount of attention, has led, I believe, to a more accurate knowledge of external or surgical anatomy, which renders the surgeon of the

present day fully competent to perform a most difficult and hazardous operation for the cure of this formidable disease.

"Since Hunter first tied the femoral artery for the radical cure of a popliteal aneurism—now seventy years ago—a vast deal has been done in this department of surgery, for the Hunterian operation has been made applicable to the radical cure of aneurism of many of the large arteries in various parts of the body. The carotids, iliacs, subclavians, &c., have been tied with signal success; and a ligature has even been placed around the abdominal aorta and innominate artery, but, unfortunately, with fatal results.

"The Hunterian treatment by ligature proving so successful the operation obtained the sanction of the profession, and young surgeons, anxious to distinguish themselves, were not slow in imitating the example; and it so happened that, if they were successful in the treatment of their cases, they obtained a well-merited *éclat*, which adds a stimulus to exertion, and always meets with its reward.

"The Hunterian operation being fully established, all other methods formerly employed had fallen, in a manner, into disuse; but, when it was shown that men had courage to countenance any new method that presented, there was then a return to plans that were in existence prior to the time of Hunter. It was also strange that one at least of these methods was esteemed much more worthy of attention than was at first supposed. The plan that I allude to is compression of the main artery leading to the tumour. No doubt this plan is far less brilliant in its details than a daring cutting operation; and more credit is due to those who were the means of its revival, when it is remembered that it was resorted to at a time when a successful operation was in continual practice with the most satisfactory results.

"The advantage of compression in the treatment of external aneurism, when practicable, over that by ligature, is at once evident. By inflicting one kind of injury, we are often enabled to get rid of a still greater mischief, and this is considered good surgery; but if, by a process less dangerous and hazardous, and without performing a cutting operation, the same disease or injury can be cured, surely it is a better style of surgery, and one that approaches nearer to the perfection of our art.

"The history of the case is short; it is briefly this: A. B., aged 22, married. Has always enjoyed pretty good health. The first symptom of her disease occurred twelve months ago. This was pain, confined to the popliteal space of the left leg; she could assign no reason for its coming on, as she never had any accident to that leg. The pain, which has lasted to the present time, occurs in paroxysms of variable length, sometimes lasting for a whole day, and at other times not an hour, and accompanied with intervals of perfect ease. The pain is always aggravated by exercise, or by merely extending the leg. It has never been bad enough to prevent her following her ordinary occupation. Soon after its first occurrence, she noticed a hard, throbbing tumour, the size of a hazelnut, in the centre of the ham; it was not tender to the touch, although it seemed to be the seat of the pain. It has remained almost unchanged since its first appearance; the only alteration that has taken place is a trifling increase in its size. It is quite firm and pulsating. Pulsation is completely arrested, if the superficial femoral be compressed. A bruit is heard on applying a stethoscope to the tumour.

"There are one or two very singular features in this case. The first is, the occurrence of a popliteal aneurism in so young a person. You may never again see a similar case. I have never met with one before in so young a subject, and I imagine hardly a similar instance is to be found in the records of English surgery. The chief point, however, to which I wish to direct your attention is the plan of treatment that was pursued. It is one that was in vogue with surgeons two hundred years ago, and various accounts of it are to be found in the works of older authors. After the successful trial of Hunter's operation it was given up; but within the last few years it has been revived by some of the most eminent surgeons, especially those of Dublin, who have taken precedence in this interesting subject. A full account of the practice will be found in some of the established modern works, and a slight sketch in my own book on 'Prac-

tical Surgery.' Since the revival of this method, there has been sufficient success to strike most surgeons with astonishment; for out of a long list of cases treated by compression, success has been great beyond anticipation, and much greater than when the disease had been subjected to the treatment by ligature.

"This method of treatment is now very generally sanctioned by the profession, and I am not aware of any distinguished surgeon, save one, who strongly objects to at least a trial of this plan. I allude to Mr. Syme, who has been remarkably successful in a great number of cases in which he used the ligature; for he states that in tying the superficial femoral for the cure of popliteal aneurism in upwards of twenty cases, he has not met with a fatal result. But I have seen the artery tied equally well by other surgeons of eminence, in well-selected cases, though not with the same beneficial effects. Mr. Syme has expressed his opinion so strongly in favour of the ligature over compression, that he has endeavoured to cast a certain amount of discredit on those who have ably devoted their time and energy to the trial of a more simple and less hazardous cure of this formidable disease. On the revival of compression for the cure of popliteal aneurism, several improvements were made with regard to the manner of applying the pressure, and old notions regarding the duration of time it was necessary to employ were soon eradicated. It was early found that there was no need to apply the pressure so forcibly as to keep the sides of the vessel together, thereby inducing inflammation and subsequent adhesion, which led to the obliteration of the canal; for the operation of Bradsor and Wardrop of placing a ligature on the distal side of the tumour, showed that to obtain a satisfactory result it was not necessary to cut off the entire flow of blood through the tumour; and subsequent experience has proved, that in the treatment by compression it is not requisite so to do, but that gentle and continuous pressure modifying the course of the current is quite sufficient, in the generality of cases, to cause consolidation within the tumour.

"Again, the pressure need not be constantly applied to one particular spot, but may be shifted from place to place along the course of the artery, so as to give the patient as little pain and annoyance as possible.

"The result to be hoped for in the treatment of these cases is, coagulation of blood and deposition of fibrin in the aneurismal tumour, which, as I have said, can be produced in many cases by simply modifying the flow of blood through the main artery. The femoral artery is never obliterated at the point where pressure is applied, but can be traced almost down to the tumour itself. If the case go on favourably, and the patient be a proper subject for the plan of treatment, a cure may take place in a few days, or in a week or two; but if, on the other hand, the patient be not content to put up with the inconvenience and pain for a time, the cure may be protracted for a lengthened period. This was well illustrated in a case I had under my care some time ago, when, owing to the extreme irritability of the skin and the condition of the patient, sloughing occurred in the groin, occasioned by the pressure, which greatly retarded the cure.

"I do not intend to enter more fully into the merits of the several methods for the cure of aneurism, but shall content myself with stating the treatment I employed in the case under notice, and the results which followed. The aneurism belonged to that class which by Scarpa was termed a 'mixed aneurism;' that is, there was probably a dilatation of all the tunics, and then subsequent rupture of the two internal coats.

"I employed the instrument that is most in favour among the Irish surgeons. Carté's 'circular compressor,' in which a very ingenious addition to the common screw force is applied by means of bands of caoutchouc, whereby a certain resiliency is acquired, which keeps up effectual pressure, and at the same time possibly obviates any injurious effects from the screw. Among other contrivances, I may mention the tourniquet of Signoroni; also the one invented by my friend, Mr. Bulley, of Reading, which has two screw pads, allowing the pressure to be applied at different spots at various periods. There are other equally ingenious instruments which possess their individual merits.

"At first, I must confess, I did not anticipate a favourable result, as the patient did not appear to be fully aware of the great danger of the disease, and

was seemingly very loth to submit to the treatment; but, by explaining to her the importance of resolutely bearing the inconvenience attendant upon such a plan, in all probability a more serious operation would be avoided, she accordingly determined to aid us in the cure; and the following sketch of the treatment shows the satisfactory result:—

"I applied the pressure to the common femoral immediately below Poupart's ligament, regulating the force to such a degree as to keep up an amount of pressure just sufficient to stop the pulsation in the tumour. It was continued for twenty hours, when, some uneasiness being complained of, it was omitted for an hour or two, and then reapplied. On the third day the pulsation was not so full or throbbing as before. On the ninth day the pressure was modified to such an extent, sufficient to weaken, but not completely to arrest, the flow of blood into the artery. On the fourteenth day, the woman not feeling at all well, all pressure was removed for a few days. On the nineteenth day it was readjusted, but, in the evening of that day, the pulsation in the tumour became feeble in a marked degree, and some slight pain was complained of in the ham. On the twenty-first day all pulsation had completely ceased, and the tumour was found diminished in size and of considerable hardness. It was now evident that a cure was effected; and no return of the disease has as yet shown itself.

"The most interesting point in the treatment of this case is the rapidity and easiness with which this disease was cured; and it also well illustrates the fact that it is not always absolutely necessary to make continuous pressure on the artery for any length of time."—*Med. Times and Gaz.*, Dec. 30, 1854.

39. *Aneurism.*—The *Medical Times and Gazette* (January 20, 1855) contains a report of two cases of aneurism of the lower extremity, lately treated at the Liverpool Royal Infirmary. In one case, compression was successfully employed; in the other, the femoral artery was ligatured, which was followed by gangrene and death.

40. *Fracture of the Pelvis, with Laceration of the Urethra.* By JAMES SYME, Esq.—[The following very interesting cases are related in a clinical lecture, delivered during the winter session of 1854-5]:—

"The next case to which I shall allude is one of great interest, being very serious, and also of considerable rarity. The patient is A. M—, aged thirty-four, pointsman at the Cobbingshaw station of the Caledonian Railway. On Monday, the 13th November, he was crossing the line, at 8 P. M., when an engine, coming upon him unawares, at a speed of about fourteen miles an hour, struck him upon the right side of the pelvis, and knocked him down. He was insensible at the moment, but soon came to himself, and was brought by train to Edinburgh, and admitted into the Infirmary about 10 P. M. At this time there was some swelling of the right thigh and groin, and, on examination of the pubic arch, an abrupt deviation from its natural line was felt about the junction of the rami of the ischium and pubes on the right side, and on pressure with the finger on this spot, very distinct crepitus was perceived, a small portion of bone being apparently detached, and freely movable. As a measure of precaution, an attempt was made to introduce a catheter, but no urine could be drawn off, although the instrument passed pretty freely backwards, and was supposed to have entered the bladder. He was, therefore, put to bed, a flannel bandage being passed round the pelvis. Next morning, however, the patient complained of inability to pass urine, and a tumour began to appear in the hypogastrium. Another attempt was now made to introduce the catheter, but without success, its point having a constant tendency to slip out of the membranous part of the urethra. I was then asked to see the patient, and passed a full-sized instrument, which was tied in and remained for two days. About midnight of the day of its removal, I was again called to the case, in consequence of retention, with distressing symptoms. I again passed the catheter, which was now retained three days and a half, the patient not complaining of any inconvenience from its presence. But, within a few hours after it had been removed the second time, it became again necessary to introduce it, the patient being unable to pass a drop of urine. The instrument has been since retained,

having been now, for the second time, three days in the bladder, in addition to the first two days. It now becomes a question, What is to be done? A man cannot always wear a catheter in his bladder: a metallic instrument is liable to cause injury by producing ulceration, and I believe it would be impossible to pass a flexible one. It would appear that the injury inflicted on the urethra has been such as to produce a valvular condition at the lacerated part; certainly no extravasation of urine has occurred, otherwise death must speedily have resulted from its escape from the part of the canal injured.

"There seems to be something wrong in this case, which it is not in the power of nature to remedy. After careful consideration of the circumstances, I have concluded that the catheter must now be removed, and that if he should be still unable to pass urine, an incision must be made, so as to reduce the injury to the condition of a simple wound. There are two situations where this might be done; either laterally, as in lithotomy, or in the median line. It is not a matter of indifference which of the situations be chosen, that in the middle line being much the safer; for the artery of the bulb, which is a vessel of very considerable size, enters the bulb at its lateral aspect, just at the anterior part of the membranous portion of the urethra. Now, if a knife be entered into the membranous part of the canal at its lateral aspect, and carried forwards into the bulb, then (and engrave what I now say upon your memories till the last day of your existence as practitioners of surgery!) to an absolute certainty, must the artery of the bulb be wounded. Hence the common cause of wound of this vessel in lithotomy is division of the urethra too far forwards, and this is particularly liable to occur to those who use a straight staff. In the present case we shall have, very probably, to cut as far forwards as the bulb. The artery of the bulb is hardly within reach of ligature, and if pressure be applied to repress the hemorrhage, extravasation of urine into the cellular tissue of the pelvis must of necessity occur when the patient makes an effort to pass urine. All this risk will be avoided by cutting in the middle line upon a staff grooved on the convexity, and then introducing a short tube through the wound into the bladder. The short tube is far preferable to a catheter in such a case, for here the incision, unlike that for stricture, involves the deep fascia of the perineum, and, if the beak of the catheter should slip for a short distance, the patient would be liable to fatal extravasation of urine. I speak from sad experience. A healthy young man once applied to me with a stone in the prostatic part of the urethra. I extracted it easily, introduced a catheter, and tied it in. Next morning, however, I found him dying; the catheter had slipped, and urine had become extravasated into the pelvis.

"The patient continued unable to pass urine after the removal of the catheter, which was again introduced by Mr. Syme in the night. He found that the parts in the vicinity of the injury of the urethra were in a less satisfactory state than before; they were becoming softened, and a cavity containing urine had formed around the seat of laceration. Mr. Syme, therefore, resolved to proceed to the operation to which he had alluded in his lecture; and accordingly the next day (Nov. 24th), the patient being under the influence of chloroform, he introduced into the bladder a staff grooved on the convexity, made an incision in the middle line of the perineum, and, having introduced the point of the knife into the staff, pushed it along so as to divide freely the membranous part of the urethra, which he could distinctly feel to be lacerated; a short tube, such as is used after lithotomy, was then passed into the bladder through the wound, and tied in. Not more than an ounce of blood was lost in all, including subsequent oozing.

"Mr. Syme, in remarking on the operation, said, that the only source of regret was that it had not been performed earlier. The prognosis was now rendered very unfavourable by the jaundice which had come on during the last twenty-four hours, and by a livid discoloration in the perineum, which looked very like sloughing of that part. The patient felt at first much relieved after the operation; but, on the following day, he was seized with very severe pain in the hepatic region, and acute tenderness below the right costal margin. Leeches were applied, and calomel and opium administered; and, under this treatment, the pain and tenderness subsided, and in a few days the jaundice

disappeared. Healthy lymph upon the surface of the wound showed also that the suspicion of sloughing of the perineum was groundless; and the prognosis became, on the whole, much improved. The almost total loss of appetite, however, which he had before the operation, and during the hepatic attack, produced great emaciation; and this, together with his lying always on his back, in consequence of the fracture of the pelvis, while the bed was more or less wet with urine, induced extensive bed sores, which have since been the chief difficulty to contend with in this case.

"The tube was removed forty-eight hours after the operation. The urine then came freely away, and passed entirely by the wound for a fortnight. Mr. Syme then passed a full-sized bougie, which went smoothly into the bladder; after which the patient passed a few drops of urine by the penis. During the following month the amount of urine passed by the natural passage increased, the bougie being introduced at intervals of about ten days. The use of the bougies has not been continued since; and a considerable proportion of the urine is still (February 6th) passed by the fistulous opening in the perineum; hence the difficulty of healing the bed sores continues to be great, and he is prevented from making the progress which he doubtless would otherwise make. Still, by the use of a large water-pillow, and frequent change of bed-clothes, the back is now almost healed, and his general strength is on the increase, so that he is able to support himself in bed, during the dressing of the back, more than ever before.

"On the 22d of January, Mr. Syme showed to the class the urethra and parts of the pelvis of a young man eight years of age, who had fallen from the roof of a lofty house, and died in the hospital a few hours after. On admission he was almost unconscious, and moving his limbs about in a wild manner, and these symptoms, together with a severe bruise on the right side of the head, led to the suspicion of laceration of the brain, which was found to exist on *post-mortem* examination. A few drops of blood from the penis led to a careful examination of the pelvis, when a comminuted fracture of the descending ramus of the right os pubis was detected, and a catheter was kept in the bladder till a little before his death. On examination of the body, it was found that the horizontal and descending rami of both pubic bones were broken; and there was a comminuted fracture of the right side of the sacrum, through the line of the foramina for the passage of the nerves. Very extensive ecchymosis existed about the fractures, and the membranous part of the urethra was completely separated from the surrounding parts by coagulated blood, while some of the fragments of the pubes lay very close to the canal. On laying open the urethra, however, on a full-sized bougie, it was found to be quite entire, some bruising of the membranous part, and of the spongy portion near the bulb, being the only injuries it had sustained. Mr. Syme, after pointing out these facts, spoke as follows:—]

"You recollect a patient who is still in the hospital, to whose case I directed your attention in a former lecture, who had suffered fracture of the pelvis, and laceration of the membranous part of the urethra, and in whom I found it necessary to make an opening into the perineum, on account of protracted retention of urine. In that man's case, the injury sustained by the urethra was only a degree further than what you see here; there the urethra was lacerated at its membranous part, by the fragments of the pelvis; here it is only bruised. You see, from the condition of the parts here present, how necessary was the precaution of introducing a catheter in that case. You observe the membranous part of the urethra is lying insulated from surrounding parts in a cavity filled with effused blood, and had the urethra been torn in this situation, and no catheter introduced, extensive extravasation of urine must inevitably have occurred. After bruising or laceration of the urethra, it is very necessary to introduce bougies occasionally to guard against the occurrence of obstinate contraction of the canal at the seat of injury.

"This has been well illustrated by another case still in the hospital. You remember my speaking to you, in a former lecture, of a miner from Bonness, who, having occasion to descend a shaft eighty-four feet deep, adopted the dangerous method of slipping down a rope. He had not gone far, however, when he found

that the rope was wet and very slippery, but he still kept his hold, and shot down with great velocity, lighting on the perineum, upon the edge of a wooden cage used for drawing up the mineral. He then suffered from retention of urine, which the medical men of the district were unable to relieve, and he was, therefore, sent to this hospital. His bladder was then greatly distended, and the perineum was bruised near the scrotum. A full-sized catheter was, however, introduced without much difficulty, although the urethra was obviously injured, and a large quantity of clear urine was drawn off. After a time, bougies were passed with the object of preventing contraction, but not until it had occurred to a small extent; but now, the use of the instruments having been discontinued for ten days, the stricture has increased so much that it will admit only a No. 1 bougie. This same patient, on his admission, complained of severe pain in the back, extending round the waist, and into the legs; and, on examination of the spine, a marked unnatural convexity was found on the lower part of the dorsal region, which was very tender when touched, and surrounded by ecchymosis. There could be no doubt that, the patient having lighted on his seat, the back had been violently bent, and fracture of the vertebræ had occurred, the bodies being compressed, while the spines were separated from one another at the part. The patient, however, has had no paralysis of the lower limbs, and the pain of which he at first complained has now almost entirely subsided. The spinal canal is of much greater capacity than the cord; and this accounts for the fact, that fracture of the vertebræ may occur without any injury to the cord. I have seen repeated instances of this in the hospital; and the present case is a good example of it."—*Lancet*, Feb. 17, 1855.

41. *Fracture of the Head of the Astragalus*.—M. VOLLMAR relates (*Zeitsch. f. Chirurg. und Geburtsh.*, iii. 1854) the following example of this rare accident:—

A stout countryman fell from the height of eight or nine feet. After the removal of the boot from the left foot, there was noticed a bony prominence in the arch of the foot; and the patient, imagining there must be a fracture, applied cold lotions to the part to reduce the swelling. The foot was found in its natural position, with moderate flexion. In front of the articular extremities of the tibia and fibula there lay, under the raised integument, a bony swelling, separated by a deep depression from the outer malleolus. No hollow, from which the bony mass had escaped, could be detected; the tarsal bones seemed in their normal relations. Extension effected the reposition of the displaced parts, and in four weeks the patient was able to go about, proper bandages having been maintained to keep the separated portions of the astragalus together.—*Med. Times and Gaz.*, Jan. 27, 1855.

42. *Carbuncle*.—The number of the *Medical Times and Gazette* for December 2d last, contains a report on this disease, with a tabular view of thirty-five cases. The following are the conclusions drawn from the facts there collected:—

1. The frequency of carbuncle has vastly increased of late years, and still continues to do so.
2. But little is known respecting its predisposing causes.
3. It may affect any age, excepting perhaps the very young.
4. Men are much more liable to it than women.
5. It occurs without distinction, at all periods of the year.
6. It occurs in almost equal proportions among the temperate and well-fed and the intemperate and ill-fed.
7. It has a premonitory stage in a considerable proportion of instances.
8. Its general treatment should be by purgatives and alteratives in all cases, and by stimulants or salines, according to the character of the constitutional disturbance.
9. Incisions are demanded when a carbuncle is spreading, or attended by much pain.
10. In a great majority of cases, free incisions relieve the pain, and in a considerable degree arrest also the spread of the disease.
11. If the spreading and the pain have already ceased, no benefit will be

derived from incisions, but the sloughing and suppuration will be much increased.

12. The "core" which separates consists, for the most part, of dead areolar tissue.

13. No proof exists that carbuncle exerts any eliminative influence on the system.

43. *Treatment of Hospital Phagedæna.*—The prevalence of phagedæna, which, during the past nine months, has been pretty general in the London Hospitals, seems now to be steadily diminishing. It has been very difficult to arrive at any satisfactory conclusions respecting the laws regulating its occurrence. At one period, it has appeared to spread through a certain ward, or to prevail in a certain Hospital, as if by contagion; while at others, observations have been made tending strongly to support the opinion, that it was largely under the influence of atmospheric changes. It has prevailed very irregularly in the different Hospitals, being now epidemic in one, and then, after the lapse of a short time, appearing in another. On the whole, it has been a mild form of the disease. Very few have, we believe, died of it, and a vast majority have recovered after a short and not very destructive attack. In several instances, however, in which stumps, after amputation, have been attacked by it, so much of the soft parts have been destroyed, that a second removal of the bones has become necessary. With regard to the treatment of the disease, the following recommendations might, we think, be summed up as the results of the combined experience of the surgeons who have been most engaged with it:—

1. *As soon as a wound shows a tendency to become sloughy or phagedænic, to have the patient change his bed, and, if possible, his ward.*—This practice was pursued in almost all cases in Guy's and in the London Hospital, and more or less in most others. Often very sudden benefit was remarked. The recommendation, of course, proceeds on the supposed desirability of removing the patient away from any local influences, contagious or endemic, which may have had part in producing the disease. The following case may be quoted as illustrative: A boy, in excellent health, submitted to primary amputation of the arm, on account of a crush, under the care of Mr. Wordsworth, in the London Hospital. On the day following the operation he was remarkably well, and had not the least of constitutional disturbance. During the next six days he continued well, and the stump was granulating healthily, when it became necessary to change his bed, and to put him into one from which a man who had died of pyæmia had been removed. Mr. Wordsworth directed that all the bed furniture should, as a measure of precaution, be removed; and, with the exception of the mattress, this order was complied with. On the morning following the change of bed, the lad was feverish and restless, and his stump had lost its granulations and presented a sloughy surface. He was at once ordered back to his original bed. The phagedæna did not spread; but, almost immediately after the second change, the condition of the stump began to improve, and ever afterwards the advance was uninterrupted.

2. *To destroy fætor by the employment of charcoal.*—In this way, probably, not only is a gas decomposed which was likely to have acted prejudicially on the animal functions, but one which might not improbably have been the means of direct infection.

3. *To employ nitric acid as an application to the sore.*—Most surgeons have formed a very high opinion of the value of this remedy when efficiently used. The acid should be concentrated and pure, and should be liberally applied. We have already at such length spoken in its praise that anything further need scarcely be here added. (See *Medical Times and Gazette* for January 6, page 9.)

4. *To employ as constitutional measures tonics and general stimulants, with in some cases opium, or the chlorate of potash as specific remedies.*—Respecting the use of the latter, a considerable difference of opinion prevails; but instances have occurred in some hospitals which appeared to show almost incontestably their potency in at least some individual cases. The chlorate of potash well deserves a much more thorough investigation as to its remedial powers than it has yet received.—*Med. Times and Gaz.*, Feb. 17, 1855.

44. *Injection of Iodine in Ovarian Dropsy.*—Dr. SIMPSON mentioned, at a meeting of the Medico-Chirurg. Soc. of Edinburgh (Nov. 15), that he had injected a case in his wards a few days ago, from which he had removed 200 ounces of fluid, and that he had tested the urine for iodine, but without success. It was usually present, however, after the injection of a hydrocele. He (Dr. S.) had now injected some 10 or 12 cases of ovarian dropsy; the disease had recurred in a few of the cases, but in the others, the cure had as yet been permanent. The operation was unattended with any bad results. Indeed, the pulse never rose, and little or no uneasiness was expressed, and that only (if present) in drawing the fluid, apparently the result of adhesions. He employed the Edinburgh tincture of iodine, which had also been recommended for its strength by Mr. Martin, in his paper on the cure of hydrocele by injection.—*Monthly Journ. Med. Sci.*, Dec., 1854.

45. *Radical cure of Hernia by injections of Iodine.*—M. JOBERT lately presented to the Academy of Medicine three cases of inguinal hernia in which this new mode of treatment had been productive of the best results.

The first case was that of a young man, 18 years old, with complete inguinal hernia of the left side, which, though it did not interfere with his ordinary avocations, rendered him ineligible for the military service. The tumour did not descend to the bottom of the scrotum, and it was separated from the testis by a constricting band. It was easily reducible by taxis, but reappeared upon the patient coughing or assuming the erect posture.

At the patient's request M. Jobert performed the following operation on the 12th May. An incision, 2 centim. in length, having been made over the inguinal canal, a fine trocar was introduced into the sac, through which 5 grammes of pure tincture of iodine were injected. The wound was then closed by means of twisted sutures, and dressed with simple ointment. The patient suffered almost no pain. In the evening there was slight tumefaction and pain of the inguinal region, but no fever or constitutional disturbance.

May 14.—The swelling and redness were considerable. The sutures were removed, and the wound was found imperfectly healed.

In a few days the external wound cicatrized, and the swelling and redness disappeared. The left testicle was not atrophied. On 5th June the patient was able to go about, and to cough without the least inconvenience or reappearance of the tumour. As a precautionary measure he continued to wear a suspensory bandage for some time.

The second case was that of a patient, æt. 34, who entered the Hôtel Dieu on the 18th Nov., 1853, having not only a congenital inguinal hernia, but also a hydrocele of the same side. These were separated from each other by an annular constriction of the tunica vaginalis, which contained the protruded bowel above, and the testicle and sermo fluid below. M. Jobert resolved to obliterate the tunica vaginalis, for the purpose of curing both the hydrocele and the hernia at the same time. He accordingly passed a trocar obliquely into the lower portion of the constricted sac, and evacuated the serosity therein contained; and then, having previously made pressure with the finger over the inguinal canal to prevent the possibility of there being any communication between the tunica vaginalis and the peritoneum, he injected some tincture of iodine into the tunica vaginalis and the hernial sac. The external wound was finally closed by adhesive plaster. Next day the scrotum was reddened, somewhat tender to touch, and distended with fluid effused from the tunica vaginalis; but there was no pain or constitutional disturbance. On the 5th Dec., the swelling, which had been gradually subsiding, had nearly disappeared; and on the 12th the scrotum had regained its normal size. The scrotum and the spermatic cord on the affected side were firmer than before; and a cylindrical cord extended along the whole of the inguinal canal, as well as along the spermatic vessels down to the testicle. The patient was completely cured, and he left the hospital able to walk easily and without fatigue, and without any reappearance of the hernial tumour.

The third case occurred in a young man, aged 27, who had had a large oblique inguinal hernia of the left side for eight years, for which he had worn all sorts

of trusses without benefit. At the desire of the patient, and after a consultation with his colleagues, MM. Rayer, Begin, and Cloquet, M. Jobert punctured the hernial sac, and injected the tincture of iodine into the tunica vaginalis. The patient was laid in the horizontal posture after the operation; his legs were flexed on his thighs, and maintained in this position by means of a bolster. The hernial sac swelled immediately after the injection, and in fifteen hours it was as large as the hernial tumour, and had a fluctuating, half-solid feeling. The parts continued thus for eight days, and then the swelling speedily diminished. On the twenty-eighth day after the operation the cure was complete; the track of the spermatic cord was occupied by a hard and compact cylindrical substance, which completely prevented any return of the hernial protrusion, and allowed the patient to go about without any recurrence of his malady. Four months had elapsed at the time when M. Jobert related his case, and the cure was still as complete as at first; the inguinal canal was filled with a hard, solid cord; and there was no atrophy of the testicle.

Remarks.—When the sac is adherent to the surrounding parts—when the hernia is congenital—when the sac is distended with fluid, or is thickened or cartilaginous—M. Jobert recommends puncturing the sac at once with a trocar, without previously making an incision. The trocar is to be inserted perpendicularly at first, then obliquely upwards, and, when it has reached a certain depth, it is to be directed cautiously backwards so as to puncture the sac—the accomplishment of which process is known by the operator no longer meeting with resistance, and by his feeling the canula move about without obstruction in the empty sac. Having entered the sac, the pure tincture of iodine should be injected into it by means of a glass syringe—whereby we may be able to know how much fluid has been used. M. Jobert ordinarily injects one or two spoonfuls (*cuillerées*) of the tincture, but often much less. The quantity injected should always be in proportion to the size of the serous pouch. The injection should be suspended whenever we feel an obstacle to the entrance of the liquid. Instead of allowing the fluid to escape through the canula, M. Jobert withdraws it by means of the suction of the syringe.

When the sac is thin, movable, and easily displaced, Jobert exposes it by dividing the integuments with a bistoury, and *thereafter* punctures it with the trocar—*never with the knife*. In such cases he unites the lips of the external wound by means of twisted sutures.

What occurs after the operation? Immediately after the injected fluid is withdrawn, a new liquid begins to be thrown out within the hernial sac. After twenty-four or forty-eight hours have elapsed, the sac is filled with this fluid; it continues in this state for eight days, and after the first week it begins to diminish gradually and visibly in bulk. In proportion as the tumour diminishes it begins to acquire solidity, and becomes as firm as wood; nay, it eventually becomes as hard as bone or ivory.

M. Jobert affirms that this method by injection is superior to all the other operative procedures which have yet been devised for the radical cure of hernia, inasmuch as it is at once simple and harmless. M. Jobert, in conclusion, acknowledges that this operation is not invented, but merely revived, by him; inasmuch as M. Velpeau, in his *Annales de Chirurgie*, published ten years ago, first suggested the likelihood of injections of iodine proving beneficial for the radical cure of reducible hernia.—*Month. Journ. of Med. Sci.*, Dec., 1854; from *Gazette des Hôpitaux*, August 10, 1854.

OPHTHALMOLOGY.

46. *Employment of Chloroform in Operations on the Eyeball.*—MR. HAYNES WALTON, in a clinical lecture delivered at the Central London Ophthalmic Hospital, made the following remarks on the amount of assistance that chloroform is capable of affording to an operator, and the mode in which it may enable

him to obtain a more successful result; how far that assistance is really needed, and when it is judicious to dispense with it.

"Most unquestionably in infancy," he remarked, "chloroform does afford very great assistance; for, without it, resistance on the part of the little patient is certain. You must have witnessed repeatedly the difficulty there is, in certain diseases, in unclosing the eye of an infant so as to obtain a satisfactory view; and surely you cannot forget the little battle that ensues, and the terrible screams that accompany and follow the attempt. The very diminutiveness of the organ, whereby there is much less room for the use of the fingers and for instruments, together with the great delicacy of the parts, demand the utmost exposure of the surface of the eyeball, with the greatest steadiness. In former years, the operation for congenital cataract was frequently postponed, because these desiderata could not be commanded; and I believe that occasionally in the present day their acquisition is considered impossible, and an operation delayed to the great detriment of the patient. I strongly suspect that the reason why the posterior operation for 'solution' has been so frequently advised in infancy, is because of the greater nicety required to perform the anterior one; and you will at once understand, that, without the paralyzing effect of chloroform, to retract the palpebræ, introduce a needle in cornea near the circumference, direct the point in the manner desired against the capsule of the crystalline lens and the lens itself, without touching the iris, or using injurious force, is no easy matter, or, at least, it may not be easy. True it is, that a modern lid retractor, of silver wire, or steel, removes some of the difficulty, but only a part, for the difficulty of securing the child, as well as other obstacles, still remain.

"Again, from what I hear, and indeed from what I see, it would appear that, before the use of the anæsthetic agent, the operation for congenital cataract was not unfrequently left unfinished. The capsule which blocked up the pupil was not always removed; and the operation, in any given case, was more often repeated, not merely from the erroneous idea that then existed, and unfortunately still does exist, about repeating needle operations; but also on account of the difficulty that frequently prevented the surgeon from carrying out his previous intentions.

"With children, and young persons in general, even when an operation is painless, there is an expectation of something worse than what is actually felt, and generally a deficiency of resolution, that renders it impossible for them to be sufficiently quiet without violent resistance, or the employment of mechanical restraint; and failures and mishaps are more commonly due than could be supposed, to the unsteadiness of the eye. If I had noted down all the instances that I have witnessed of foiled endeavours, they would form a large page in my note-book.

"I have alluded to the necessity of mechanical restraint to subdue the resistance of a refractory patient, and this is frequently very distressing to the friends. The public are becoming alive to the disadvantages of the plan, and frequently demand the use of chloroform. A lady called on me recently to make an arrangement for an operation on her child's eye, and at once asked if I used chloroform in such cases. I answered that I did, and, on requesting her reason for the question, she said, 'I have just left the house of Mr. ———, who has declined to employ it; he declared that, by rolling up my child in a tablecloth, and with the aid of two strong assistants, he could manage her, or the most unruly young lady. I told him that her timidity and highly nervous susceptibility made me dread the effect of such an ordeal on her mind.'

"Although there is no necessity to quit my own experience to furnish examples of the difficulty that there may be in operating when opposition is to be encountered, I shall borrow one which occurred to Mr. Wardrop, who had the disadvantage of practising when a calm sleep could not be exchanged for the violent struggles of a refractory patient. The quotation is from Dr. Mackenzie's work. 'A boy fifteen years of age, blind and deaf, at first yielded readily, and allowed himself to be placed and held on the table. The uneasiness, however, occasioned by the pressure necessary to keep the eyeball steady and the lids open seemed to overcome his resolution; and his exertions became so violent,

that it was quite impossible to secure even his head. A second attempt was made the day following, more precautions being taken to secure him; but so violent were his exertions and cries, and so irascible did he become, that all present were glad to relinquish their posts. Some days after, a wooden box, the sides of which moved on hinges, was folded round his body, and fixed by circular ropes, and in this way, notwithstanding a powerful resistance, he was placed on a table and kept quite steady. Much difficulty was found in holding open the lids, and keeping the globe of the eye steady. As soon, however, as the needles touched the eye, he remained quite still, and his screaming ceased. Mr. Wardrop and others have bled persons till they fainted, in order to obtain a passive state.

"I now pass to those operations on the adult eye, in which we may receive considerable assistance from the insensibility of the patient. It is evident that here there should be a distinction between such cases as require chloroform merely because of lack of moral courage, and those in which it is of positive advantage under any condition; as in the one we may leave the choice to the patient, in the other it is our duty to recommend it; as it is a fact that, with the fullest consent and greatest determination on the part of a patient, indeed, with a resolution that could endure a limb to be severed from the body without a groan or a cry, and with every desire to assist the operator, there are cases in which anæsthetic sleep may be advantageous. The majority of operations for artificial pupil, especially where the proceeding is complicated, and requires the use of more than one, or the re-introduction of the same instrument, falls under the latter category. An eye, for the most part, that requires this aid, is much damaged; the parts with which we have to deal are altered, and the vitreous humour is too frequently disorganized; so that we need the greatest steadiness of the eyeball, with long continuance of a given position, and an absence of much pressure. Now, the movements of the eyeball may be quite involuntary, and the eyelids will twitch, in spite of the most resolute will. But not the least disadvantage of consciousness is the compression that the straight and oblique muscles can and do exercise in such operations. When acting violently, they exert considerable influence; and the effect of such an agency, at such a time, is always hazardous, in several ways. Again, in many operations of general surgery, the sooner the manipulation ends, and the instruments are out of the body, the more certain is the result; this is doubly true of the eyeball. These remarks may be said to apply, in the main, when the eyeball is to be opened for the extraction of any body, be it capsule, animalcule, or any particle driven into it from without, when decided difficulty or intricacy is apprehended.

"I have purposely delayed till now to refer particularly to the formation of an artificial pupil in an infant or a child, because, after what I have said, you may the better appreciate the difficulty attending the execution of such a task, and the better recognize the assistance to be derived from a perfectly passive state. An infant that has lost its pupil from purulent ophthalmia, or any other cause, is not now doomed to darkness till the adult period, or, at least, need not be, as in past years. We can operate on the smallest eye, and the consent of the patient is not necessary.

"The extraction of a cataract may undoubtedly be better performed under the influence of chloroform, both when there is a certain deficiency of moral courage, and when there exist conformations of the eye that render the operation peculiarly difficult, and requiring more than usual dexterity. But as it is after the meridian of life, and often in the very aged, that extraction is needed, we should be careful not to use chloroform needlessly. In the early period of our existence, the risk of a fatal termination is exceedingly small. Not so, however, in the old and enfeebled, in whom the heart is so often diseased by being degenerated—a state which the most rigid scrutiny during life may fail to detect. Disease of the heart, however, has not been always found in those who have been killed by chloroform. Remember, too, that the operation is unattended with pain when well executed. Therefore, you should not use such an agent as a matter of routine, but withhold it when it may be dispensed with; and whenever you intend to employ it, exercise the most searching scru-

tiny as to its admissibility. We should not lightly place an aged person in a state so closely resembling apoplexy, that the most astute physician could not, at the moment, point out the difference.

"Timidity, and the accompanying restlessness of a patient, may render it impossible to operate successfully. I have seen this over and over, even under the hands of operators who have not been surpassed in this kingdom for self-possession and brilliancy in execution. On each occasion, the eye was either lost or much damaged, from the unavoidable results of operating against the patient's resistance. Here chloroform removes all difficulty. I cannot give a case more to the point than the following, which is reported from this Hospital in the 'London Practice of Medicine and Surgery,' in the *Medical Times and Gazette*.

"A man, 65 years of age, with hard cataract in each eye, was brought up to the operating-room, for extraction to be performed on one of his eyes; but so great was his fright and agitation, that he could not be induced to keep quiet enough to allow his eye to be sufficiently open for the operation. He almost fainted when ascending the stairs, and required to be lifted by the porter. This was from sheer mental emotion, for he was neither feeble nor infirm. As there was no probability of his ever having self-command enough, Mr. Walton determined to give him chloroform. It was decided, however, not to do it on that day, for he had recently taken food, but on the following morning, when his stomach should be empty. This was carried into effect on Friday, and the operation was then easily and successfully performed."

"It remains for me to mention those physical peculiarities of the eye which impede extraction, and which may be surmounted by the aid of chloroform. They are mainly those that present impediments to exposing and steadying the eyeball sufficiently to enable the cornea to be divided in an ample manner, such as a sunken eye, a narrow palpebral aperture, unusual prominence of the orbital ridge. In any of these states, more pressure with the fingers is generally required than the eye will bear. During stupor, the eyelids can be more widely extended, and the eyeball fixed with a lightness of touch that would, on account of the peculiarities, be insufficient during sensibility; there being, in fact, all the difference between involuntary resistance, however slight, and absolute quiet. Beyond this chloroform does not assist us.

"When a patient has tolerable fortitude, at all events whatever be his mental emotion, so long as he remains master of his will, and can direct his eye to the position desired, and there are not impediments to exposing his eyeball to the required extent, I would rather that he retain his senses during the operation, for then I believe that the crystalline lens is better started from its position, that it escapes more readily, and that the pupil is the more quickly restored to its natural state, and the iris less liable to prolapse after the terrible stretching it has received. Even supposing this not to be the case, and things are equal, is it not far better to save an aged person, if only from all the formality and distress of an inhalation. Is it not better to see a patient rise and walk to his bed or couch, rational and thankful, than for him to be removed, half-conscious, sick, miserable, and requiring careful and anxious attention, both on account of the constitutional effects of the chloroform, and the injury he might inflict on his eye. At a meeting of the Royal Medical and Chirurgical Society, on the 14th November, when a paper on an ophthalmic subject was read, Mr. Ferguson asked to what extent chloroform was employed in operations on the eye, meaning the eyeball, and with what success. Mr. Dixon said that a patient might not vomit soon after the inhalation of chloroform, but he might remain twelve or twenty-four hours in a sickly, squeamish state, with but little appetite for food. This was more injurious than vomiting, for it was of the greatest importance, especially in old persons, that the nutrition of the patient should be well maintained, and the eye kept perfectly at rest.

"I have several times calmed the fears of elderly persons on whom I was about to perform extraction, and dissuaded them from inhaling chloroform, and in every instance I have been thanked for my advice. I have been asked repeatedly, even by patients, if the anæsthetic sleep does not give confidence to the operator, and enable the operation to be the better done. I can only say, that

if, in any given instance, this is likely to be true, the timid surgeon had better avail himself of every admissible assistance that is likely to restore sight to his patient. He had better narcotize him, and, if needs be, stimulate himself.

"I shall not enter into the details of preparing a patient for taking chloroform, the mode of giving it, nor the after treatment. This I have dwelt on elsewhere; but I will tell you—be certain of insuring complete insensibility before you begin to operate, that all the steps of the operation may be over before the patient is sensible, for you can have no greater misfortune than for him to awake before the termination, and to commence struggling. There is a particular point in the after treatment that must be attended to, the neglect of which has cost several eyes; and this has induced me to say in my work on the Eye: 'Except the operation for the extraction of cataract, or the division of the cornea to a like or nearly equal extent, for any other purpose, there is no operation on the eye in which well-founded objections exist to the previous use of chloroform or ether as anæsthetic agents. In the exceptions mentioned, the objection arises from fear of vomiting, and the loss of the vitreous humour. It is true, that vomiting is but an occasional effect of the use of these agents, and it can nearly always be prevented by emptiness of the stomach previous to inhalation, and which is secured by enjoining a strict fast for four or five previous hours; but, after every precaution, vomiting may occur, or severe retching, which is equally injurious.'

"I wrote this from instances that had come under my notice, and I dreaded to employ the chloroform. Subsequent experience convinced me that all necessary precautions had not been used. If the eyelids are carefully retained together with court-plaster, there is little if any ground for misapprehension, except there be that degree of vomiting which could perhaps arise only under an absence of those preliminary precautions to which I have alluded. In the case above quoted from 'The London Practice of Medicine and Surgery,' delirium tremens came on, and, in spite of the violent agitation of the man, during which the head was tossed much about, the success of the operation was not interfered with, because the eyelids were plastered together, and the eyeball protected by a guard made of wire and cloth."—*Med. Times and Gaz.*, Jan. 27th, 1855.

47. *Opacities of the Vitreous Humour.*—Opacities of the vitreous humour, of every variety of form, belong to the commonest ophthalmoscopic discoveries in amblyopic affections. Whence do they arise? Dr. GRAEFÉ believes their causes are manifold; but he draws particular attention to one, namely, intra-ocular hemorrhages. A sudden cloud, sometimes of reddish hue, may obscure vision wholly or in part, or spots of every variety of form may float before the sight. In some cases there is complete absorption of the extravasated mass. In most instances, however, filamentous, or flocky dark spots, remain. The commonest forms are punctated, filamentous, membranous, flocky, or irregular. The first are not easily overlooked. The causes of intra-ocular hemorrhage lies, doubtless, in disease of the coats of the bloodvessels; for the contents of the globe are constantly under a state of pressure, and, therefore, the commonly exciting causes of hemorrhage are removed. The chief source is sclerotico-choroiditis posterior, which leads to chronic inflammation and distention of the capillaries in the back part of the globe. But intra-ocular hemorrhage may ensue from simple hyperæmia of the choroid, dependent upon some unknown change in the strength of the walls of the vessels.—*Med. Times and Gaz.*, Feb. 17, 1855, from *Archiv. für Ophthalmologie*.

48. *Effusion of Blood into the Orbit.*—A labouring man, aged 19, consulted Dr. GRAEFÉ for double vision of four days' duration. A fixed expression of the left eye immediately struck the attention; its axis of vision was either in a state of unnatural convergence or divergence with the right eye, in various movements of the head. More accurate investigation convinced the author that there was complete paralysis of the musculus rectus inferior, musculus rectus superior, and musculus obliquus superior; partial paralysis of the musculus rectus internus, musculus rectus externus, and of the nervus opticus; so that the func-

tions of four nerves were affected—namely, the N. oculo-motorius, N. trochlearis, N. abduceus, N. opticus. There were no cerebral symptoms whatever. Taking this circumstance into consideration, as well as the fact of the suddenness of the attack, Dr. Graefe diagnosed effusion of blood behind the bulb of the eye, with pressure on the nerves.

He also relates two cases of suddenly excited exophthalmus, from caries of the orbit; and the *post-mortem* observations on a case of paralysis of the oculo-motor nerve. The corresponding optic nerve was shrunken up to the chiasma; the oculo-motor nerve one-half thinner than the opposite; but there were interspersed opaque white blots in its tissue, which were seen to be caused by thickening of the neurilemma. In the left corpus striatum there was a softened spot. The muscles of the globe were atrophied.—*Ibid.*

49. *New Ophthalmoscope.*—After having reviewed the different ophthalmoscopes of MM. Helmholtz, Follin and Nachet, Ruette, Jaeger, Jr., and Coccius, M. A. ANAGNOSTAKIS describes that which he has invented. This instrument consists simply in a small concave mirror, round, and of a diameter of one inch and a-half, of a focal distance of four inches and a-half; the surface, tinned over, is covered by a plate of blackened copper. The centre of the mirror is pierced by a widening aperture of two lines (four millimetres) diameter. A little ivory handle serves to hold the instrument.

The patient, whose pupils have been previously dilated by a solution of the neutral sulphate of atrophine, is seated in a darkened chamber by the side of a table, on which is put a good lamp deprived of its globe. The lamp should be as close as possible to the patient, and the flame brought to a level with the eye about to be examined. The surgeon, seated in front of the patient, holds the instrument, the reflecting surface turned towards the eye to be examined, the posterior surface applied to the eye of the examiner in such a way that the eye can look through the central aperture of the mirror.

This being done, the surgeon turns the instrument a little inwards; a luminous reflection is cast upon the diseased eye. On withdrawing it a little, he sees the reflection growing smaller and smaller, until it becomes oblong, constricted, and very brilliant.

A look is then taken obliquely, through the hole in the mirror, into the eye subjected to exploration. It is in this situation, a little modified, according to circumstances, that one perceives the different alterations in the retina and in the media of the eye.

The author, who has explored in this manner a great number of diseased eyes alone or in concert with M. Desmarres, has obtained the following important results:—

1. *Exploration of the Crystalline Lens.*—Upon three patients, supposed to be suffering from amblyopia, M. Desmarres ascertained that there were three incipient cataracts, which had escaped observation. The author affirms that every opacity can be thus detected.

2. *Exploration of the Vitreous Humour.*—In three patients suffering from different degrees of troubled vision, M. Anagnostakis has observed corpuscles of a more or less deep brown colour, of different forms, and of variable volume seated in the vitreous humour. Are they inflammatory effusions, or rather traces of former hemorrhages. In a patient affected with synchysis, M. Desmarres has seen these corpuscles replaced by crystals of cholesterin.

3. *The Retina.*—The instrument has revealed injections of the vessels of the retina and sub-retinoid ecchymoses. In one patient there was a true sanguineous effusion under the whole retina, apparently from former hemorrhages. Three times did M. Anagnostakis diagnose sub-retinoid dropsies.

Finally, M. Anagnostakis thinks that in one patient he saw thickening of the retina.—*Lond. Med. Times and Gaz.*, from *Gazette Médicale de Paris*, Jan. 6, 1855.

MIDWIFERY.

50. *Secondary or Blighted Fœtus at the third month, with Fatty Degeneration of the Membranes, retained and expelled with its Living Co-twin at the eighth month of Utero-gestation.*—The following example of this was read before the Glasgow Medical Society, by JAMES G. WILSON, M. D.:—

Mrs. T., aged 32, a strong, healthy, and masculine looking woman, was, on the 21st November last, after a short and easy labour, delivered of a living female child. It presented with the breech in the right anterior dorsal position, and weighed 5½ lbs., which, from appearance as well as from the mother's statement, indicated its age to be about the eighth month. The placenta was expelled entire in about the usual time. Patient states that five years ago she gave birth to twins, both boys, alive and at the full time, and that, with the exception of the present, she has had no children or abortion since. Her catamenia were regular during the interval, and she menstruated for the last time about the beginning of March. When three months or thereby advanced in her present pregnancy she had twice in succession a considerable sanguineous discharge per vaginam, accompanied with slight uterine pains. She, however, paid little attention to these symptoms at the time, as they did not occasion any anxiety or alarm.

The placenta, on examination, seemed generally healthy, with the exception of a few small indurated points of a tubercular character scattered over various parts of its extent. Towards the margin of the placenta there was found a small fœtus closely enveloped in a yellowish fatty-like mass, which lay in a groove or sulcus formed in the placenta by the overlapping or bulging over of a portion of its foetal surface. The whole was connected together by a prolongation of the placental substance into the inner border of the investing membranes of this small fœtus. The bond of attachment was by a spot not larger than a shilling, which was very slender, and separated in two places. The fatty-like mass, when opened up, was of an irregular circular form, measuring eight inches in circumference, and was of the thickness of ordinary plate glass. It appears to be a homogeneous mass on cutting into it, and of a uniform density and consistency throughout. On subjecting a portion of it to microscopic examination it is found to consist of a fibrinous or lymph substance with numerous oil globules intermixed. It is soluble to some extent in ether. There was no appearance of liquor amnii. The fœtus appears to be about the third month—is very much flattened out and disfigured, having a white exsanguine look, with no traces of incipient putrefaction. It measures 4½ inches in length. The right arm is to some extent malformed and displaced; the humerus, with its atrophied muscles, is completely incorporated with the side of the head, while the forearm is altogether above the head, and appears as if originating from it. In consequence of the pressure to which the fœtus has been subjected a large portion of the bowels protrude. The sex of the child cannot be ascertained. Proceeding from the fœtus is a small thready or filiform umbilical cord, 2½ inches long, with the spiral convolutions formed by its vessels in great part obliterated. It is inserted into a spongy cellular substance (about the size of half a crown), much resembling placental tissue, situated at the inner border of this mass of fat, and which seemed to be the point of union with the placenta of the living child above referred to. There is, however, no distinct line of division observed between these two textures, as the one passes gradually and imperceptibly into the other. On the supposition that this substance, to which the cord was attached, was the placenta of the fœtus, the formation of the peculiar groove in which the whole was embedded may be thus explained: The two placenta appear to have been originally united by their edges, the lesser one having ceased to enlarge, the other one gradually extended in circumference, till ultimately a flap-like portion, developed from its foetal surface, covered in the other along with the fœtus. The relative position, however, of the double set of membranes, in consequence of some accidental laceration either during or after the expulsion of the placenta, could not be accurately ascertained. It is impossible to tell, on inspecting the prepara-

tion, what uterine attachment the blighted fœtus, through the medium of its placenta, originally possessed.

There can be no doubt, I think, but this was primarily a case of twin conception, while one was blighted at or near the third month, and the other carried to the eighth, when both were nearly simultaneously expelled from the uterus. One peculiarity in this case is the retention of a dead fœtus *in utero* for such a length of time, and its ejection a few minutes after the birth of the living child; yet when the placenta are united in a twin gestation, it is difficult to conceive how one fœtus could be discharged without the expulsion of both. Were the symptoms of threatened abortion, already noticed, an attempt of nature to throw off and get rid of the dead child? or had they anything to do with its death, or were they the cause or the result of the partial separation that was afterwards found to exist between the two placenta? Whatever view may be taken of these points, one thing seems evident, viz: that the symptoms of miscarriage closely correspond with that supposed period of utero-gestation at which the blighted fœtus had arrived.—*Monthly Journal of Medicine*, January, 1855.

51. *Instrument for Dilating the Os Uteri*.—Dr. BRAUN has contrived an instrument, which he names *colpeurynter*, for the dilatation of the os uteri. It consists of a vulcanized India-rubber bladder, from two to four inches in diameter, and four inches in length, with an India-rubber tube enclosed in horn, fitted with a brass stopcock, and a ring through which to pass a silk belt. When used, the India-rubber bladder is introduced empty into the vagina, then gradually distended by injecting cold or warm water. It is retained *in situ* by the belt fastened round one or both thighs or hip. The horn cylinder is curved in the direction of the pelvic axis; it allows only the upper end of the vagina to be stretched by the bladder: and obviates any unnecessary pressure upon the urethra or external parts. The operation is called *colpeurynter*. The advantages of this proceeding over other methods of dilating the os uteri, whatever the indication for that operation, are highly extolled.—*Brit. and For. Med.-Chirurg. Rev.*, Jan., 1855, from *Klinik der Geburts*.

52. *Influence of Uræmic Intoxication in the Production of Puerperal Convulsions*.—New light has been thrown upon the influence of uræmic intoxication in the production of eclampsia by Dr. CARL BRAUN, though some of his propositions require further testing before they can be accepted. The following is a recapitulation of the views he entertains on the subject:—

1. Convulsions in females during the generative period, depend either upon hysteria, epilepsy, diseases of the brain, mineral or vegetable poisoning, or upon uræmic intoxication.

2. The most frequent cause of eclampsia is uræmia and Bright's disease.

3. Chronic hysteria and epilepsy exert no injurious influence upon pregnancy and labour, or upon the life of the fœtus; they are never connected with Bright's disease, nor are fibrinous cylinders or a large quantity of albumen found in the urine.

4. Primary diseases of the brain, as apoplexy, meningitis, &c., are rarely causes of convulsions; and when they occur simultaneously with Bright's disease, they are the consequences, not the cause, of the convulsions.

5. All forms of convulsion, with their different causes, may occur without pregnancy.

6. They may also occur in males from any of the above-mentioned causes, except hysteria.

7. Amongst the more important causes of Bright's disease in the pregnant, may be considered venous congestion of the kidneys from the pressure of the enlarged uterus and distended abdomen, as well as the sanguineous diathesis often connected with pregnancy.

8. In *nephritis diffusa*, urea becomes detained in the blood, is converted into carbonate of ammonia, and then excites convulsions.

9. In cases of Bright's disease during pregnancy, if carbonate of ammonia be found in the blood, a speedy outbreak of convulsions may be expected.

10. No convulsions, however, occur in Bright's disease in the pregnant, provided but a small quantity of urea accumulates in the blood, or does not become converted into carbonate of ammonia.

11. The strong action of the uterus during labour is not the cause of this conversion, or of the uræmic convulsions; for these occur nearly as frequently without labour, during pregnancy and in the puerperal state; and sometimes in those who are not pregnant.

12. The abortions which frequently occur in uræmic convulsions are the effect of the uræmia, not the cause of the convulsions.

13. Eclampsia—*i. e.*, uræmic convulsions—has no immediate connection with the pains or process of labour.

14. The albuminuria cannot be the result of congestion occasioned by the convulsions, for it precedes the uræmic convulsions for days or weeks, and does not, as a rule, occur under other forms of convulsion, as from hysteria, epilepsy, &c.

15. Attacks of eclampsia *during pregnancy* may cease, and the albuminuria continue.

16. But if the attacks cease *after labour*, and there be present only a slight degree of Bright's disease, the albuminuria disappears after a few days; but continues for a longer time if the disease of the kidney be in a more advanced stage.

17. The diminution in the size of the uterus from the completion of labour contributes very much to the disappearance of albuminuria, when there is neither fatty degeneration nor atrophy of the kidneys.

18. Bright's disease (without convulsions) never gets well during pregnancy; after labour it mostly disappears within a much shorter time.

19. Albuminuria will be found in all cases of eclampsia not dependent upon hysteria, epilepsy, primary cerebral diseases, or poisoning.

20. Epilepsy may take place simultaneously with Bright's disease and uræmic convulsions; and Bright's urine may be found in an individual affected with habitual epilepsy.

21. With a frequent repetition of uræmic convulsions the foetus dies, through the injurious influence of blood impregnated with carbonate of ammonia. The life of the foetus is not compromised by the mechanical effects of convulsions which depend upon hysteria, epilepsy, or diseases of the brain.

22. Examination after death from uræmic convulsions always discovers Bright's disease of the kidneys; more frequently in the primary than in the latter stages (of fatty degeneration and atrophy).

23. Oedema and anæmia of the brain are as frequently found after death from eclampsia, as hyperæmia and consecutive apoplexy.

24. Reflex sensibility is entirely abolished during each uræmic attack, but during the intervals loss of consciousness generally increases.

25. The injurious effects of venesection generally in eclampsia have been observed by Kiwisch, Litzmann, Sedgwick, Blot, and King, and the uncertainty of the practice has been confirmed by our own experience. In cerebral eclampsia, however, venesection is a proper remedy.

26. Chloroform inhalations are the best means of mitigating and bringing to an end uræmic convulsions, either during pregnancy, labour, or in the puerperal period.

27. The diuretics most to be relied upon for the relief of uræmia and Bright's disease are the benzoic, citric, and tartaric acids.

28. Bright's disease during pregnancy admits of relief only, not of cure.

29. Lessening the size of the uterus, and removing the child, are the most effectual means of curing the affection of the kidneys and uræmic intoxication.

30. Exciting and hastening labour by artificial means diminishes the danger both to mother and child arising from uræmic convulsions.

31. Artificial premature delivery is not, as a rule, to be resorted to in Bright's disease; but is to be recommended in actual uræmic convulsions.

32. The most appropriate method of producing premature labour, and hastening delivery in cases of uræmic eclampsia, is by forcibly dilating the vagina by means of a caoutchouc apparatus (*Colpeuryse*).—*Brit. and For. Med.-Chirurg. Rev.*, from *Klinik der Geburtshilfe*, &c.

53. *Puerperal Mania*.—Dr. WINN, in a paper on this subject, read before the Medical Society of London (December 2, 1854), observed that the prevalence of opinions with regard to the moral management of patients suffering from puerperal mania, which he held to be not only erroneous, but dangerous, had induced him to bring the subject before the Society. He expressed his intention of passing briefly over the division of the subject which related to the physical treatment of the disease, in order that he might dwell at more length on its moral management—a question of far greater importance, and on which he considered the successful termination of a case more especially to depend. Dr. Winn remarked, that insanity might attack a patient at three periods during the puerperal state: 1, during gestation; 2, subsequent to delivery; and, 3, during lactation, as a result of protracted suckling. The observations which he was about to offer had especial reference to the second form of the malady, to that maniacal excitement which supervenes during the first months after delivery, and appeared to be the result of extreme irritability of the brain associated with great nervous exhaustion. Before entering upon the immediate subject of his paper, Dr. Winn took objection to the term “puerperal mania,” used, as it commonly is, in a specific sense, and the use of which term had led to serious errors in practice. Adopted as an expression of a mere variety of insanity, it was sufficiently distinctive and appropriate; but a serious mistake was involved in the supposition, that puerperal mania is a special form of insanity, requiring a treatment entirely different from that which is laid down for the cure of mania in general. No doubt the peculiar condition of blood and the excitable state of the nervous system which obtain after delivery tended to modify a maniacal attack, and rendered it necessary that the treatment should be adapted to these qualifying circumstances. Nevertheless, the affection was essentially mania, and was to be treated as such. He then took a review of the symptoms of puerperal insanity, to show that they were perfectly identical with those of an ordinary attack of mania. The importance of this generalization would be clearly shown when he came to consider the expediency of removing puerperal lunatics to an asylum. Dr. Winn proceeded next to determine, as far as the present state of mental pathology would permit, the condition of the system which gave rise to the puerperal mania. Careful observation had led him to infer that the disease was the result of cerebral irritation, combined with nervous exhaustion, a deteriorated condition of the blood, and an imperfect nutrition of the nervous tissue. Granting these facts, an antiphlogistic mode of treatment was decidedly objectionable. The classes of remedies most likely to prove beneficial were, sedatives, depuratives, and tonics. A few leeches might occasionally be employed to allay the erethism of the brain, but on no account as a depleting measure. Venesection and blistering were positively injurious. A nourishing though not a stimulating diet was generally required; while opiates, aperients, and diuretics, would all be found more or less useful. It was, however, on the moral treatment that the recovery chiefly, if not wholly depended. This should be precisely the same as that which is generally indicated in any variety of mania. An efficient nurse must be provided, capable of maintaining a kind but firm control over the patient. Every precaution must be adopted to prevent self-injury. The greatest tranquillity must be preserved, and the patient's friends and relatives rigorously removed from her presence. The great object was, to break the morbid current of thought by a seclusion more or less complete, and thus give rise to emotions and ideas entirely new. The most important point for consideration, however, was the question of the patient's removal to an asylum when the ordinary remedies had failed to afford relief. However inexpedient and culpable it might be to hurry a patient to an asylum at the outset of puerperal or any other form of mania, still a period might arrive when delay in having recourse to this measure would subject the patient's health and life to the greatest danger. Dr. Winn stated, that the general opinion was opposed to the removal of a puerperal lunatic to an asylum. To combat a dogma, fraught with such imminent peril, was the principal object of his communication. Dr. Forbes Winslow had clearly established the fact, in his valuable treatise on the Incubation of Insanity, that it was only during the earlier periods of an attack of mania that a cure could be expected from the use of appropriate remedies.

By a strange perversion of a general principle, this important truth had been commonly lost sight of in the treatment of puerperal mania. In support of this assertion, Dr. Winn quoted passages from the works of various authors who had condemned the removal of puerperal patients to an asylum. One of these authorities had made use of the very objectionable expression "madhouse." Dr. Winn commented very strongly on the use of such a term. It was appropriate enough for the asylum of the darker ages, when every description of cruel restraint was practised; but singularly inapplicable now, when humanity and science were alike enlisted to allay the sufferings and meliorate the condition of their inmates. It was to be hoped that, ere long, even the milder term of "lunatic asylum" would be abolished, and the far more appropriate title of "Hospital for the Cure of the Insane," as recommended by Dr. Winslow, be generally adopted. He next related cases from his practice to prove the truth of his views. One patient whom he attended with Mr. Meeres, proved in the most striking manner the advantage of an early removal. Although the patient had been ill only a week, Dr. Winn thought it advisable to send her to an asylum at once. Her recovery was rapid, and she has been confined again since her return to her home, but has fortunately had no recurrence of the disease. Another case which he mentioned showed, on the other hand, the bad effects of delay. This patient had been ill for many months before Dr. Winn saw her, and all the routine practice had been tried in vain. Had early removal been adopted, he thought the case would have speedily recovered. The returns of lunatic asylums showed, that the treatment of puerperal mania in these institutions was pre-eminently successful. Dr. Webster's tables proved, that more recoveries from puerperal insanity were reported than from any other form of lunacy. Dr. Boisragon, of the Cornwall Asylum, had kindly furnished Dr. Winn with the result of his experience. He (Dr. Boisragon) stated, that the majority of cases recover; he had not, in fact, discharged a single case that was not convalescent. Some, who admitted the efficacy of this plan of treatment, disapproved of it on the ground, that the recollection of having been confined in an asylum would produce an injurious effect on the mind of the patient in a subsequent labour. Such an instance Dr. Winn had never witnessed. Another common prejudice, which interfered with the timely removal of a patient to an asylum, was the dread entertained by the friends of the individual lest some sort of opprobrium would be incurred by her having been the inmate of what they call a "madhouse." This prejudice, the offspring of ignorance and mistaken delicacy, was fast passing away. The supposition that puerperal mania was generally a very transient affection was another erroneous opinion, which had occasionally interfered with the early removal of patients. In conclusion, Dr. Winn wished it to be clearly understood that he did not recommend the hasty and indiscriminate removal of patients; at the present time he had a patient, affected with insanity dependent on uterine derangement, under his care, and who had been ill for several months before he saw her. In this case, physical treatment had almost completed a cure, and removal to an asylum had not been had recourse to. In the humble walks of life, cases of insanity should be generally removed at an early period to an asylum. For rich patients this measure was not always necessary; they could obtain a quiet residence, all the comforts, and many of the advantages of an asylum. Not so with the poor; confined, perhaps, to a close room in a narrow and noisy street, insufficiently nourished, and badly nursed, the poor patient was cut off from all hope of a cure. For cases of this description an asylum offered the only chance of a cure.

Dr. Webster concurred with the author in most of his views and recommendations; but he considered that much benefit was derived by patients, especially those in the lower ranks of life, becoming inmates of lunatic asylums. 167 cases of puerperal mania had been received into the Bethlehem Hospital during the last ten years. Of those cases in which the disease commenced immediately after delivery, a very large porportion (65 per cent.) had been cured. In the cases arising from protracted lactation, the cures were not so numerous. The mortality attendant upon puerperal mania had, of late years, considerably diminished, it being now only 4 per cent.; and this, he believed, was owing to the discontinuance of the practice of bleeding, and better treatment generally.

In the higher ranks of society, where puerperal mania was more common than in the lower, it was not so necessary that patients should be sent to an asylum; but even they might sometimes be benefited by such a removal from familiar scenes. The disease resembled delirium tremens, and should be treated much on the same principles.

Several members expressed objections to sending cases of puerperal mania to lunatic asylums.—*Med. Times and Gaz.*, Dec. 9, 1854.

54. *Discussion at the Imperial Academy of Medicine on M. Depaul's Report on the Treatment of Displacements of the Uterus by Intra-Uterine Pessaries.*—After some remarks from M. Piorry, the discussion on this report, of which an abstract was given in our preceding No., p. 257, *et seq.*, was opened by M. MAL-CAIGNE, who, while admitting the zeal and talent of the reporter, regretted to find so much party spirit in the report, which more resembled the impassioned argument of a special pleader, than a plain and impartial judgment of facts. He could not consider as *repugnant to common sense* a treatment, which had been practised by Velpeau, Amussat, Simpson, Huguier, and Valleix. It appeared even from the report that in some cases, the intra-uterine pessary had been useful, and sometimes it had procured instantaneous relief, showing that the symptoms were really owing to displacement, and he had often witnessed cases of descent of the uterus, where the use of a pessary had put a stop to all the symptoms; the hypogastric belt also, by replacing the uterus, had often produced the same beneficial results. It appeared to him, that the important inquiry was to establish a differential diagnosis of uterine affections; since the same symptoms were ascribed by some exclusively to displacements, by others exclusively to enlargement (*engorgement*), by others to granulations, ulcerations, etc. etc. In neuralgia of the uterus, the pessary, he thought, might certainly be serviceable. In short, the intra-uterine pessaries ought not to be entirely rejected, but their use should be restricted to cases without inflammation, acute or chronic, the instrument should be employed cautiously, and not be allowed to remain permanently; and, above all, the treatment should be controlled according to rational indications; the practitioner should know *what he does and why he does it*.

M. HUGUIER, who had not signed the report in consequence of differing from M. Depaul in some of his conclusions, made a full and able statement of his views. With regard to the intra-uterine pessaries, he thought that they ought not to be entirely rejected; but that their use should be confined to those exceptional cases of displacement giving rise to serious symptoms, which resist other means of treatment; that their application should be made with great caution and carefully watched. He especially commends their efficacy as emmenagogues, when other means have failed. On the pathological question, he explained the circumstances under which uterine displacements occasion no symptoms; and after distinguishing the local and sympathetic affections which are occasioned by inflammations, etc. of the uterus, he defines those symptoms which properly belong to and are occasioned by uterine displacement, particularly disorder of the menstruation, and distressing affections of the bladder and rectum (frequent micturition, retention, obstruction of the rectum, etc.). He considers that deviations, especially ante and retro-flexion, may occasion sterility; and maintains that the displacements are undoubtedly curable. In conclusion, M. Huguier relates his experience of the general and local treatment of the displacements by pessaries, etc., and describes the methods by which he has succeeded in replacing the uterus when retro-flexed by the insertion of pledgets (*mèche*) of lint in the rectum.

M. HERVEZ DE CHEGOIN was of opinion that the displacements may occasion most distressing symptoms, which are only to be relieved by replacing the organ. But this result ought to be accomplished by vaginal pessaries external to the uterine cavity. The intra-uterine pessaries had produced so many serious accidents, that he was surprised any one could follow a method attended with such risks. He considered that they ought to be altogether dismissed from practice.

M. P. DUBOIS denied that the vaginal pessaries recommended by M. Chégoïn,

could ever effectually replace a retroverted uterus; consequently, he considered the intra-uterine instruments to be perfectly rational, and indeed, the only means capable of effecting the end in view. The only question was, are they without danger? It is certain that their introduction frequently gives rise to severe, sometimes even fatal consequences; sometimes the instruments cannot be tolerated; while, in some cases, they are quite innocuous, and occasionally their use is attended with immediate, or even permanent, relief. M. Dubois, however, maintains that in no instance do they ever attain their proposed object; the uterus is never replaced permanently in position by their use. This assertion he supports both on theoretical grounds, and by the results of his examination of cases treated by himself, by Valleix, and by Simpson. Although, however, the displacement is never removed by this method, the uterine symptoms may be alleviated or even cured by it. These results are owing to the pessary relieving the weight of the uterus, and the tension of its ligaments; to the alteration in the morbid action by contact when the uterus is inflamed, and to the alteration of its morbid sensibility in cases of neuralgia. In the latter class of cases the method is of particular service, and often relieves all the symptoms, even where no deviation had existed. Many of M. Valleix's cures occurred in cases of neuralgia. Consequently, the intra-uterine pessaries ought to be retained in practice, not as directors of the position of the uterus, but as modifying agents which may be placed in competition with other efficacious means already known as such, cauterizations, douches, etc. And in all cases, the general treatment should be attended to as well as the local, a principal condition to which M. Valleix owes much of his success.

M. CAZEAUX expressed his opinions at great length. He objected to the report for stating the unfortunate and fatal cases, and omitting the successful ones. He admitted that by removing the displacements, both the symptoms which it occasions, and even the concomitant lesions, granulations, etc. may be cured. In opposition to M. Dubois, he maintained the efficacy of the (M. Chégoïn's) vaginal pessaries in retroversion; and he considered inflexion to be a congenital condition in the child, adopting the opinion of M. Boulard, Follin, and Verneuil. He is opposed to the intra-uterine pessary, as mostly useless and often dangerous, but he considered that the uterine sound, with proper precautions, may be rendered safe, and is of real service.

M. GIBERT maintained that too much importance had been attached to uterine diseases altogether, and great illusions existed in regard to them; that they often depended on constitutional states; and he mentioned instances of several diseases of the uterus, where erroneous and overstrained views of their pathological consequence had led to methods of treatment as severe and dangerous as they were useless and uncalled for.

M. VELPEAU, in one of the most important addresses delivered in the discussion, entered at considerable length into the details of the question, and opposed, in decided terms, the conclusions of M. Depaul's report. He passed a high eulogium on M. Valleix, and attached great importance to the results of his practice. He maintained that practitioners have not exaggerated the effects of uterine displacements, nor have they attributed to them those affections which resulted from other pathological conditions. Wherever disease of the uterus, enlargement or engorgement (if not mistaken for antelexion, as has often happened), or ulceration, or leucorrhœa occurred, no practitioners could overlook these conditions or confound them with the effects of simple uterine displacement. And, although there may be cases of uterine displacement, without urgent symptoms, just as there is hardly any important organic disease the symptoms of which may not be latent, still no one could doubt the serious affections which may be caused solely by uterine displacements, and may be removed by replacing the uterus. M. Velpeau was convinced that, although these deviations yielded, in general, a favourable prognosis, yet that they produced, especially in nervous women, affections of real danger. M. Velpeau next examined the means of remedying these affections, enumerated by M. Depaul and others; and, while recognizing their usefulness in certain cases, he was quite persuaded of their radical inefficiency. In consequence of this conviction, he had himself long ago attempted the use of intra-uterine pessaries,

but had desisted after the trial of a few months, being deterred more by theoretical considerations than by the results of practice. He now thought that the dangers of these pessaries had been exaggerated, and might be avoided by more careful and experienced manipulation. The intra-uterine pessaries had not indeed solved the difficulties in the radical treatment of uterine displacements, but they had already rendered great services, and would with further experience render more; they ought not to be rejected, but their employment ought to be regulated judiciously; and he trusted to the experience of M. Valleix to disclose the advantages and the disadvantages which belonged to this method of treatment. M. Velpeau was of opinion that the conclusions of the report ought to be suppressed, and that the Academy could only vote in reference to the cases brought forward by MM. Broca and Cruveilhier.

M. DEPAUL explained that the conclusions already given were merely the summing up of his report, and that he would, after the discussion was finished, submit to the Academy a series of conclusions embodying the unanimous opinion of the committee.

M. AMUSSAT said that the idea of introducing bougies (*tiges*) within the cavity of the uterus, first suggested itself to him in 1826, on seeing M. Recamier use the uterine sound for diagnostic purposes. Among the first cases in which he used the pessary was that of a woman with anteversion; he introduced a pessary with an ivory stem into the uterus, and allowed the patient to go about with the instrument *in situ*. Inflammation supervened, and the patient died. Deeply grieved in consequence of this mishap, he tried to remedy all displacements by extra-uterine instruments. He employed for this purpose conical pessaries, which resembled ivory cupping-glasses; but these were found productive of so much pain, that he had to abandon their use. For thirty years he had used both intra and extra-uterine instruments. (Here M. Amussat exhibited to the members of the Academy his various instruments—some made of ivory and steel, and others of india-rubber, elastic gum, etc.) He was discouraged, however, by the general results of instrumental treatment. He had heard of many spontaneous cures of uterine displacements, resulting from local inflammations and ulcerations having produced adhesions between the uterus and vagina, and this led him to imitate nature, and to attempt their cure by gently cauterizing the posterior part of the cervix, that by the adhesions formed during the cicatrization of the sore, the displacements might be radically and permanently cured. The successful results of this mode of treatment had far surpassed his anticipations. Relaxation of the vagina and procidentia of the uterus often rendered the application of the caustic somewhat difficult; but by putting the fingers into the rectum, and simultaneously using the sound, the position of the uterus could be sufficiently maintained during its application, and by a pessary thereafter. He related several successful cases illustrative of this mode of treatment in cases of retroversion. He had not found cauterization of the anterior lip of the cervix to be beneficial in anteversion. In such cases he used a cup-shaped pessary (*en gobelet*), to support the uterus. In conclusion, he believed the indiscriminate use of pessaries for the cure of displacements to be prejudicial; he considered the employment of intra-uterine instruments as dangerous in some instances, and as productive of merely temporary benefit in the great majority of cases; but he characterized his own method of treatment as free from all danger, and as permanent in its results—except after pregnancy.

M. MALGAIGNE again spoke at great length. He agreed with M. Hervez de Chégoïn, in considering that uterine displacements occasioned great pain, and also engorgement, and that these disappeared after the replacement of the organ. But he affirmed that it was impossible to recognize engorgements of the uterus apart from, and independent of, displacements. He commented, with some severity, on the statements made by Gibert, in which that gentleman had underrated the importance of uterine affections, and attributed their symptoms to general pathological conditions of the system. Referring to what had been said by M. Dubois, he alleged that neuralgia of the uterus did not appear to be a very common complaint, for although he *had* seen cases of it, they were very few in number; but he bowed deferentially to the experience of Dubois. He

maintained that the pain occasioned by pure displacements of the uterus was removable by mechanical means; and that the recumbent posture caused its cessation. This circumstance he regarded as pathognomonic of the affection. He looked upon the speculum as of little use in the diagnosis of displacements, as he had known such cases when examined by means of it, diagnosed as ulcerations of the cervix, and treated needlessly with cauterants. M. Huguier had alleged that displacements, when trifling in amount, occasioned no pain, but herein he was in error. The suffering occasioned by displacements was by no means regulated by their extent, but solely by the special susceptibility of the patients in whom they occurred. Great prolapsus might occasion no pain, while trifling deviations of position might be quite intolerable. M. Cazeaux had said: "Congenital inflexions are seldom the cause of pain, while those occurring accidentally are always the source of suffering, which is sometimes very acute." He asked M. Cazeaux for some proof of the existence of congenital inflexions, and by what means he distinguished them from the others. He could conceive of no way except saying to the patient: "Madam, have you had this inflexion since your birth?" After passing a high compliment on M. Velpeau, he made some remarks on certain portions of his address. Velpeau had stated his belief that the displacements occasioned pain in the round and broad ligaments, which he said were filled with nervous filaments derived from the great sympathetic, and continuous with the mesenteric nerves. This doctrine Malgaigne condemned as unphysiological. He did not agree with Velpeau in thinking that "no experienced practitioners could overlook these conditions." The present discussion was the best proof of the possibility of such mistakes. As to the hypogastric belt, which had been originated and greatly used by Velpeau, he affirmed that it could always be borne by patients who had simply uncomplicated displacements; and that where it was not borne well at first, it seldom was so even after usage for some time. *Can uterine displacements be replaced?* Undoubtedly, for he had seen three cases of retroflexion cured spontaneously. There was no difficulty in the replacement of so mobile an organ; and in ordinary cases, where there were no adhesions, the instrument when introduced into it, replaced it instantaneously, and without any effort. He could not see the advantage to be gained by allowing the instrument to remain (as proposed by Simpson, Valleix, and others) within the uterus, for days, weeks, or months. What was the purpose of this? To give tonicity to the ligaments? They were not placed in their normal direction. If it was in order to allow the intestines (which were normally interposed between the uterus and the rectum, and keep it *in situ*) time to slip behind the organ again, it was unnecessary, as this was the work of a moment. He was of opinion that the instrument for replacing the uterus should not be allowed to remain within it more than a few minutes at the utmost. *When the uterus is replaced, can we maintain it permanently so?* That was much less probable, for the organ having once been displaced, was very prone to become so again. He believed that some complete recoveries had been effected, but that in no case was a cure certain. Seeing, then, that a cure was so very uncertain, and palliation alone could be hoped for, he considered it was neither prudent nor reasonable that we should subject our patients to the risk of danger and death. He concluded thus: "I have only one word more to add, and it is this: If, unfortunately, I had to treat my wife or my daughter for a uterine displacement, I should not choose the more dangerous of two palliative measures, and most assuredly, I should not subject them to the use of the intra-uterine instrument."

M. Ricord desired to make a few remarks on the treatment of certain hyperæsthetic conditions of the cervix and vagina, which were sometimes consequences or complications of various displacements of the uterus, with or without some other morbid condition. There were many cases of displacement where the pain was alleviated or removed by lying in a certain position. He considered that much of M. Valleix's success was due to his displacing and isolating the uterus from its painful position, without making pressure (*sans prendre un point d'appui*) on the sensitive hyperæsthetic parts. For a serious case of this nature, in which all the resources of therapeutics had failed in the ablest hands, and in which *considerable benefit* had been derived from the use of M. Valleix's

instrument—he had invented a new instrument, whereby the uterus was isolated, supported, and maintained in a suitable position, without the instrument entering the cavity of the womb, or touching the vagina. Its use was quite painless and free from bad consequences in cases where the cervix could be touched by an instrument or by the finger of the accoucheur. This instrument consisted of a forceps, which was introduced by the aid of a speculum, and the points of which seized the cervix. By means of a ring, moved by a screw, the uterus could be raised to any desired height; and the whole apparatus was fixed by means of the plate (*plastron*) of M. Valleix's instrument. In conclusion, he considered, with MM. Paul Dubois and Velpeau, as well as with M. Huguier and with M. Malgaigne in his first speech, that the mode of treatment practised by Simpson and Valleix was productive of great benefit, when applied to cases requiring it, and that consequently it should not be rejected from practice.

M. ROBERT said he rose with some hesitation to address the Academy, but having been a member of the commission for whom M. Depaul acted as reporter, he considered it right to state wherein he differed in opinion from M. Depaul. He did not, with M. Depaul, consider that the influence exercised by uterine displacement on the health of females, had been at all exaggerated. In his opinion, there existed two classes of deviations—the one comprising such as were primitive, and resulted from accidental and occasional causes; the other, such as were secondary, occasioned by different lesions of the uterus, more especially of the body of the organ. The former class of cases were observed to occur thus: "A woman in good health makes a sudden or violent effort, or falls on the breech; she feels at the time a crack and a painful sensation in the lower part of the abdomen. From this time she suffers more or less pain; menstruation becomes deranged, and is either too scanty or too profuse. She consults us, and we find retroversion; the uterus is painful, tumefied, etc." He observed that such cases should not be neglected, as they did not always remain uncomplicated, and functional disorders, as well as organic lesions, were liable to result from them. The latter class of cases followed chronic metritis, uterine catarrh, and inflammation of the cellular tissue surrounding the uterus. They occurred after protracted labours, and especially after miscarriages; and were observed in females of lymphatic temperament, in whom uterine congestion occurred without signs of phlegmasia. The fundus of the uterus was generally at fault in these cases; and was often so voluminous and heavy that it swayed backwards and forwards. In retroversion, suffering resulted from the interrupted circulation in the uterus, from the obstruction offered to the passage of feces, and from the pressure of the organ on the sacral nerves. He expressed astonishment at what had been said by Depaul and Velpeau as to the ease with which displacements could be diagnosed. He had studied diseases of the uterus carefully during the twenty years which he had been surgeon to the Hôpital de Lourcine, and he still often found great difficulty in diagnosing displacements. He considered the pathognomonic sign of displacements adduced by Velpeau and Malgaigne; viz: freedom from pain in the recumbent posture and return of uneasiness on rising—applicable only to cases of anteversion complicated with procidentia (*abaissement*), and not to retroversion.

He had for a long time been in the habit of introducing dilating bougies into the cavity of the uterus, and never saw any bad effects; and he had also tried to replace the organ by means of an instrument like that shown by M. Velpeau, but without success. He had also employed Simpson's pessary. He first used it in a case of retroversion which had resisted all other treatment. No evil result occurred during its use, but, on the contrary, the catamenia, which had been absent more than three months, returned in normal quantity on the fourth day after its application. It was worn for a period of six weeks, and during this time, a second menstrual epoch occurred, without any pain or inconvenience. At the end of the six weeks it was withdrawn, and he then found that the uterus had regained its normal position. The retroversion returned, however, and the pessary was again introduced. The patient left the hospital, and went about in Paris for ten or twelve days without experiencing any pain; but feeling it uneasy after that time she applied for its removal. Since then he had never seen her. He used Simpson's instrument in a second

case of retroversion, but as it induced a continual discharge of blood, he was obliged to withdraw it on the tenth day. In a third case of retroversion, occurring in a woman of sanguine and irritable constitution, he used this pessary. He had to withdraw it, however, at the end of three hours, inasmuch as it caused severe uterine pains, accompanied by vomiting. He tried it again three times, at intervals of several days, but without success. He had therefore abandoned the use of the instrument. Although injurious results had followed the use of this instrument, he admitted that it was proved by incontestable facts that in very many cases, where all other measures had failed, this mode of treatment had been productive of complete and lasting success. He thought that it would be premature to exclude this instrument from practice, but he considered it very desirable that its mode of action was better understood, and above all, that we knew accurately all its indications and counter-indications. This knowledge would render its employment more efficacious, and would diminish the number of accidents resulting from its use.

M. DEPAUL then replied at great length, and maintained that his report had been impartially prepared. M. Huguier had said that intra-uterine pessaries should be used in "those exceptional cases of displacement, giving rise to serious symptoms, which resist other means of treatment." If displacements occasioning severe symptoms were rare, how much more so must be those cases which resisted treatment? He could fearlessly affirm that such cases were hardly to be met with during the course of a long career. As regarded the use of pessaries for emmenagogue purposes, he could say that cases of amenorrhœa affecting the life and health of women were very rare, and that those which resisted ordinary medical treatment were still more exceptional. He believed that the sanguineous discharge provoked by the introduction of these instruments was *not* the catamenial fluid, but a true uterine hemorrhage, which had been mistaken for menstruation. As to the influence of displacements upon the health, he believed that statistics, collected from a number of rigorously observed cases, were necessary to set the question at rest. He still remained convinced that there was great exaggeration in the opinions of the partisans of mechanical replacement; and he thought that he had proved this from their own observations. He had never denied that displacements or flexions might occasion sterility, and he believed this to be due to an alteration both of structure and of form. Were uterine displacements incurable? No one had affirmed this absolutely. There were a great many displacements, especially congenital inflexions, which resisted all treatment, but there were others which were easily removed. The first question he always asked himself when called to treat such cases, was: "Is the tissue of the organ sound?" If not, he tried to remedy the disease before the displacement. He did not mean absolutely to assert that uterine displacements were of no consequence, or that they caused no accidents. He had seen enough of these diseases to know that it was not so. He agreed with M. Hervez de Chégoin that many displacements resulted from diseases of the uterus, but he did not consider, with him, that cases of uncomplicated displacement, producing distressing symptoms, were at all common; he believed them to be very rare, but he did not deny their existence. It was far more usual to find displacements—flexions and versions—existing to a considerable extent, without in the least affecting the health, or leading the women in whom they occurred to suspect their existence. As regarded the introduction of intra-uterine pessaries, he might mention that M. Michon had not only abandoned their use, but had expressed scruples about introducing foreign bodies even into the cervix, inasmuch as he had seen peri-uterine inflammation and the formation of abscesses follow the introduction of a hollow sound. He agreed with the principles of treatment laid down by M. de Chégoin, and considered that mechanical treatment should never be resorted to until all other pathological conditions, existing simultaneously, had been removed; that we should not confine our attention *solely* to the local affection, but by all suitable means endeavour to remedy the general health, and those derangements of innervation, nutrition, and circulation which were so common in such cases. He believed further that, in certain cases, it might be advantageous to associate with the local treatment of some uterine affections, the use of me-

chanical means, which by fixing (*immobilisant*) the organ without irritating it, might aid in removing chronic inflammations, of a partial or general character. He protested against the injustice of M. Cazeaux's accusation of having quoted only the unfortunate, and suppressed all the successful cases; he had drawn his data from all the cases he was cognizant of; and he knew of no really successful ones. He appealed to the statistics of M. Gosselin which he had quoted, for the truth of his assertion, that many uterine affections—even the most serious—might exist without giving rise to any uneasy symptoms. Displacement was a simple unimportant deformity. M. Cazeaux had alluded to catarrhs, granulations, ulcerations, neuralgias, etc. coexisting with displacements. In such cases these lesions should be attacked before the displacements; and in sixty cases he had treated on this principle there were only three in which any bad symptoms remained after two or three months. He advanced a series of arguments and statistics, at great length, to prove the fallacy of the assertion—made originally by MM. Boulard and Verneuil, and quoted in the speech of M. Cazeaux—that anteversion was the normal condition of the uterus in children and women who had never borne children. He had shown by his researches that intestinal expansion, in the dead body, might raise up the organ, and so deceive observers. But he admitted that there was a *predisposition to anteversion* at all periods of life. He regretted that M. Cazeaux had not stated, for the enlightenment of MM. Huguier, Valleix, Nonat, and others, the “proper precautions” which rendered uterine catheterism safe. He did not deny the usefulness of the uterine sound (*hystéromètre*); he considered that it was wrong to make the use of it general, and thought its employment should be confined to the *few* cases in which it was indispensable. With considerable asperity, Depaul next attacked the assertion which had been made by Cazeaux, that there was no such thing as inflammation of the tissue-proper of the uterus; and he quoted cases and authorities to disprove it. He alluded to the statement made by M. Dubois, that the uterus was never permanently replaced by the use of instruments, and asked if, in the event of this statement being true, it did not militate against the use of intra-uterine pessaries. After the cases cited by M. Dubois and himself, he thought there could be no doubt of the fact that these instruments were useless. (*“Les redresseurs ne redressent pas.”*) He believed that a modified form of the pessary might perhaps, as Dubois said, relieve uterine neuralgia; but he did not hesitate to express his opinion, that the evil results and fatal cases caused by the intra-uterine pessary ought to exclude it from use in the treatment of all serious uterine phlegmasiæ. He next alluded to M. Malgaigne's speech; and considered as very inconclusive and unsatisfactory the test he gave for the diagnosis of displacements, viz: cessation of pain on assuming the horizontal posture. He also objected to Malgaigne's detestation of the speculum, and of vaginal examination in such cases. He had accepted an invitation from M. Malgaigne, on the 2d July, to go to the Hôpital St. Louis, to see two cases there which had been cured by M.'s hypogastric belt. They were said by Malgaigne to be cases of simple displacement—the one a case of anteversion, the other of retroversion. On examining the patients manually and by means of the speculum, he found considerable ulceration of the cervix to be present in both cases; in the first case there existed, in addition, slight anteversion and considerable prolapsus, in the other there was no displacement whatever. He agreed with all that had been said by his learned master, M. Velpeau, in praise of the high scientific character and general probity of M. Valleix; but he dissented from the opinions expressed by Velpeau as to his practice. If Valleix had had cases proving the successful use of intra-uterine instruments, he was astonished that he had not published them. Velpeau had attributed most of the evil results of pessaries to errors of practice and diagnosis, and to want of expertness and ease in manipulation. He could not see wherein lay the difficulty of introducing a bougie into the uterine cavity, and could not imagine practitioners like MM. Valleix, Nélaton, Huguier, Aran, etc. being deficient in proper precautions or expertness. What always astonished him was that the uterus should be so tolerant of instrumental interference, when it was recollected how easily it was irritated by the presence of a small foreign body, and how the presence of a simple clot

or polypus within its cavity induced violent contractions of it, which only ceased when the offending substance was expelled. There was a great difference between these foreign bodies and an intra-uterine pessary formed of solid, inflexible material. It was said that the end of the stalk should not touch the fundus uteri; but it was forgotten that this organ had, properly speaking, no cavity; and, however short might be the instrument introduced, it acted on the organ by all the parts of its surface, and especially by its point, against which the uterus was continually being impinged, either by its own inherent force of contractility, or by the impulsions given to the organ by muscular movements and the pressure of the intestines. M. Velpeau had concluded his speech by expressing a hope that M. Valleix would decide the question as to whether these instruments should or should not be used. M. Valleix had already done so; but his opinions were dangerous, and therefore he (M. Depaul) could not accept them. He concluded by laying before the Academy the following conclusions made in the name of the commission, and carried by a majority, with the exception of the third, in which M. Depaul said he was at variance with his colleagues:—

1. *The cases reported to the Academy by MM. Broca and Cruveilhier, along with numerous others on record, prove that the use of the intra-uterine pessary may often occasion serious accidents, and sometimes even death.*

2. *In the rare cases in which this instrument has appeared productive of advantageous results, it is not proved that it had always acted by replacing the uterus.*

3. *In some exceptional cases where uterine displacements (déviation) have occasioned serious functional derangements, and where all known therapeutic measures have been tried in vain, the application of the intra-uterine pessary may be tried as a last resource.*

4. *The commission has the honour to propose a vote of thanks to M. Broca, for his very interesting communication which has originated the late debates; and also to M. Cruveilhier.*

The President put the first conclusion to the vote.

M. VELPEAU.—This conclusion appears to me to have been adopted; but it is too severe. Instead of saying “the use of the intra-uterine pessary may often,” I would substitute the words, “may sometimes occasion,” etc.

The PRESIDENT.—It is proposed to suppress the adverbs altogether. It should be read: “The use of the pessary *may* occasion,” etc.

The President then put the first conclusion to the vote, with the word “often” suppressed. It was adopted in this modified form.

The second conclusion was carried unanimously, without any modification.

The third conclusion was then read, and M. Gibert demanded its suppression.

M. VELPEAU.—I insist on its being allowed to remain.

M. CAZEAUX.—I am convinced that the use of the intra-uterine pessary is always useless, and sometimes dangerous. I cannot adopt this conclusion.

The PRESIDENT.—It contains the words: “as a last resource.” For this purpose we may try anything (*laughter*).

Several members proposed the suppression of the conclusion, and the President proposed to put it to the vote.

M. JOBERT requested the suppression of the words “as a last resource” (*comme dernière ressource*).

M. Velpeau supported this amendment.

The conclusion thus modified was put to the vote, and was rejected.—*Monthly Journ. Med. Sciences*, Aug. and Sept. 1854.

FOREIGN CORRESPONDENCE.

Parisian Medical News. Communicated in a letter to the editor, from E. WARREN, M. D.—M. Bernard has been making a number of experiments in regard to the secretion of sugar by the liver. In a recent lecture, he took a dog which had eaten nothing for four days, and examined the blood of the vena porta and sub-hepatic veins. In the first he found no sugar at all, but in the other he discovered that substance in considerable quantity. From these facts, he drew the necessary inference that the faculty which the liver possesses of forming sugar exists when no aliment has been taken into the system. He affirmed, in this connection, that this creative power of the liver was not at all destroyed in animals who had fasted even for eighteen days.

He took another dog, the pneumogastric nerve of which had been cut twenty-four hours before, and, after making an examination of his blood, demonstrated that it contained no sugar at all. He found none of that material in the hepatic veins, and scarcely a trace of it in the liver itself. He said that if the dog were examined at the expiration of forty-eight hours not a particle of sugar could be detected in that organ; from which he concluded that by arresting the action of the nerves which preside over the functions of the liver, its power of forming sugar was entirely destroyed. He then laid down the general law that "all the secreting organs are directly under the influence of the nerves, and that their secreting powers may be increased or diminished by exciting or depressing the nervous system."

In illustration of this proposition, he proceeded to show that he could at pleasure either increase the amount of sugar secreted, lessen or arrest this peculiar function of the liver, or force that organ to produce a different substance, which might subsequently be transformed into sugar.

1. As a means of demonstrating his ability to increase the amount of sugar, he took a rabbit, and punctured its medulla oblongata, at a point situated between the origins of the acoustic and pneumogastric nerves. He then affirmed that in an hour the rabbit would be diabetic, in consequence of the irritation of the nerves thus produced. At the end of that time, he made an examination of the urine of the animal, in the presence of the whole class, and found that sugar presented itself in considerable quantity in that secretion. He accounted for the existence of sugar in the urine by the fact that the liver is the great reservoir of sugar in the economy, and that when the quantity of that substance is increased in this organ, the blood becomes saturated with it, and the urinary secretion is impregnated as a necessary consequence. He said that he had made many experiments for the purpose of ascertaining whether the kidneys themselves are capable of secreting sugar, and to settle the question of its transmission outwards by other passages. The result of his researches was a conviction that, like everything else excreted from the economy, it manifested a certain preference in its selection of organs of elimination and channels of escape. For his experiments in this direction, he had employed: 1, different kinds of sugar; 2, iodide of potassium; and, 3, prussiate of potash, as the substances most easily recognized in liquids. When these materials are taken into the system, all correct observation proves that they are found in certain secretions and not in others.

Sugar finds its way out but by two passages: 1. By the kidneys; 2. By the gastric juice. Never mind how great the quantity of sugar injected in

the veins, it does not make its exit from the body by saliva, the pancreatic juice, the tears, or the bile, but always selects the urine and gastric juice as the vehicles of its passage. M. Bernard had often examined the saliva of persons affected with diabetes at La Charité, and had never been able to find in it a trace of sugar. He accounted for the "sweet taste," in the mouths of such persons, by saying that the sugar which circulated in the blood came in contact with the gustatory nerves, and produced the sensation. As an evidence of this, he stated that Magendie had found that, when bitter substances had been injected into the veins, a disagreeable taste was immediately produced in the mouth. He also explained satisfactorily why certain observers had found sugar in the expectorations of persons affected simultaneously with diabetic and pulmonary disease. He said that the sputa of such persons differed materially from ordinary saliva; the one being the mucus produced in the bronchial tubes; the other, the secretion from the salivary glands of the mouth. The first, when analyzed, sometimes gave a slight saccharine reaction; the second was always entirely free from the slightest trace of sugar. As a means of showing that substances introduced into the economy are not eliminated indiscriminately by all the secretions, he performed the following experiment:—

He took a dog, into whose parotid gland he had previously introduced a small metallic tube, and injected his jugular vein with a liquid composed of a mixture of sugar, iodide of potassium, and prussiate of potash. After waiting a short time, he procured a quantity of saliva and urine and subjected them to certain chemical tests. The iodide of potash alone presented itself in the salivary secretion, whilst all three of the substances were found in the urine.

M. Bernard believes that the presence of sugar in the gastric juice had deceived and misled many observers. The liver, and not the stomach, is the great sugar generator. The fact that wine in the stomach of diabetic persons becomes sweet after the lapse of a certain time, does not prove to the contrary in the least degree, as has been supposed by Mr. McGregor and others. The gastric juice, which is impregnated with sugar, mixes with the wine, and imparts a saccharine taste to it. The gastric juice also allows the passage of the prussiate of potash, as can easily be established in the following manner: Let some aliment containing iron be passed into the stomach, and a preparation of prussiate of *potash* injected into the veins. Then an examination of the contents of the stomach will show that the prussiate of *iron* has been deposited among them.

These experiments, with their explanations, and the deductions from them, constituted the substance matter of one of the most interesting lectures delivered by M. Bernard since the opening of his present course. He is not a fine speaker, but he is undoubtedly the most successful experimenter, and the first practical physiologist in the world. His rooms are always crowded with students, both French and foreign, and he never fails to interest and instruct all who give him their attention.

M. Bourguignon, who has distinguished himself by many observations and investigations in regard to the subject of entomology, read an interesting paper before the Academy, at its last sitting, upon the itch of different animals. It had been generally believed that in each animal this disease was peculiar, and transmissible only to one of the same species. The acarus of man had been deposited upon dogs, cats, rabbits, birds, &c., without reproducing itself, or communicating the itch to those animals. And, on the other hand, the acari of horses, sheep, &c., when placed under the skin of certain medical students, and surrounded by all the circumstances most favourable to their

germination, had died almost instantaneously. M. Bourguignon discovered, by means of the microscope, a remarkable resemblance between the acari of man and those of the lion, and immediately proceeded to test the susceptibility of the one to the disease of the other. According to his account, the experiment was perfectly satisfactory, and it is now an established fact that an analogy exists in this particular between the king of the forest and the lord of creation. Thus has a most important service been rendered to pathology in general, and dermatology in particular, by the much abused and *fallacious* microscope.

A new remedy has recently been discovered for mercurial stomatitis, which is likely to prove a most valuable therapeutical agent. It is well known that many experiments have been made for the purpose of ascertaining what chemical changes can be effected in mercury taken into the system, so as to render it harmless in its effects, and to make its elimination expeditious and easy. For the purpose of disengaging it from its combinations with albumen, restoring its natural solubility, and facilitating its exit from the economy, many different substances have been administered, as the acetate of lead, preparations of sulphur, the sulphite of soda, &c. All who have had any experience in the practice of medicine, know that these remedies are not sufficiently certain and efficient in their effects to elicit much faith in their potency when the mercurial poison has really taken possession of the system. According to the experience of Messrs. Herpin and Blanch, of Geneva, the chlorate of potash, administered internally, answers the ends which have so long occupied the attention of the profession, and fulfils all the indications present in a case of mercurial stomatitis. They report a number of cases in which this remedy effected cures with marvellous certainty and rapidity. They say that it is equally sure in its effects upon adults and children, and that in every particular it is worthy the confidence and respect of the profession. It is administered in doses of 15 to 30 grains dissolved in a solution of gum acacia and water or some other vehicle of the same description. The whole quantity is taken in the space of twenty-four hours, if the urgency of the case demands it. The dose for children is, of course, smaller, following, in fact, the general rule established for other medicines.

An animated discussion came off at the last meetings of the Academy, between M. Bousquet and M. Piorry, in regard to the specific nature of disease. The latter read a long paper before the Society in which he took strong grounds against the nomenclature now employed by medical men, for the reason that it is based upon mistaken ideas of disease itself. He attempted to demonstrate an absolute non-identity between diseases, and to show that they are separable into a certain number of distinct organic morbid conditions. In fact, he seemed to look upon man, in his relation to pathology, as nothing more than the juxtaposition of a certain number of organs and members, between which there is no distinct and necessary connection or sympathy. This called up M. Bousquet, who made an animated and able reply. He attacked the foundations of M. Piorry's argument by showing that an intimate and necessary connection existed between the different component parts of the system, and the maladies which pertain to them. He ridiculed the changes proposed in the nomenclature of medicine as absurd and unnecessary in every particular, innovations which the necessities of science did not in the least degree demand. Unfortunately, however, he went too far. He declared that he placed the causes of all maladies in the source of life itself; that he was a *vitalist* in the fullest force of that term. This doctrine is more dangerous than the other, since it condemns therapeutics to an absolute sterility, closes the door against all further research in medicine, and prevents the

advance of the profession in its most important and practical direction. To place the root of all diseases in "vital action" is to transfer it to a *terra incognita*, which mortal ken cannot penetrate or explore. A French journal in giving an account of the discussion, closes its remarks with the following expressive exclamation: "O, vitalism! how many faults have been committed in thy name!" There is certainly much truth as well as force in the expression, and it should be inscribed upon the tombs of the advocates of this peculiar doctrine, as a warning to their followers.

M. Dubois has performed the Cæsarean operation twice within the last two months, without saving the mother in either case. After the last operation, he delivered a very interesting lecture to the class, giving his reasons in full for resorting to so extreme a measure, and stating the principles upon which he had acted under such circumstances during his life. He said that the inferior strait was too small to allow the passage of the child, either living or dead; that he knew the child was alive, and that experience demonstrated the *possibility* of saving the mother after the operation.* In this state of the case, he had no alternative left but to resort to the Cæsarean section. As a general rule, he preferred cephalotripsy to the Cæsarean operation, for the reason that it was better to sacrifice a child which had formed no ties in the world than a woman who had already many complicated relations with society. He considered the question between the two operations to be one for the conscience of each individual physician, and about which no general rules could be established. He said that, among the poor, the chances for the life of a motherless child were very slight, and that as experience showed that the *probabilities* were against saving the woman after the Cæsarean section, it was his practice, particularly in hospitals, to perform cephalotripsy instead of the other operation. One could hardly have supposed that views so sensible and just, would have met with any opposition. They have, however, been warmly combated, in a recent number of the *Gazette des Hôpitaux*, by Dr. Liégard. He thinks that it is a question for science, not for conscience. He believes that the experience of the past has established a rule on the subject, which should be universal in its application. He declares it to be the duty of the physician to save the child, without taking into the account the right of either to life. He considers it better to sacrifice the mother, who has been *baptised*, than the child to whom circumstances have denied that *inestimable* right. These are his views on the subject, and I am afraid that they are entertained by but too many physicians of France. There is no telling to what extremities of folly blind superstition may drive even the most intelligent men, and when this *holy fear* of sacrificing both the *soul* and *body* if the child comes in to warp the judgment of the profession, it is to be apprehended that the learned professor may find himself in a small minority. Dr. Liégard gave some valuable statistics on the subject, which I will repeat.

Sounias, a physician of Bruges, reports that his own wife had submitted seven times to the Cæsarean section.

Simon has collected sixty instances in which the women had survived this operation. Tenon has reported seventy cases at Hôtel Dieu, which had presented themselves since those previously noticed by Caspar Baubin, at the end of the 17th century; and Michel and Weideman have reported many successful cases.

Baudelocque, out of 111 Cæsarean operations, saved 48 patients.

In my next letter, I will report the progress of this discussion, as well as give a general sketch of the progress of medicine in Paris.

PARIS, March, 1855.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Abstract of Experiments upon the Physical Influences exerted by Living, Organic and Inorganic Membranes upon Chemical Substances, in Solution, passing through them by Endosmose. By JOSEPH JONES, A. B., Student of Medicine in the University of Pennsylvania. (With a Plate.) Read before the Academy of Natural Sciences, Philadelphia, Oct. 25, 1854. Communicated by the Author.

The phenomenon of endosmose is coeval with that of life. All organized beings spring from organic cells. Each cell is a chemical laboratory; the materials for its nutrition, growth and elaboration all pass through its walls by endosmose. What is true of the organic cell, is also true of every living organism, however simple or complex, because all its acts of nutrition, secretion and growth, are carried on by organic cells. These facts show the important bearing of endosmose upon physiology and pathology, and demonstrate the absolute necessity of studying and understanding its phenomena, and developing, as far as possible, all its physical laws.

A true knowledge of these can only be gained by careful, laborious, and numerous experiments.

Experiments upon Living Animals, showing that their Membranes exert a Physical Influence capable of changing the molecular arrangement of Chemical Substances in solution, passing through them by Endosmose.—I immersed the belly and lower extremities of a large spring-frog, in a strong solution of the chloride of calcium, and allowed it to remain for an hour; then washing it carefully with water, immersed its lower extremities in a strong solution of the oxalate of ammonia. It remained in this solution for one hour, when it was again carefully washed and placed in the solution of the chloride of calcium, where it remained for a similar length of time. Finally, it was removed from this, back to the solution of the oxalate of ammonia.

During the course of the experiment, which lasted four hours, the frog gradually became weaker and more inactive, and finally died in the solution of the oxalate of ammonia in which it was last placed. The specific gravity of the solution of the oxalate of ammonia was much less than that of the chloride of calcium, consequently the object in moving the frog from one solution to another, was to obtain as large a deposit of the oxalate of lime within its bloodvessels and tissues as possible, by promoting the actions of imbibition, endosmose, and exosmose.

As a general rule, the rapidity of the action of endosmose is proportional to the difference of the densities of the exterior and interior fluids. The effects of this action, or rather of the membranes, upon the chemical substances passing through, will be manifested by the change of physical form exhibited by the precipitate of the oxalate of lime.

Immediately after the frog had been removed from the solution of the oxalate of ammonia, an incision was made below and to the side of its sternum,

into the cavity of the thorax and abdomen. The heart was found still pulsating. The bloodvessels beneath the skin, upon the surface of the stomach and intestines, in the muscular fold of the peritoneum, and between the kidneys, appeared thoroughly injected with blood, as if the animal had died of violent inflammation of all these parts.

The blood from the heart and lungs was first examined under the microscope, in which, with a magnifying power of 210 diameters, were seen innumerable minute cubical particles, floating amongst the large elliptical blood corpuscles. In the fibrous tissue between the epidermis and pectoralis major muscle, square plates, cubical crystals, and parallelograms were seen, together with minute particles, the largest of which appeared to be cubes.

The mesenteric fold of the peritoneum contained the same equilateral, equi-angular plates, and cubical crystals. In the fascia of the thigh, besides numbers of these, there appeared, also, beautiful octohedral crystals of the oxalate of lime, similar in all respects to those formed when the intestines of a raccoon (*Procyon lotor*) were filled with a solution of the chloride of calcium, and immersed in a solution of the oxalate of ammonia.

When the eggs of the frog were mashed, and their contents spread out on a glass slide and examined under the microscope, they contained multitudes of equi-angular plates, and cubical crystals of the oxalate of lime. It is probable that the exterior fluids passed through the anus and cloaca into the oviducts and ovaries, and finally by endosmose into the eggs themselves.

In the fibrous tissue of the walls of the abdomen, in addition to the cubes and octohedra, and equilateral plates, there appeared, also, delicately formed dumb-bell and ellipsoidal crystals.

When the plantar fascia of the foot was cut through, from the incision flowed a fluid resembling the liquor sanguinis mixed with a little blood, which, under a magnifying power of 210 diameters, contained beautiful octohedral and dumb-bell crystals of the oxalate of lime.

This experiment was repeated, with slight variations as to the length of the time and the density of the fluids, and in every instance, without any exception, the results were the same.

In one instance, the deposit formed within the blood and tissues of the frog assumed the form of delicate dumb-bell and cruciform crystals of the oxalate of lime. Fig. I. represents the appearance of these crystals and their relative size, when compared with the blood corpuscles. In another experiment the blood from the ventricle of the heart contained large and perfectly formed octohedra, with a few dumb-bell crystals, while the various tissues and muscles contained chiefly delicately formed dumb-bell crystals, and the aqueous humour of the eye contained octohedral, and comparatively large acicular crystals.

Fig. II. represents the crystals from the blood of the heart.

Fig. III. those found in the tissues, and between the fibres of the muscles.

Fig. IV. the acicular and octohedral crystals found in great numbers in the aqueous humour of the eye.

We see, then, by these microscopical examinations, that the oxalate of lime has assumed forms different in all respects from those of the precipitate thrown down, when solutions of the chloride of calcium and oxalate of ammonia are brought into immediate contact. The precipitate thus formed consists of minute irregular granules. Fig. V. represents this deposit. What has changed the physical form of this deposit? It has been accomplished, without doubt, by the action of the membranes upon the chemical substances in solution passing through them.

Experiments upon the Physical Influence exerted by Mucous Membranes, removed from all vitality, upon chemical substances in solution passing through them.—I filled the intestine of a raccoon (*Procyon lotor*) with a solution of the chloride of calcium, whose specific gravity was 1031, and immersed it in a solution of the oxalate of ammonia, having a specific gravity of 1007. In the course of an hour the exterior fluids became cloudy, with a white precipitate of the oxalate of lime. At the end of two days, a copious white deposit had settled to the bottom of the jar, which, under a magnifying power of 210 diameters, presented the appearance of innumerable acicular, rectangular, and irregular particles, often conglomerated together in great numbers, forming miniature representations of plants with their branches and leaves. Fig. VI. represents their appearance.

Specific gravity of exterior fluid 1005.

The interior fluid was next examined, the intestine having been punctured and its contents carefully removed.

Specific gravity of interior fluid 1003.

This marked change of its specific gravity from 1031 to 1003, shows that a free interchange of the fluids must have taken place. The slight change in the exterior fluid, of 1007 to 1005, is readily explained, when we consider the fact, that the exterior was 12, whilst the interior was only 4 fluidounces.

Within the intestines but a small deposit had taken place, in comparison with that of the exterior fluid. Under the microscope this presented a magnificent crystalline appearance, differing wholly from that of the exterior fluids, and also from that formed when solutions of the chlorides of calcium and oxalate of ammonia are brought into immediate contact. The appearance of this magnificent microscopical object is represented in Fig. VIII.

Amongst the crystals there were no less than seven well defined regularly formed varieties. We recognize the octohedral and dumb-bell crystals, as the form in which the oxalate of lime almost invariably occurs in the urine, not only of man, but also of other animals, and even in that of birds.

Does not this experiment indicate that the peculiar forms of the oxalate of lime, occurring in urine, may be the result of the physical action of the basement membrane of the tubuli uriniferi and its secretory cells?

The tissues of the intestine were next examined.

The cellular tissue was not equally injected; in some places there was scarcely any, while in others there was a very abundant deposit.

In all places the mucous membrane appeared free from any deposit of the oxalate of lime. It was found difficult, however, to decide this question by the microscope, on account of the difficulty of separating the fibrous tissue completely in which occurred a copious crystalline deposit.

This is not the only instance; out of numerous examples, we will select the following:—

When the bladder of a raccoon (*Procyon lotor*) was filled with a solution of the bichloride of mercury, and immersed in a solution of the iodide of potassium, a brilliant red crystalline deposit of the biniodide of mercury took place upon the exterior, whilst upon the interior a light yellow mass of lozenge-shaped crystals of the protiodide of mercury was precipitated. In this case also the mucous membrane appeared free from any deposit.

When the intestines of a raccoon were filled with a solution of the acetate of lead, and immersed in a solution of the bichromate of potassa, the deposit upon the exterior consisted of innumerable small irregular granules, while that upon the interior consisted of beautiful stellate crystals.

When a sheep's bladder was filled with a solution of the oxalate of ammo-

nia, and immersed in a solution of the chloride of calcium, no deposit took place in the exterior fluid, whilst a precipitate of the oxalate of lime fell in the interior fluid.

The appearance of this under a magnifying power of 210 diameters, is represented in Fig. X. Within the muscular and fibrous coats of the bladder this deposit presented the same appearance. The fact that solutions of certain chemical substances will pass through a membrane in one direction, but not in another, might be illustrated by many examples.

The stomach of a raccoon was filled with a solution of the bichromate of potassa, and immersed in a solution of the acetate of lead; a copious deposit of the chromate of lead took place in the exterior fluid, whilst none whatever occurred in the interior; it retained its natural color and appearance. The results were in all respects the same when the intestines of this animal were treated in a similar manner.

When the stomach was treated in this manner a deposit took place only upon the interior. When the œsophagus of a large rattlesnake (*Crotalus adamanteus*) was treated in a similar way, only a small deposit occurred in the exterior fluid.

I filled the stomach of a raccoon, and a portion of the intestines of a large rattlesnake with a solution of the iodide of potassium, and immersed them in a solution of the bichloride of mercury; in both cases a copious deposit occurred upon the exterior, whilst little or no precipitate fell in the interior fluid.

These facts are due to one of two causes. Either certain chemical substances in solution exert an influence upon mucous membranes, changing their minute anatomical structure, and thus destroying their power of carrying on the physical phenomena of endosmose and exosmose; or else mucous membranes possess a power of choice, as it were, dependent upon their physical constitution, allowing one fluid to pass through in one direction, but not another fluid holding a different chemical substance in solution in an opposite direction. When a portion of the small intestine of a sheep was filled with a solution of the nitrate of lime and immersed in a solution of the oxalate of ammonia, a copious precipitate of the oxalate of lime took place in the exterior fluid, which under a magnifying power of 210 diameters was found to consist of innumerable octohedral crystals of different sizes, also a few dumb-bell crystals. The appearance of this deposit is represented in Fig. VII.

In the exterior deposit the octohedra were about one hundred times more numerous than the dumb-bell crystals.

In the interior fluid a deposit of the oxalate of lime had taken place, which under the microscope presented the appearance represented in Fig. IX. The entire deposit consisted of innumerable minute and delicately formed dumb-bell crystals, with here and there an octohedral crystal. In the interior fluid, unlike the exterior, there were over a hundred dumb-bell crystals to one octohedral crystal. Minute octohedra, and delicately formed dumb-bells, were found within the meshes of the fibrous tissue. By comparing this experiment with the former ones in which solutions of the same chemical substances were used, we are forcibly taught the following laws:—

1. Mucous membranes from the same relative part of the bodies of different animals, exert different physical influences upon the same chemical substances.

2. Mucous membranes from the same animal, but from different parts of the body, exert different physical effects upon solutions of the same chemical substances.

Fig. 1.

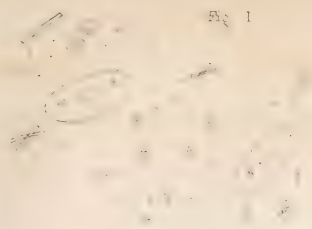


Fig. 2.

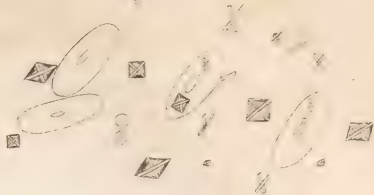


Fig. III.

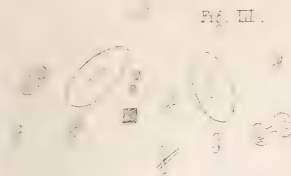


Fig. VI.

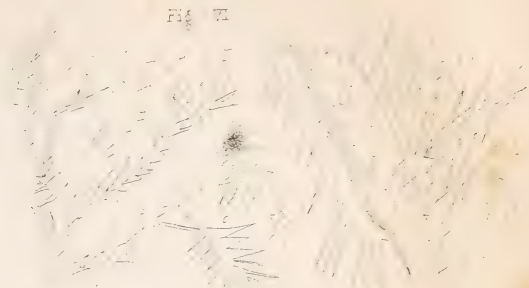


Fig. VIII.

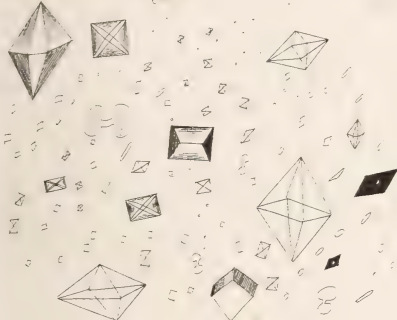


Fig. 7.



Fig. IV.

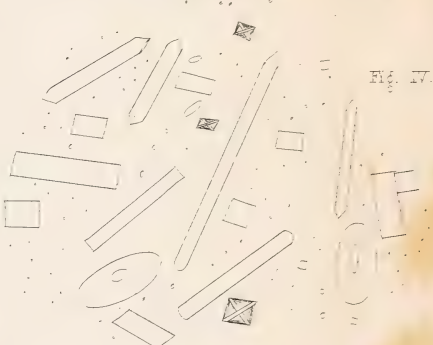


Fig. V.

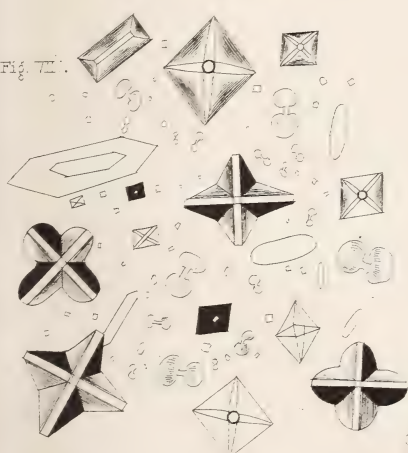


Fig. I.

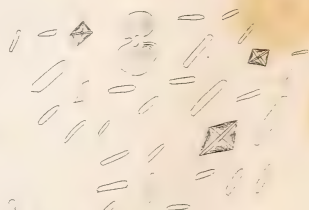
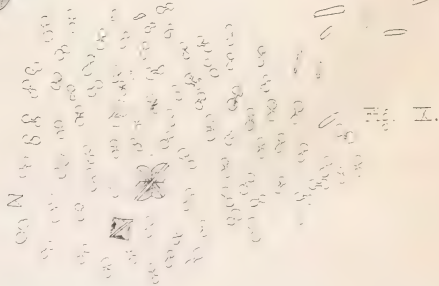


Fig. II.



Fig. III.



3. The physical influence exerted by the membrane is not the same in endosmose and exosmose; it differs with the direction of the current.

A very important question now presents itself for our consideration. May not this change of form in the precipitates be due to the presence of some animal substance or fluid, as fibrin, blood, albumen, or serum, and not to the physical action of the membranes? To determine this point a series of careful experiments were instituted with the following substances; albumen, fibrin, yolk of hen's egg, warm blood, cold blood, putrescent blood, warm serum, cold serum, putrescent serum, and urine. In no case did the presence of these substances produce a crystalline deposit of the oxalate of lime. Each experiment was performed in several different ways, and under different circumstances; sometimes the densities of the solutions of the chloride of calcium and oxalate of ammonia were varied, at others the temperature and conditions of the foreign body. But one result attended all these experiments—no crystalline deposit. In many instances we have mingled the interior and exterior fluids, in both of which crystals had been produced by the action of the membrane, and in every case the precipitate of the oxalate of lime thrown down consisted of irregular granules without any crystalline form whatever. Does not this prove conclusively that the simple presence of the different membranes did not cause the change of the physical form of the precipitate?

The next question which presented itself was, whether dry membranes exert a physical influence upon substances passing in solution through them, capable of changing their physical forms? To determine this point we performed several experiments with dry membranes, with solutions of the chloride of calcium and oxalate of ammonia, varying the relative positions and densities of the fluids in each experiment. In no instance was a regular crystalline deposit obtained. In only one experiment two or three octohedral crystals occurred in the midst of millions of irregular particles.

All our experiments, thus far, prove that dry membranes exert little or no physical influence upon chemical substances in solution passing through them.

Our next subject was to ascertain the influence of inorganic septa, during the endosmotic action. In our experiments we used thin vessels of baked clay. These were filled with a solution of the chloride of calcium and immersed in a glass jar containing a solution of the oxalate of ammonia. The relative positions and densities of these fluids were also changed. In no instance did we obtain a regular crystalline deposit. So far, then, as our experiments go, we are able to assert, that inorganic septa do not exert a physical influence upon chemical substances passing through them, capable of changing the arrangement of their molecules.

Experiment upon the Physical Influence exerted upon Chemical Substances in solution, as they pass through the cell walls of vegetables.—I immersed the cut end of a stalk of Indian corn in a solution of the chloride of calcium, and allowed it to remain for 18 hours, at the end of which time it was removed and placed in a solution of the oxalate of ammonia, in which it remained for a similar length of time. When thin slices of the corn-stalk were examined under a magnifying power of 210 diameters, a crystalline deposit of the oxalate of lime was found to have taken place within the hexagonal cells of the vegetable, which presented the appearance represented in Fig. XI. This differs widely from the deposit formed when solutions of the oxalate of ammonia and chloride of calcium are brought into immediate contact. The precipitate thus formed consisted of irregular granules.

The crystals deposited within the cells of the corn, differed widely also from those formed when the intestines of a raccoon or a sheep were filled

with solutions of the oxalate of ammonia, and immersed in solutions of the chloride of calcium.

I immersed sections of different lengths of the stem of a young and verdant benne plant, in a solution of the acetate of lead, and then transferred them to a strong solution of the proto-sulphate of iron. When thin sections were examined under the microscope, beautiful square and lozenge-shaped plates had crystallized in all the hexagonal cells.

When the broad thick leaf of an endogenous plant was placed alternately in solutions of the same chemical substances, a crystalline deposit took place within its cells, which differed in form from that within the cells of the benne plant. Different vegetables were immersed in different chemical solutions, which, when mingled, produced a deposit of irregular granules, and in every instance a regularly crystalline deposit took place within their cells.

It is unnecessary to do more than recapitulate the following results:—

1. Cell-walls, like animal membranes, exert a physical influence upon the chemical substances held in solution passing through them. This physical influence is capable of altering the arrangement of the molecules of the precipitate formed within the cells, so that the precipitate which under ordinary circumstances consists of irregular granules, under the influence of the endosmotic action assumes a regular crystalline form.

2. The cells of different vegetables, like different animal membranes, change in different manners the arrangement of the molecules of the same substance.

It may yet be demonstrated, by experiment, that cells in the same plant, having different offices, elaborating different products, exert a different physical influence upon the same chemical substance. Or, in other words, the crystalline deposit of the same substance, will vary in physical properties with different cells.

In conclusion, we would state that we are still investigating this subject, and hope to be able to substantiate, by numerous experiments, performed under every possible circumstance, all the laws and facts asserted in this brief abstract of experiments.

Case of Quadruple Birth.—By WM. RANKIN, M. D., of Shippensburg.—April 20, 1854. I was called to visit the wife of Mr. John Tarvin, farmer, who is supposed to be in labour, having completed the full term of her fourth pregnancy. On my arrival, I found a child delivered; but on examination, ascertained that there was another contained in the uterus. After waiting nearly an hour from the time the first child was born, for further uterine contractions, I ruptured a set of membranes that presented in the form of a pouch at the os uteri, when a pain came on and delivered another child. On examination over the region of the uterus, I discovered the presence of another child; and no pain coming on, after a reasonable delay, I ruptured a third set of membranes, when a powerfully expulsive contraction took place and expelled a third child. From the usual examination, I ascertained that there was yet another child to be delivered; and no pains coming on, I resorted to the plan of rupturing the membranes, when almost immediately a pain occurred and effected the delivery of a fourth child. By a little manipulation over the region of the uterus it contracted and expelled from its cavity two placentæ, to one of which three cords were attached, and to the other but one; the whole mass not larger, I think, than I have often seen after the delivery of one child. The cords were something smaller than the average size. The uterus afterwards contracted well, and there was no more hemorrhage than takes place after the majority of cases of parturition. The woman appeared very much exhausted and overcome by distracting emo-

tions in consequence of the enormous brood of children to which she had just given birth; but after encouraging and consoling her, as well as I could, by pointing out her duty in the case and assuring her that there would, undoubtedly, be ample means provided for their maintenance, she soon assumed a cheerful countenance, her general debility gradually abated, and she progressed through her confinement as favourably as most women do after giving birth to a single child.

The first and last born were rather larger than their mates, and were all apparently healthy and viable, and, according to their mother's reckoning, had come to their full time. Although I unfortunately neglected weighing them, I think they would have averaged four or five pounds.

They were all of the male sex. The hair nearly red, and the complexion florid in all of them.

The mother is a woman of slender and rather delicate form, of medium height, and generally healthy, of about thirty-five years old; and, before this quadruple birth, had had three daughters at as many births; five years have elapsed since the last. Her complexion and hair are dark. The father is a stout, athletic, healthy man, of about thirty-eight years of age; hair fair and complexion florid; of sanguine temperament.

The first born of the children, about eight weeks after birth, sickened and died after a few hours' illness, most probably from the almost unavoidable irregularity in giving it nourishment.

After the milk was fully secreted the mother had, for a month or so, plenty of nourishment for them all, after which time food was required from other sources, principally cow's milk diluted with water and sweetened with loaf sugar. The three surviving are now, 14th of January, 1855, quite healthy and thriving children.

Paste of Allium Cepa as a remedy for Epistaxis.—BENJ. RORER, M. D., of Germantown, has favoured us with a communication in which he extols the efficacy of a paste made of the *Allium cepa* in arresting atonic hemorrhage from the Schneiderian membrane.

The directions for making the paste are as follows:—

"Take the inner portions of the bulb of the *A. cepa* or common onion, cut it fine, mix it with an equal portion of flour or bread crumbs, adding a sufficient quantity of strong vinegar to make a consistent paste." This paste is applied by pressing it into the nostril with the ball of the thumb until no more can be introduced, when the plug is to be secured by a bandage. Several cases are detailed illustrative of its efficacy, one of which we shall quote.

G. F., aged 44 years, a baker, was attacked, March 6th, 1847, with epistaxis without any premonition, while attending to his heated oven, and had lost, he stated, 10 or 12 ounces of blood before Dr. Rorer saw him. Dr. R. had him placed in a cool room, washed his face and head with iced water, and injected the same into his nares, and afterwards solutions of alum and of the metallic salts. Alum and finely powdered galls were then applied by means of a quill, and lastly plugging with lint wet with astringent solutions resorted to. At the same time 20 drops of muriated tincture of iron were given every two hours, all without any avail. At this time a German named Seibert advised the onion paste, which was used with the effect of completely arresting the hemorrhage.

Blighted Fœtus at the fifth month retained and expelled with a Living Child at Full Term. By WM. M. BELT, M. D., of Independence, Mo.—I was called on the 23d day of January, 1852, to see a girl in the employ of

Hiram Young, of this place, supposed to be threatened with abortion. She stated that the waters had escaped; there was slight hemorrhage and strong uterine pains. I kept her in a recumbent position, and gave a mild cathartic. In five or six days she was able to walk about the house, and soon recovered her usual health. On the 21st day of May following, I delivered her of a healthy child. In tracking the cord for the placenta, I came in contact with a *dead fetus* of five months.

INDEPENDENCE, Mo., Jan. 1, 1855.

DOMESTIC SUMMARY.

Peritonitis in Typhoid Fever, without Intestinal Perforation.—The following very interesting case was read to the College of Physicians of Philadelphia, by Prof. G. B. Wood.

"J. B., aged seventeen years, entered the Pennsylvania Hospital on the 2d of January. According to his own account, he had been three days ill; but the symptoms, which were those of typhoid fever in the middle of its course, evinced that he was in the second week of the disease. The tongue was quite dry, and the abdomen tympanitic; and the characteristic red spots, if not present at the time of his entrance, made their appearance very soon afterwards. Under the plan of treatment usually pursued in the hospital, including the use of oil of turpentine as an alterative to the ulcerated surfaces in the intestinal mucous membrane, he gradually amended; and, on the 20th, I presented him to the class in attendance upon the clinical lectures as quite convalescent. On the 21st, however, he was suddenly seized with severe abdominal pains, tenderness on pressure, and great prostration; and it was evident that he was labouring under an attack of violent peritonitis. I had no doubt of the existence of perforation of the bowel, which I felt disposed to ascribe to an orange which he had surreptitiously eaten a few days previously, and of which, in order to conceal his violation of the rules of the ward, he had probably swallowed the seeds and rind. With this conviction of the nature of the case, I directed the use of sulphate of morphia largely, according to the well-known plan of Drs. Graves and Stokes, with perfect rest, a blister over the abdomen, and, towards the close of the case, stimulants in order to support life. The system never fully reacted, and death took place on the 25th.

"Dr. Forbes, one of the resident physicians of the hospital, opened the body, and found a small quantity of turbid liquid in the abdominal cavity, with evidences of almost universal inflammation of the peritoneum; but, on the closest and most careful inspection, could discover no perforation of the bowel; nor was there the slightest fecal odor in the liquid effused. I had afterwards an opportunity of examining the whole of the intestines, though not *in situ*. The internal surface of the ileum presented several recent and well-defined *cicatrices*, finely marking the boundary of the patches of Peyer's glands, which I exhibited to the class as the finest example I had seen of this appearance; but there was not a single remaining ulcer visible, and I could find no sign of an opening. The peritoneal coating was covered with a very thin, glutinous, and translucent exudation of coagulable lymph, but with little redness. At no one point upon the surface were there more decided marks of inflammation than elsewhere, to indicate the possible seat of an opening. I was compelled, therefore, to regard the case as one of peritonitis without perforation. No discoverable cause of the affection existed; and I am inclined to rank this with those not very uncommon cases, in which peritonitis comes on without any special known cause, at the close of long-continued and exhausting affections, perhaps in consequence of depravation of the blood.

"This case is calculated to throw great doubt upon the existence of intestinal perforation in those instances of peritonitis, occurring in the advanced

stage of typhoid fever, in which cures have been effected, of which I have been so fortunate as to witness two, in my own experience. They may have been, as in the case just related, nothing more than simple peritonitis without any opening whatever through the coats of the bowel. One important practical inference, however, may be deduced from them, viz: that the opiate treatment is the one best adapted to peritonitis occurring under these circumstances, whether with or without perforation; as several instances of recovery have taken place under that treatment, while I am not aware that one is on record effected under any other plan."—*Summary of Transactions Coll. Phys. Phil.*, Vol. II., N. S., No. VII.

A Grain of Coffee in the Air-Passages—fatal after being retained nearly three years and a half.—At the Meeting of the College of Physicians of Philadelphia, Nov. 1, 1854, Dr. WM. PEPPER related the following interesting case:—

On the 22d March, 1844, a little girl, aged eighteen months, whilst playing on the floor, was suddenly seized with a violent spasmodic cough, threatening strangulation. The mother recollected that the child had been amusing itself with an uncooked grain of coffee, and immediately suspected that it had been swallowed and was the cause of all the difficulty. The room was carefully searched, but the grain could not be found; and this but confirmed the mother's conviction. Dr. Maris saw the child an hour or two after the accident; she was then in a disturbed sleep, with hurried breathing and suffused face; in a few moments she was aroused by a return of the same agonizing cough and spasm. Various expectorants were now administered, but without relief; and the remissions and exacerbations of the cough continued to recur every few minutes.

After an interval of about six hours, I was requested to see the child; but found her so much oppressed and restless, that it was almost impossible to auscult with any degree of accuracy. The respiratory murmur on the right side was exceedingly feeble, and, in a great measure, masked by loud sonorous râles, whilst on the opposite side the murmur was unusually loud; throughout the entire chest, the percussion was perfectly natural. The history of the case, confirmed by the physical signs, left but little doubt as to the nature of the accident, and tracheotomy was accordingly proposed. This, however, was rejected by the father, under the impression that the symptoms might be owing to some other cause, and in the hope that, even if our fears were well founded, the coffee-grain might be spontaneously expelled. Upon several occasions, when the violence of the cough indicated that the grain had been driven into the larynx, the child was inverted, in the hope that gravity might assist in its expulsion; but this expedient only tended to increase the alarming symptoms, and was therefore abandoned. From this time, I did not see her until the early part of August, 1847, a period of more than three years. Dr. Maris, however, informed me that, a few weeks after the accident, the paroxysms of cough gradually became less violent and of shorter duration, whilst the intervals were much longer, lasting sometimes for several weeks, during which the child was comparatively comfortable, and gained both flesh and strength, but never entirely free from difficulty of breathing. Finally, hectic fever and purulent expectoration set in, and she became greatly emaciated. Upon examination, I now found dulness on percussion over the lower part of both lungs, but particularly on the right side, and the respiratory murmur throughout was completely masked by loose mucous râles; the prostration, however, was so great as to preclude the possibility of a more extended examination. From this time she gradually sank, and finally became comatose, and died on the 24th of the month—just two weeks after my last visit, and about three years and five months from the time of the accident.

The body was examined the following day. Upon opening the thorax, care was taken to remove both lungs, with the trachea, larynx, and œsophagus adherent, without incising these parts; gentle pressure was now made with the finger along the course of the trachea, from below upwards, until the larynx was reached, when the grain of coffee was seen to escape from the chink of the glottis. It, apparently, had undergone no important change from its long

stay in the air-passages: it had not materially increased in bulk from absorption of moisture, as might have been expected, nor was it even coated with pus, mucus, or other animal matter, and its usual colour was well preserved; upon the application of considerable force, it could, however, be slightly flexed. The mucous membrane of the larynx and trachea was thickened, minutely injected, and coated throughout with tenacious muco-puriform matter; and this condition of things extended throughout the bronchial tubes, as far as examined. At the bifurcation of right bronchus, the membrane presented a worm-eaten appearance, and was much softened, though not absolutely ulcerated. The entire right lung was hepatized, and about one-half of the left lung, at its lower part, was in a similar condition. No abscess or tubercular degeneration existed in either lung, nor were there any pleuritic adhesions.

The condition of the right bronchus, at its bifurcation, renders it highly probable that the foreign body was lodged the greater part of the time at this point. The chronic inflammation of the left bronchus and lung may have been owing to an extension of the inflammation from the right side, where it undoubtedly first commenced; or it may have been induced by the occasional lodgement of the coffee-grain in the left bronchus, after having been driven into the trachea or larynx. Judging from the size of the foreign body, compared with the caliber of the bronchus at the tender age at which the accident happened, it is highly probable that the air must at the first have been almost entirely excluded from the right lung; and this must have been still more the case when lodged in the left bronchus, owing to its comparatively small size. It has been admitted by all observers, that when the foreign body is irregular in shape it is more apt to become permanently fixed in the bronchial tubes, and at the same time to interfere less with the respiratory movement, than when it is round or oblong, and thus completely plugs up the tube, as was the case in the present instance; and this fact fully explains the great fluctuations in the symptoms.—*Quarterly Summary Trans. Coll. Phys. of Phila.*, Vol. II., N. S., No. VII.

Achillæa Millefolium in Hemorrhage.—Dr. B. H. COATES stated, at the meeting of the College of Physicians of Philadelphia, on Dec. 6, 1854, that he had used an infusion of *achillæa millefolium*, with material advantage, in several cases of hemorrhage; two from the kidneys, three (in the same subject) from the uterus, and one from the lungs. The formula was half an ounce of the herb to a pint of hot water; to digest for twenty minutes. The dose of this infusion to be taken cold is half a teacupful every two hours.—*Summary of Trans. Coll. Phys. Phil.*, Vol. II., N. S., No. VII.

Erigeron Philadelphicum in Uterine Hemorrhage.—Dr. WILSON read to the Philadelphia College of Physicians, Nov. 1, 1854, notes of some cases of uterine hemorrhage successfully treated by the essential oil of *Erigeron Philadelphicum*. Dr. Wilson, whose attention was drawn to this article by Dr. A. C. Bournonville, who had used it in some few cases with advantage, states that he has prescribed it in many cases, and very generally with decided advantage. He prescribes it in doses of five drops every two hours.—*Quarterly Summary Trans. Coll. Phys. Phil.*, Vol II., N. S., No. VII.

Ligature of the Gluteal Artery.—Dr. WARREN STONE has lately performed this operation in the Charity Hospital, on a boy, ten years of age, who had a large pulsating tumour in the gluteal region, following a wound by a narrow bladed knife, on the 28th December, 1854. The operation was performed on the 7th of February last, in the following manner: Dr. Stone made an incision across the top of the tumour, following the course of the fibres of the gluteus major muscle, eight inches in length, scooped out the coagulated blood, the sack being freely opened, and placed his finger upon the artery to control hemorrhage; a ligature was then placed around the artery, and a compress of lint gently applied to stop a slight oozing which existed from the neighbouring parts. The wound was drawn together by adhesive straps, an opiate was given,

and the patient put to bed. On the 24th February the patient was discharged, the wound being nearly healed.

Dr. Stone states that this is the third case of this lesion which has come under his care.

"The first case was brought to the Charity Hospital in 1835, considerably exhausted by the loss of blood, from a wound of this artery; it was treated by compression. The wound healed; but, about two weeks after, a small tumour was discovered in the region of the gluteal artery, which, upon auscultation, gave the usual aneurismal murmur. The tumour seemed dense, as if considerable lymph had been thrown out and organized for its protection, and pulsation was only observed when its walls were compressed. This patient remained in the Hospital about six weeks, and as the tumour did not increase, he was discharged; being warned of his condition and advised to return if the swelling should increase; but I heard no more of him. The second case was admitted into the Hospital in 1853, completely exhausted from the loss of blood. The wound was made six days previous to my visit to him, and he had suffered repeated bleedings. At the time I first saw him, he appeared perfectly bloodless, and nearly pulseless, having just had a hemorrhage, and it was thought that he could not bear the operation. Stimulants were administered, and in about an hour the pulse became steady; and as the operation offered the only possible chance for life, he was placed upon the operating table, and the artery tied. The operation was made by making an incision from near the posterior superior spine of the ischium, to the upper part of the trochanter major (which, from the distention of the parts by the coagulum beneath the muscle, was near a foot in length), the coagulum was scooped out, and the finger placed upon the artery just where it emerges from the pelvis. In the upper part of the ischiatic notch, it was found that the artery was cut very near to the finger, and it was with great difficulty that a ligature was placed upon it; this was finally done, and the patient was carefully stimulated and nourished; but, in spite of the greatest care, gangrene took place in the wound, in consequence of the great loss of blood, and he died on the fourth day after the operation.

"By most surgeons," Dr. Stone remarks, "it is considered sufficient to tie the artery on the proximal side of the wound; but Mr. Guthrie insists that it is important to tie the artery on both sides of the wound, at least when it occurs in the arteries of the extremities, as the free anastomosis, he asserts, renders hemorrhage very certain from the distal extremity. I have tied most of the large arteries for wounds, and in most cases have applied a simple ligature at a convenient place on the proximal side of the wound, and have never been troubled with hemorrhage from the distal orifice, except in one case. This was in a wound of the plantar arch, in a case that was admitted into the Hospital sixteen days after it was received. The patient, a little boy of ten years, was completely exhausted from repeated hemorrhages, and I tied the two tibial arteries, the anterior one on the dorsum of the foot, and the posterior one behind the inner malleolus. Three days after, he bled again, but not very freely, and it was thought a little pressure would command it; but the next day it recurred with more violence, and the patient was completely exhausted, and evidently could not survive any further loss of blood. The foot was swollen and painful, and it seemed impossible to secure the artery in the sole of the foot, without destroying the organ, and I determined upon tying the femoral artery, which was done without the use of chloroform as the patient lay in his bed, for he was so weak that I feared to disturb him; this secured the bleeding, and the little patient recovered perfectly.

"But notwithstanding the confidence which my experience gives me in the safety of tying the artery at a convenient place on the proximal side of the wound, I would advise, in cases of false aneurism, when there is considerable extravasation of blood, to lay the sack open freely, and tie the vessel at the wound, whenever it can be done without cutting through important parts. The impression is, that a large sac or cavity, containing blood, laid open and exposed to the air, will take on ill-conditioned inflammation. This is generally true, where the extravasation is from contusion, but there is a great dissimilarity in the cases. In the former case, the blood is isolated, and can be scooped out

clean, and the cavity heals kindly; but in the latter case, the blood mingles and clings to the contused parts, and cannot be sundered, and when exposed to the air decomposes and becomes a poison. Another fear that deters from opening the tumour is, that the artery may not be readily found, and the patient suffer from loss of blood. When the artery can be commanded by pressure on the proximal side, there can be no objection, and the bleeding mouth of the wounded vessel is readily discovered. The flesh is pushed away by the pressure of the extravasated blood, and leaves the mouth of the vessel isolated, compared with its position in a fresh open wound. The advantage of this method of operating is, that we get rid of the mass of blood, which will generally have to be discharged sooner or later by a separate opening, when the quantity is large, and we make the operation more certain, for the sac may be fed when the anastomosis is free.—*New Orleans Hospital Gazette*, March, 1855.

Case of Spontaneous Rupture of the Bladder.—Extravasation of Urine.—Operation.—Recovery. Dr. A. V. WILLIAMS, late Physician-in-chief to Ward's Island Hospital, records (*New York Medical Times*, Jan. 1855) the following extremely remarkable case.

A man, aged 32 years, of spare habit, but of great endurance, has for several years laboured under stricture of the urethra, with frequent desire to urinate from irritability of the bladder. He states that on several occasions, he has been unable to pass any water for several hours. On the 9th day of June, 1854, I was called to see him, when he stated that he had not passed any urine for two days; that this morning, when making a violent effort to do so, he felt something give way in his belly, and "felt a snap;" since which he has felt no desire to urinate, but the pain is very great over the belly. I endeavoured to pass a catheter into the bladder, but found it impossible to do so. He was put in a warm bath, a large anodyne administered, and a warm poultice applied over the abdomen.

June 10. He slept some last night, but otherwise remains in the same state, except that the abdomen was not swollen. I could not make any other *diagnosis* than that some portion of the bladder had given way when he felt the "snap;" and that the effusion of urine into the cavity of the abdomen accounted for the subsidence of the desire to make water, and also caused the other symptoms.

I requested Dr. Willard Parker to meet me, who, on examination, concurred with me as to the nature of the case, and of its desperate character.

On consultation, we agreed that the only chance for the man (and that a very small one), was to open the abdomen above the pubis, cut into the bladder, and pass a catheter, if we could, from within out through the penis, and re-establish a passage in that way. To cut into the bladder from the perineum was useless, as the effusion was above the pelvic fascia; and a puncture through the rectum was impracticable, as the bladder was empty, and could not be felt.

The case was fully stated to the patient, who requested me to "cut away."

After shaving the pubis, I made an incision about four inches long, from the pubis upward in a line with the linea alba, dividing the fibres of the pyramidalis; then very carefully divided the tendon beneath, and with a director dilated it freely. The urine flowed out abundantly from the wound. There was but little hemorrhage. The bladder was deep, and firmly contracted behind the pubis, and so altered in appearance that it could not be recognized as that organ. I pushed up the perineum with one finger, whilst Dr. Parker, with a hook drawing up the bladder, I punctured it with a bistoury; on dilating this opening with the finger, the internal surface was found corrugated and thickened. The opening into the urethra could not be felt, so that the original design of forcing a passage from within outwards, could not be done. Whilst my finger was retained in the bladder, Dr. Parker accordingly passed a grooved sound into the penis, down to the strictured part, and forced it forward until the point was felt by my finger through the thickened coats of the bladder, which were so tough I could not tear it with the nail, but cut upon the end of the sound, with a probe-pointed bistoury, carefully passed along the finger. The wound made in the abdomen was brought together by a single

suture; a catheter introduced through the false passage made into the bladder, a large anodyne given to the patient, and he placed in bed as soon as the operation was completed; he felt greatly relieved, and soon fell asleep.

11th. The patient says he feels pretty comfortable; he only complains of the general soreness of the belly. The urine flows from the wound, and also from the catheter.

12th. Had a bad night; pulse, 130; abdomen tender; urine flows as yesterday; bowels not opened; vomits a green matter; skin hot; but says "he shall get well," which opinion he stuck to throughout. Gave him opium grs. iij, with calomel grs. viij, and directed a pill of calomel gr. ij and opium $\frac{1}{2}$ gr. to be given every two, three, or four hours, according as he is restless; diet accordingly, with poultice to abdomen.

12th. Passed a more comfortable night; the vomiting has ceased; bowels freely purged; pulse, 130; abdomen tender; wound healing above the suture, whilst the urine flows from the lower part above the pubis, and through the catheter; continue the calomel and opium every four hours. The constant dribbling of the urine over the scrotum has produced excoriation, which annoys the patient more than his other troubles. This was relieved by simple cerate, and the parts protected from the urine by an oiled-silk bag.

13th. Patient attacked with violent hiccough, from which he suffers dreadfully. This symptom caused great distress, and continued without intermission for an entire week. Nothing seemed to do any good, and all remedies for it were abandoned, except a pill of opium three or four times a day; the calomel was omitted on account of his gums being sore. The symptoms of peritonitis are subsiding.

21st. The subsidence of the hiccough was a great relief. The patient began to show his sufferings. Beef-tea and milk punch were ordered, and a general supporting regimen was followed throughout the remainder of the treatment, with quinine.

A slough came out of the wound above the pubis, about the size of a pullet's egg, leaving a clean, granulating sore, which healed kindly. On the 27th day after the operation, the wound had entirely closed, and the urine passed through the penis in a fuller stream than it has done for years. The patient entirely recovered, and engaged in his labouring business as a journeyman butcher.

Dr. Cammann's Self-Adjusting Stethoscope.—Dr. GEO. P. CAMMANN, of New York, has invented a self-adjusting stethoscope, which consists of an objective end made of ebony, the extremity of which is about two inches in diameter, two tubes composed of gum elastic and metallic wire, two metallic tubes of German silver, two ivory knobs at the aural extremity, and a movable elastic spring, so arranged as to adjust it, and keep it in its proper position.

M. Landouzy, of Paris, previous to 1850, formed a stethoscope having a number of gum-elastic tubes, by means of which several persons could listen at the same time. Dr. N. B. Marsh, of Cincinnati, in 1851, patented a stethoscope with two gum-elastic tubes, and a membrane over its objective end. Dr. Cammann does not, therefore, claim any originality on account of the two branches of his instrument, but on account of other advantages which it possesses.

The bell-like expansion of the objective extremity has a convoluted excavation, gently curving outwards to present a rounded edge to the chest, in order to prevent causing pain to the patient. The bore of the instrument is two and a half lines in diameter, care being taken to have it made smooth and even. The tubes are made of German silver, with a double curve towards the aural extremities, which curves require to be constructed with great care, so that the ivory knobs may rest closely upon the external openings of the ears. When applied, it is necessary that the orifices of the knobs should point upwards. Some of the instruments are constructed with a spiral, and others with an elastic spring, as shown by the plate. Some of them are so arranged that they can be disjoined, to render them more portable.

One point, heretofore *sub judice*, is settled by this instrument, viz: that the

sound is conducted entirely through the air, and not at all through the media, as these were, for experiment's sake, changed nine or more times, without affecting in the least the intensity of the conducted sound. On making the objective end solid, all sound was lost.

The advantages claimed for the instrument of Dr. Cammann are:—

1. That being applied, it adjusts itself closely to both ears, excluding all external sound.

2. It leaves both hands of the examiner free.

3. It gives sounds pure, and greatly increased in intensity, though differing in quality from those hitherto afforded by auscultation, the pitch being lower. This intensity is produced by both ears being acted upon at once, by the ear-pieces of the instrument fitting closely into the meatus of both ears, and by the smoothness and careful construction of the bore of the stethoscope as to curves, &c., according to the law of reflected sound.

4. Sounds not heard through the instrument in common use can be detected by this.

5. Sounds which are doubtful by ordinary instruments are made perfectly certain. Even when disease is seated in the central part of the lungs, they can be detected, when the ordinary stethoscope will fail to render them recognizable. The same advantages are obtained in examining the morbid sounds of the heart.

The great increase of intensity of sound by this instrument renders it valuable to those with impaired hearing.

In the use of this stethoscope it is necessary that the chest should be uncovered, to prevent all friction between it and the clothes; otherwise the sound thus generated is conducted with such intensity as to embarrass the examiner. A short practice may be required to become familiar with it, in consequence of the increased intensity of the sounds produced by it, and the difference between them and those afforded by ordinary auscultation. Many of the recognized physical signs of thoracic disease will be so modified as to be new to the examiner, but a short experience will enable him to appreciate them, and give them their true value.

These stethoscopes are manufactured and sold by Messrs. George Tieman & Co., No. 63 Chatham street, who pay particular attention to their construction—a point very essential to an instrument of this kind.—*New York Med. Times*, Jan. 1855.

Reduction of Dislocation of the Femur by Manipulation alone.—Dr. THOMAS M. MARKOE, one of the Surgeons of the New York Hospital, has published (*New York Journal of Med.*, Jan. 1855) an account of the cases of dislocation of the femur at the hip-joint, treated by manipulation alone, during the past two years in the hospital to which he is attached, with some very interesting remarks on this method of reduction.

Our readers are aware, that Dr. Wm. W. Reid, of Rochester, N. Y., published in the Transactions of the Medical Society of the State of New York, for 1852, a paper on “dislocation of the femur on the dorsum ilii, reducible without pulleys or any other mechanical power,” in which it is the object of the writer to show that this displacement can be remedied “by flexing the leg on the thigh, carrying the thigh over the sound one, upward over the pelvis, as high as the umbilicus, and then by abducting and rotating it.”

“The fact,” Dr. Markoe observes, “that luxations of the hip-joint might be reduced by manipulations, not requiring forcible extension by pulleys or other mechanical contrivance, seems to have presented itself to the mind of several writers both of ancient and of modern times. The idea would appear sometimes to have been suggested by accident, and sometimes to have been the result of reasoning on the mechanism of the joint and its displacements. Such a suggestive accidental reduction occurred in the hands of Dr. Physick, and is related in Dorsey's Surgery. A patient was the subject “of dislocation of the femur directly backwards, and after very powerful efforts had been made to effect the reduction by extension, Prof. Physick, conjecturing that the head might be confined in a slit in the capsular ligament, discontinued the extend-

ing force entirely, and then, with no more force than that of his own hands, abducted the thigh, when the bone instantly slipped into its place."

"A similar occurrence took place in our hospital, about the time of the introduction of the use of chloroform, in a patient with dislocated hip, under the care of Dr. Jno. C. Cheesman. While the preparations for applying mechanical force were being made, Dr. C. made some preliminary movements of the joint, while the patient was fully under the influence of chloroform, and, unexpectedly, the head of the bone slipped into its proper place. Dr. Nathan Smith, Prof. of Surgery in Yale College, succeeded in reducing a luxated femur, by manipulating, with the thigh and leg used as a lever, after the usual plan had failed. It would seem from the report of this case, which, however, is made from memory by his son, Dr. Nathan R. Smith, that it was a deliberate and prearranged plan of procedure, though the considerations which led to its adoption are not mentioned in the record.

"Chelius, in his work on Surgery, gives an outline of the views of four writers, who have proposed to effect the reduction of the dislocated hip by the hands alone of the surgeon and his assistants, and without the aid of pulleys, or, indeed, of any forcible extension. Three of these gentlemen, Wattman, Kluge, and Rust, have described methods not very different from each other, which essentially consist of moderate extension with the hand, combined with movements of rotation of the axis of the limb, adduction, flexion, etc., but with the addition of force applied by a hand passed around the upper part of the limb, which is committed to an assistant, and, in the different forms of dislocation, is so applied as to lift the head of the bone from its unnatural resting-place, while the other movements made with the shaft of the limb give an opportunity to the muscles, stretched by the luxation, to draw the head back into the acetabulum. Another surgeon, referred to by Chelius, Colombat, speaks of a method which he has always employed with success, in which the patient stands at a table leaning forward with his chest upon it, and the surgeon, standing behind, bends the knee with one hand, and makes the various movements of the hip-joint, while, with the other hand on the ham, he makes gradual extension by pulling downwards. By this manœuvre, he states that he dislodges the head of the bone, and without any force accomplishes its reduction. In Casper's *Wochenschrift* for Nov., 1849, there is an account given by Dr. Fischer, of Cologne, of his mode of reducing luxations of the hip, which consists in flexing the femur on the trunk, and then making rotation of the limb, conjoined with adduction, or abduction, as the head of the bone is on one side or the other of the acetabulum. In the *N. Y. Journal of Medicine* for March, 1852, a Dr. Mayr is spoken of as having made use of this procedure of Dr. Fischer, without being aware of Dr. F.'s published statement."

Dr. Reid's observations appear to have been made independently of any knowledge of the previous opinions of others. In his paper he describes the steps by which he was led to the adoption of his mode of operation, and gives five cases of his successful application in practice.

It would thus appear, as remarked by Dr. Markoe, that the barren honour of being the first suggester of the idea of this method does not belong to Dr. Reid, but he accords to him "the higher honour of having caused the idea to assume the shape and value of a fact, and out of the bare suggestion to have, by patient, ingenious, and long-continued investigation and experiment, deduced an available, and, as we think, an exceedingly valuable addition to the resources of Practical Surgery."

Dr. Markoe gives the details of thirteen cases of dislocation of the femur, in all of which, with two exceptions, Dr. Reid's plan was successful.

"With regard to the rationale of the process," Dr. Markoe observes, "most of those who have written on this matter are in the main points agreed. The head of a dislocated femur is retained in its new position by a mechanism which does not exist in any other joint, and which is produced by the fact of the muscles not being inserted into the head, but into the trochanter, nearly three inches from the head, and that from this point of principal muscular insertion the neck goes off at a large angle from the axis of the shaft. From this, it happens that when the head of the femur is thrown out of its socket, the

trochanter no longer stands out more prominent than before, but being held firmly by the muscles which are inserted into its base, it is prevented from rising any more than enough to let the head out of the acetabulum, while the head and neck, slipping to the one side or the other, are found lying in such a manner that the side of the head, neck and trochanter are in contact with some part of the outer surface of the pelvis, varying, of course, in the different forms of luxation. This being borne in mind, it will be clear that any attempt at reduction, which merely brings the head of the bone to the acetabulum, will not succeed in making it enter that cavity, because of the lying-down position of the neck and trochanter against the side of the pelvis. We need, therefore, not only to bring the head over the socket, but at the same time to raise up the trochanter and neck, so as to allow the head to enter. Now, in the ordinary methods of reduction, this raising up of the trochanter, so as to put the neck in the proper direction for the head to enter its socket, is done first, by the action of the pulleys, and the approximation of the head to the socket is done second, by the continuation of the extension. This raising of the trochanter is, of course, opposed strongly by the muscles inserted into its base, causing the head to be pressed more and more firmly against the pelvis, and increasing the friction, and thereby causing, by far, the greater part of the difficulty in bringing down the head to the level of the acetabulum. It is in this, principally, and I am myself disposed to say only, that any active muscular contraction opposes the reduction of a dislocation of the hip-joint. True, the large muscles around the joint are thrown into action as soon as extension is made; but this is an action excited by the extension, and, that it is a very feeble opposing force, is evidenced by the facility with which these muscles give way to the force of a single unaided arm, when a fracture of the neck of the femur is concerned, in which, of course, none of the friction alluded to can occur. This comparative action of the muscles, in fracture and in dislocation, is very strongly and appropriately insisted upon by Dr. Reid.

"The process by manipulation avoids this main difficulty, and, as it were, eludes the opposition of the muscles. The trochanter, being fixed by the insertion into its base of the pyriformis, the two obturators, the gemelli, and the upper part of the quadratus, acts as a fixed point or fulcrum, upon which, by moving the limb, the head of the bone can be made to describe a circle round the fulcrum. When we remember that this fulcrum is not, strictly speaking, a fixed point, but has a certain degree of motion of its own, we can easily see how, by means of this movable fulcrum, the head of the bone can be placed, by varying the motions of the limb, on almost any point within two inches around the acetabulum, and, of course, over the acetabulum itself. If this manipulation is made in such a way as not to raise the trochanter from lying against the pelvis, then, when the head comes over the acetabulum, a slight rotation, such as is given by the rocking motion employed, will sufficiently raise the trochanter to let the head slip in without provoking to opposition the trochanteric muscles, and if the movements be made in such a direction as to relax the stretched muscles, the whole may be accomplished without calling forth the slightest muscular opposition from the beginning to the end of the procedure. This principle, in its application to the different forms of dislocation, presents some variations. In the dislocations on the dorsum, and on the ischiatic notch, for their mechanism is for our purpose identical, the principle has its best illustration; and, if any one will take the skeleton or the dead subject, and go through the process, he will perceive that, by adduction, the tense rotators are relaxed, and that, by flexion of the thigh upon the trunk, the head is caused to pass down behind and below the acetabulum, and then, by carrying the knee out so as to abduct the limb, that the head comes toward the lower portion of the acetabulum where its margin is least prominent. At this point, I wish it to be observed, that our mode of procedure varies a little from Dr. Reid's. He recommends, when the head is brought by adduction close to the lower edge of the acetabulum, that, by the rocking movement already described, it be caused to slip in. This is well, and will probably answer in many cases, but it failed us so completely from the first, that we were led to add the bringing down of the thigh to the straight position in a state of abduc-

tion, still keeping up the rocking motion, and it has been uniformly in the act of thus bringing down the limb, that the reduction has been accomplished. On looking at the parts in the dead subject, it will be seen that this movement of the limb, when the head has reached the lower margin of the acetabulum, tends directly to roll the head upwards over the edge and into the socket. The mechanism of the reduction from the foramen ovale has already been alluded to. I do not know of any case of reduction from the pubes.

"If the proposed plan should prove, on further trial, to be as successful and as free from danger as it thus far has been, one most valuable feature of it, as a surgical resource, will be its availability. Wherever a surgeon, with his bottle of ether, can go, there the dislocated hip can be reduced, without instruments, without appliances, without assistants."

"Everything," Dr. Markoe adds, "in our experience thus far seems to indicate that this method of reducing luxations is as safe, if not safer, so far as the integrity of the joint and its after usefulness is concerned, than the reduction by forcible extension with the pulleys. The method is not, however, without its dangers, and these mainly arise from the immense amount of force which can be exercised by acting on the long arm of so powerful a lever as the whole limb, while the short arm has at most a length of three inches. By the inconsiderate or misdirected action of this lever force, one of three accidents might be produced: either the muscles holding the trochanter, and thereby forming the fulcrum on which the power is applied, might be torn from their attachments; or, if they held fast, the tissues, round and among which the head passes in its movements, may be extensively lacerated or contused; or, lastly, it seems to me very possible that the neck of the bone might be broken by too violent abduction, forcing it against the side of the pelvis. Though happily these dangers are thus far only theoretical, yet the anatomy of the part shows that they are real, and each one of them might have most serious consequences. It would not be possible to lay down any specific rules whereby these dangers are to be avoided, except by insisting on that almost universal law of surgical manipulation, that everything is to be done with gentle moderate force, and never with sudden violence. As far as my recollection serves me, and I myself assisted in almost every case reported, we never have accomplished anything by proceeding in a direction where great force was required to continue the movement, but have always succeeded by finding a direction in which the mere continuance of the movement, without force, has brought the head into the proper position. It will be noticed, by looking over the cases, that in many of them, before the head went into its socket, it slipped about on the outer surface of the pelvis, taking sometimes one and sometimes another of the four positions usually spoken of as the four forms of luxation of the hip. This in some instances happened several times, and in the two instances of failure, this change of position was all that could be accomplished. Now, it cannot be doubted that this extensive movement of the head of the bone must be attended with a corresponding amount of laceration and displacement of the tissues among which it passes, and although the mischief thus done may be confined to the areolar tissue in the muscular interspaces, still it is an injury which may add to the dangers of after-inflammation, and is, if possible, to be avoided. I suppose this can only be done by defining more accurately the precise method of procedure to be adopted in each case, so that no experimental or ineffectual trials shall have to be made, but the operator shall be able at once to do exactly what is necessary to bring the head of the bone to the point desired."

The following case, given at the conclusion of Dr. M.'s paper, has such important bearing upon some of Dr. M.'s remarks that we quote it:—

"Patrick Barry, aged 42, was admitted to the New York Hospital, Oct. 23, 1854, with a dislocation of the left femur, which had occurred seven weeks previously, by a fall from a rail-car while it was in motion. The symptoms were unequivocal, the limb being shortened $1\frac{3}{4}$ inches, the ball of the great toe resting on the instep of the sound foot, and the head of the bone being distinctly felt upon the dorsum of the ilium. The patient was a man of good muscular development, but the injured limb was somewhat wasted and flabby from inaction. Two days after admission he was put under the influence of

ether, and Reid's manipulation was tried. The head descended as usual, until it came opposite to the lower margin of the acetabulum, but from that point, as the limb was brought down, it slipped on to the foramen ovale. The manipulation was repeated several times, with all care, varying the degree of abduction on the various trials, but without success. It was impossible to make the head rise over the lower border of the acetabulum so as to slip into its place. After numerous thorough and careful trials, the manipulation was abandoned and the pulleys ordered to be applied. Before this was done, it was thought best to place the head of the bone on the foramen ovale, and from that point to try and reduce it by the usual method recommended by Sir Astley Cooper. The head was accordingly placed on the foramen, and while the upper part of the thigh was grasped by an assistant and lifted strongly outwards, I took hold of the ankle and made extension and adduction. The head seemed not to move at all under this force, and while making strong adduction a crack was heard, everything became loose about the joint, and on examination it was evident that a fracture of the cervix had taken place, leaving the head in the foramen ovale. There was nothing further to be done but to put the limb up in the straight apparatus, hoping that, if we could obtain union, he would have as useful a limb as those ordinarily left by fracture of the cervix, and certainly a better limb than if the dislocation had been untouched. Thus far, Nov. 25, everything has gone well, and promises union, with a shortening of about an inch. I am sorry that we must accept this case as one of failure of the new plan after what we considered a fair trial; for myself, however, I do most profoundly believe that it failed simply because we have not yet learned enough about the manipulation to adapt it to the condition of parts concerned in this particular instance. That we shall yet acquire that knowledge, I see no reasonable ground to doubt. With regard to the fracture of the cervix, we were all surprised at the slight amount of force which was competent to produce such a mortifying accident. It adds double force to the caution given above, when speaking of the possibility of that accident, and it is not a little remarkable that the paragraph containing that caution was written on the very morning of the day when the production of the fracture verified the necessity of the warning. Dr. Watson, in a note to me, speaks of a fact which, he says, 'I have on undoubted authority, viz: from one of the professors in the School of Medicine in Toronto, Ca., that an accident, similar to that of Patrick Barry, occurred in that city, while the surgeon was attempting to reduce a luxation of the hip by Reid's method.' Finally, it must be observed that the new plan is entitled to none of the blame of the fractured cervix. The accident took place after Reid's manipulation was abandoned, and while we were attempting the reduction according to the old established and classical method."

Excision of the Head of the Femur, and Removal of the Upper Rim of the Acetabulum, for Morbus Coxarius.—Dr. LEWIS A. SAYRE records (*New York Journ. Med.*, Jan., 1855) an interesting case of this. The subject of it was a girl, nine years of age, who had been suffering for 18 months with the disease, and which resisted the usual treatment. When seen by Dr. S., an abscess had formed, involving the whole upper front and inner portion of the thigh, accompanied with chills, profuse sweats, and great prostration. The abscess had pointed in two places, and was apparently just ready to open.

"The leg was shortened $2\frac{1}{4}$ inches, and turned inward, but not permanently fixed in its position (as is usual), but allowing of considerable motion, which gave a distinct bony crepitus between the femur and ileum. The pelvis was twisted and drawn upwards. Her general health had become much affected, having lost her appetite, and she was suffering from hectic, with constant chills and profuse sweats, and was only rendered comfortable by the constant use of anodynes."

Dr. S. advised a free opening of the abscess, and, if necessary, the removal of the head of the femur. This was at first objected to by the father, but subsequently consented to, and the operation was performed on the 29th March, 1854.

Dr. S. first laid open the abscess by a free incision of about six inches, over the trochanter major, on the outer aspect of the thigh, and in a line with the femur, and then cut into the floor of the abscess (which principally occupied the inner and front portion of the thigh), and discharged about a pint of thin serous and flaky pus. The finger was then readily passed around the neck of the femur, and detected an opening in the capsular ligament on the inner surface of the neck. The upper border of the acetabulum had been absorbed, and the head of the femur was upon the dorsum of the ileum, near the anterior superior spinous process, *surrounded by its capsule* (which seemed to have been slipped up), and a large deposit of bone, apparently being an attempt of Nature to make a new acetabulum. But this cavity thus formed had no lining membrane, as the femur grated roughly upon it. Dr. S. then opened the capsular ligament on a line with the external incision, and disarticulated by bringing the leg strongly across the opposite thigh, and then, with a large pair of Luer's forceps, readily cut off the head of the femur, at the lower extremity of the neck. The bone at this point appeared perfectly healthy. Dr. S. was very cautious not to injure the insertion of the psoas magnus, or iliacus internus, or any of the rotator muscles, which are inserted just behind the trochanter major.

The upper rim of the acetabulum had been absorbed (according to the theory of Dr. March, of Albany), and the new deposit of bone, which was intended to supply its place, was denuded and carious. Dr. S. gouged it off with a sharp, firm chisel, made for that purpose, and, in this way, took off a number of flakes of bone, until he came to a healthy, bleeding surface.

The anterior superior spinous process on its outer surface, and the external lip of the crest of the ileum, was black and carious for some distance, and with the forceps he easily clipped it off until he came to healthy bone. Very little blood was lost in the operation, and after cleaning away all the debris, he brought the leg in the straight position, filled the wound with lint, and dressed with a roller and cold water compress. She was then put to bed, and a cup of strong coffee administered, after which she soon fell asleep.

The child was under the influence of chloroform during the operation, which occupied nearly 20 minutes, and was perfectly insensible the whole time.

The case went on favourably, and, on the fifth of April, Dr. S. applied the straight splint, for counter extension, to the well side, and made extension by means of the foot-board, bringing the limb down to the same length of the opposite. Though some unfavourable symptoms subsequently presented themselves, the cure progressed. After the 1st of August, at each dressing, the body was brought to a right angle with the thighs, to prevent ankylosis.

On the 1st September, when the bandage was removed, there seemed no tendency to retraction of the limb. The splint was reapplied, but the body was left free from the bandage, so as to allow of flexion, in order to prevent ankylosis. By the middle of October, began to walk on crutches, and on the 20th November, Dr. S. reports:

"To-day I placed her in the horizontal position, and measured her carefully, and find there is $\frac{1}{8}$, or nearly $\frac{1}{4}$ of an inch shortening. By taking hold of the foot, the whole body can be drawn down in bed without pain in the joint, and a pressure may be made sufficiently strong to move the pelvis and body upward without producing any shortening of the limb. When she lies upon the back, with the leg extended upon the thigh, she can elevate the heel sixteen inches from the bed, and flex the knee so as to bring the thigh at a right angle with the pelvis; she can rotate it internally so as to touch the other foot, and externally so as to touch the bed. Her general health is perfect, and the case has terminated perfectly successfully."

Dr. S. gives a table of thirty cases, in which excision of the head of the femur has been performed for morbus coxarius; of these, 20 recovered, and 10 died. Of the former, 13 were completely successful; 3 died of an intercurrent disease, at periods varying from three months to two years after the operation; 1 is reported as not having progressed favourably; the remainder were too meagrely reported, or too recently performed to decide correctly of results.

Of the latter, 4 died within one week after operation; 1 on the 12th day; 2

in two months; 1 in four and a half months; 1 in some months after; 1 unsuccessful.

Perineal Section for Stricture of the Urethra.—The *New York Journ. of Med.* for March, 1855, contains an interesting article by Dr. FRED. D. LENTE, on this operation, which our readers are aware has been latterly revived by Mr. Syme, of Edinburgh, and its propriety has given rise to much angry discussion in Great Britain. Mr. Syme never performs the operation without first introducing a guide into the bladder, which he insists can always be done, there being, he maintains, no such thing as an *impermeable* stricture. Mr. Fergusson has lately performed this operation in several cases in King's College Hospital, and in his clinical lecture on the occasion, asks whether the operation can be performed in cases in which no instrument can pass. Mr. Lente expresses his surprise at this query, and states that the perineal section, both with and without a guide passed through the stricture, has been by no means an uncommon operation in New York city, for years past, and refers to a table embracing twenty-seven cases of this operation, published by him in the 4th Vol. of the *Transactions of the American Medical Association*.

"The operation of external division without a guide is looked upon," Dr. L. states, "in the New York Hospital, as so practicable and so successful, though certainly, at times, one of the most annoying and difficult of operations, that, in the cases of obstinate retention of urine admitted into the institution during our connection with it, where the introduction of any instrument was found to be absolutely impracticable, the practice of puncturing the bladder was never once resorted to, but relief was always attempted by the perineal incision. In the London hospitals, on the contrary, it would seem that the operation of puncture is generally preferred. Mr. Hilton, Surgeon to Guy's Hospital, says that twenty or thirty such cases have occurred there during the past few years, and reports three cases himself in one number of the *Lancet*; in one of which, the catheter having slipped from the opening (into the rectum), the operation was necessarily repeated *de novo* on the succeeding day; all of these three operations were successful. The operative procedure, in cases where the obstruction is impassable, is briefly this: The patient having been secured in the usual position, a medium-sized catheter or sound is passed down to the stricture and kept in position by an assistant; the incision is then carried down in the median line to the end of the instrument, which is thus exposed; it is then continued downwards towards the posterior end of the stricture (which is usually of some length), and the anterior point of that part of the membranous portion, still unobstructed, and generally *distended* with urine, diligently sought for. This constitutes the only difficult part of the operation, and may occupy from five minutes to an hour or more; generally, from five to fifteen minutes, however, have sufficed to discover it. As soon as a fluctuating point is perceived, either by the touch or by an experimental incision in the proper direction, an opening is made with a sharp-pointed bistoury, a director introduced, and then the end of the instrument, previously introduced through the distal portion of the urethra. It has been the usual custom to retain a *silver* instrument in the canal, secured by an appropriate bandage, as being the most cleanly. This is removed from time to time, and cleaned. In some cases, when the knife or director has been inadvertently removed from the posterior part of the divided urethra, after all the urine has been evacuated, and before the catheter has been passed into the bladder, it has been extremely difficult to find the opening again, and thus to complete the last step of the operation; especially in cases where the anatomical relations of the parts have been disturbed by the existence of abscesses and fistulous canals.

"Regarding the question, so much mooted on the other side of the Atlantic, as to the possibility of passing an instrument in *every* case of stricture, we have only to say for ourselves, on this side, that, in many cases, our best surgeons, after the most persevering and gentle trials with all kinds of instruments, are sometimes completely foiled, and these are the cases to which the operation under discussion has been applied. Most, if not all of the cases contained in the Table were of this character, and all of those which are mentioned as hav-

ing occurred in private practice, with, perhaps, one exception, this having been complicated by large abscesses, and by fistulas. We are convinced of the fact which Mr. Fergusson says he has long had the idea of—"that the seeming dexterity (in passing instruments in these impermeable strictures) was nothing but actual violence;" and that surgeons "appear to pass the instrument easily into the bladder, whilst they are *really forcing* their way through a canal, the relative anatomy of which is very familiar to them." We may, in this connection, remark that, in the practice of the New York Hospital, when even the smallest-sized conical instrument can be introduced, the idea of dividing the perineum, as a means of cure, is not entertained, but a cure by gradual dilatation, with graduated instruments, is confidently expected; we remember no instance of a different result. This being the case, the first step in the operation, as performed by the great Scotch surgeon and by the surgeons of the London hospitals generally, is an argument against the propriety or necessity of any cutting operation whatever; for, why should a patient be subjected to so severe a procedure as the perineal section, when the passage of the guide through the stricture may be followed by larger instruments, and the canal thus enlarged to its normal dimensions? In cases complicated by abscess or by extensive fistulas, it may be, and is often, necessary to perform the cutting operation, even though a small instrument can be introduced. In a case which we have recently had under treatment, and which we may be permitted to introduce here as an exemplification, it was only after repeated trials, even with the important aid of an anæsthetic, and with considerable difficulty, that we could introduce an instrument, and then only one of the fine-pointed, flexible bougies manufactured by the French, and so admirably adapted to these cases, its diameter, at its largest part, being only one-half that of silver catheter No. 1, and yet, in the course of three or four weeks, by careful and gradual dilatation, the urethra was enlarged to the size of No. 10."

Of the twenty-seven cases contained in Dr. Lente's table, three died, two were much relieved, four were relieved, and eighteen were cured. "In some of the cases which were discharged as relieved, a further operation might have resulted in cure, but the patients were unwilling to submit to it," being contented with the alleviation of their sufferings already afforded. In many instances in which the operation was performed, it was under very unfavourable circumstances. In seventeen cases, in which the duration of the existence of the stricture, previous to the operation, is mentioned, the average is seven years. "In most, if not all of the cases, the operation was regarded as the only means of effecting a cure, all other means having failed; and, in many instances, it was necessary, not only for the purpose of rendering the patient's life more comfortable, but for saving it. The proportion of cutting operations for stricture, to the number treated by dilatation, is very small in the New York Hospital."

"We look upon it as a matter of the greatest importance, to establish the feasibility and comparative safety of the perineal section, and especially as performed without a guide, for in most, if not all cases requiring the operation, surgeons find it impracticable to introduce any guide, although one of small size may, perhaps, usually be forced through by one well acquainted with the course of the canal and its relations, but in no case, we think, without danger of making false passages, and thus materially increasing the danger, and diminishing the chances of a perfect cure from the subsequent operation. The cases included in our Table, and those performed in private practice in the city, with like results, will, in our opinion, go far towards increasing the confidence of surgeons in this operation."

Ovariectomy.—This operation was recently performed by Dr. A. MERCIER, of New Orleans. The patient was a woman, 28 years of age, the mother of four children. A swelling was first observed in her right iliac region, about a year before. Six months afterwards she was tapped, and six gallons of a white, thick albuminous fluid drawn off. The effusion rapidly accumulated again, and she was tapped six times subsequently. The operation was performed on the 17th Dec. 1854. The patient having been put under the influence of chlo-

reform, "an incision, nine inches in length, was made over the tumour, extending from the lower ribs to the external edge of the rectus abdominalis. Two small muscular arteries were tied during the course of the incision, and but little blood was lost, either then or subsequently. With some difficulty the adhesions which bound the tumour to the abdominal parietes, the liver, the iliac fossa, and to the omentum, were torn apart with the hand, the intestines, &c. being freely handled for this purpose. Near the pedicle of the tumour the adhesions were so strong, for more than an inch in extent, that the bistoury had to be resorted to. The tumour presented the appearance externally of an enlarged ovary, and such was its size that, in order to draw it out of the abdominal cavity, several deep incisions had first to be made into it, and a large portion of its fluid contents to be evacuated. Strong, thick twine was then tied as forcibly as possible around the pedicle of the tumour, and it was excised about half an inch above the ligature. The internal parts were freely sponged, and the lips of the wound brought together with five sutures, which included all the parts incised—the peritoneum, muscles and integument. The lips of the wound were further approximated with adhesive straps, and covered simply with lint. Openings were left, sufficiently large to admit a ready egress to any fluid contents of the abdomen.

"The tumour, when extracted, measured nine inches in length, and six in diameter. Its weight could not have been less than six pounds. It was of a fibro-cartilaginous character, and composed of a mass of small conglomerated cysts of various sizes, containing a fluid of the colour and consistency of glycerin.

"By the seventh day, the muscular portions of the wound had united, except opposite to the sutures. On the thirteenth day, the ligature came away, and indicated by its appearance that, with a little force, it might have been removed without danger several days before. From this period the flow of serum through the wound, which at first had been considerable, has rapidly decreased."

At the date of the last report, seventeen days after the operation, the wound had almost entirely healed, and the patient was sitting up.—*New Orleans Med. and Surg. Journ.*, Jan. 1855.

The entire Scrotum and Perineum, with one Testicle and its Cord attached, and nearly all the Integuments of the Penis torn off.—Dr. ROBERT W. GIBBES, of Columbia, S. C., relates (*Charleston Medical Journal*, March, 1855) a very interesting case of this. The subject was a coloured man, 22 years of age, who was caught by the shafting in a foundry, and the following sustained: "The entire scrotum was torn off, with the skin and cellular tissue, from two and a half inches above the spine of the pubis down to the edge of the sphincter, and including all the breadth of the perineum, together with the left testicle with five inches of its cord attached, and all the integument and cellular covering of the penis, except a rim nearly half an inch wide at the extremity, and continuous with the mucous membrane of the prepuce.

When seen by Dr. G. the patient was suffering most acutely. Chloroform was immediately administered, and on examination the right testicle was seen hanging down by its bare cord, and apparently covered only by the tunica vaginalis communis as high as the abdominal ring, where the elastic feel of the intestines was distinctly perceptible, as well as on the opposite side, where the end of the cord was so retracted as to render its ligation impossible. Not more than half oz. blood could have been lost, as scarcely any oozing was observed, and the parts separated were almost as clean as if they had been sponged. The gap in the perineum was brought together by three points of the interrupted suture, and, as it was impossible to form a covering for the raw surface and remaining testicle, a simple lint dressing was applied, the penis supported on the left thigh, and the testicle on a small bran cushion placed between the legs.

The patient recovered, and his sexual appetite, he says, is as great as before the accident, and coition as agreeable as ever; the only difference, he observes, is that the semen "does not come as free." One of his fellow-workmen said

to me, "Oh, he is as good a man as ever;" to which Henry replied *that he believed he was.*

Treatment of Ulcers by Anaplasty.—Having in our preceding No. (pp. 276-78) noticed the rival claims of Dr. F. H. HAMILTON and Dr. JOHN WATSON, to a method of treatment of ulcers by Anaplasty, it is but justice to the former surgeon to state that in a subsequent reclamation (*New York Journ. Med.*, Jan. 1855) he denies that Dr. Watson has not anticipated him in either the proposal or the results; but states that Dr. Watson has misapprehended the principle of his operation. This operation consists in transplanting a piece of new and perfectly healthy skin upon the *centre* of the ulcer, by which means he hopes not only to supply an amount of skin equal to the size of the piece transferred, but to furnish, also, a nucleus from which additional skin shall be formed—to establish a new centre of life—an oasis, from whose outer verge a true and healthy vegetation shall advance in every direction over the exhausted soil.

"It is not improbable," he remarks, "also, that the graft will itself expand, or be drawn centrifugally by the contraction of the surrounding granulations and cicatrix, conversely, as the skin about the ulcer had before been stretched and drawn centripetally, by a similar action of the granulations and cicatrix situated within its free margin, so that, after a time, it will cover more space, independent of any actual growth, than it did originally. The opposite of this happens usually in anaplasty, and would occur here, did the flap equal or exceed in size the wants of the parts to be supplied. The flap would contract, thicken, and project itself above the surface. But in old ulcers, it will generally be found impossible to furnish a direct supply of integument equal to the loss. A deficiency must probably still exist, and sufficient, it is believed, to determine in the transplanted skin a necessity of expansion."

He further states that his proposals dates from the year 1844.

Fungus Hæmatodes of the Eye; Extirpation; Recovery.—Dr. JAMES BOLTON relates (*Stethoscope*, Feb. and March, 1854) a case which he considers to be an example of fungus hæmatodes of the eye, occurring in a negro, 35 years of age. The disease was not traceable to any injury; its progress was slow, and for a long time painless. The system had begun to sympathize deeply, exhibiting a cachectic condition. The eyeball was very much protruded. Dr. Bolton extirpated the whole globe. A section of it afterwards showed a fungous tumour of a brownish colour occupying almost the whole interior of the ball, appearing to spring from the ciliary body, and including the iris and choroid coat. The lens and vitreous humour had disappeared; a small quantity of fluid, containing reddish granular particles, was evacuated; the optic nerve was sound. The patient continued well twenty months after the operation.

Results of Twenty-four Operations for Lithotomy.—In the No. of this Journal for July, 1850, Dr. P. C. SPENCER, of Petersburg, Va., reported the results of fifteen operations for lithotomy, and in the *Virginia Medical and Surgical Journal*, for February last, he reports nine more cases. Of these nine, 6 were whites, 5 boys, and 1 man 64 years of age; three were blacks, 2 boys, and 1 man aged 61.

Of the twenty-four cases, Dr. S. lost but two, a proportion of 1 in 12.

Choleraic Epidemic at the Massachusetts State Prison in July, 1854.—Dr. WM. B. MORRIS, Physician to the Massachusetts State Prison, gives (*Boston Medical and Surgical Journal*, March 15, 1855) the following account of the outbreak of cholera in that institution in July last:—

"At midnight, July 27, I was called to attend Peter York, a coloured convict, who had been in solitary confinement for nearly seven years. On arriving at the prison, I found him with a feeble pulse, cool skin, cramps of the abdomen and extremities, purging and vomiting. Thinking it an ordinary case of cholera morbus, I prescribed the usual remedies, and as soon as relief was afforded, returned home. I had scarcely gotten into bed when I was again called; and before reaching the prison a second time, four new cases had occurred

in different parts of the institution. The attacks became from this time so frequent that I was obliged to remain constantly in the hospital, which soon became crowded with patients. During the whole of the 28th, the succeeding night and following day, new cases continued to occur, so that in forty-eight hours from the commencement of the epidemic 205 convicts had been more or less severely attacked. The symptoms varied in intensity in different cases, but in all were characterized, to a greater or less degree, by lividity of the lips and surface of the body, cold extremities, painful cramps, vomiting and purging. In a few instances, also, there was suppression of urine.

"The causes of this sudden outbreak are, to my mind, wholly conjectural. The report that it depended upon diseased beef is entirely without foundation. *No beef had been given at all.* The ration on the 27th, consisted of 'dun-fish' (Mr. Mason and myself examined the lot from which they were taken, and found them to be of the best quality), rice and bread. On the next day there was an entire change of food. I have forgotten now what the diet for that day was, though I have it somewhere among my papers. Yet notwithstanding this change, 86 new cases occurred. There was a very sudden change of temperature on the evening of the 27th, from excessively hot to uncomfortably cool weather. I suffered so much in going to the prison to visit York, that I borrowed an overcoat to wear home. This change of temperature, together with the fact that cholera was prevailing at the time, has always inclined me to believe that atmospheric causes played an important part in the production of this sickness."

Employment of Injections into the Bronchial Tubes, and into Tubercular Cavities of the Lungs.—Dr. HORACE GREEN, of New York, claims (*American Medical Monthly*, Jan. 1855) to have treated with advantage bronchial affections and tuberculosis, "by the direct introduction into the lungs of a strong solution of nitrate of silver injected through" an "elastic tube." The subject was brought before the New York Academy of Medicine on the 6th of December last, and referred to a committee to inquire into and investigate the proposed treatment (*New York Med. Times*, Jan. 1855). Although nearly four months have now elapsed, this committee have not yet made a report. It will be time enough when the projector shall have satisfied the committee of the practicability of his procedure, and of its utility, if practicable, to give it further notice.

Vesico-Vaginal Fistula cured by Sims's Operation.—Dr. A. V. WILLIAMS records (*New York Med. Times*, March, 1855) a case of vesico-vaginal fistula, the opening large enough to admit two fingers, cured by Dr. Sims's operation.

AMERICAN MEDICAL ASSOCIATION.

The eighth annual meeting of the American Medical Association will be held in the city of *Philadelphia* on Tuesday, May 1, 1855.

The secretaries of all societies, and other bodies entitled to representation in the Association, are requested to forward to FRANCIS WEST, M. D., 352 Chestnut St., Philadelphia, one of the secretaries, correct lists of their respective delegations *as soon as they may be appointed*; and it is earnestly desired by the Committee of Arrangements that the appointments be made at as early a period as possible.

MEDICAL SOCIETY OF THE STATE OF PENNSYLVANIA.

The annual session of the Society will be held in Hollidaysburg, on the last Wednesday (30th) of May, at 10 o'clock in the forenoon.

The secretaries of the several County Societies will please forward the lists of delegates to either of the Secretaries, D. FRANCIS CONDIE, Philadelphia, or HENRY CARPENTER, Lancaster.

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GRADUATES OF JEFFERSON MEDICAL COLLEGE OF PHILADELPHIA,

MARCH, 1855.

At a Public Commencement, held on the 10th of March, 1855, the degree of DOCTOR OF MEDICINE was conferred on the following gentlemen by the HON. EDWARD KING, LL. D., President of the Institution; after which a Charge to the Graduates was delivered by PROFESSOR PANCOAST.

NAME.	STATE.	SUBJECT OF THESIS.
Allen, Milton	Pennsylvania.	Repose.
Allen, Thomas J.	Tennessee.	Inguinal Hernia.
Anawalt, James W.	Pennsylvania.	{ Susceptibility of Cold-blooded Animals of Inflammation.
Anthony, Wm.	Pennsylvania.	Scarlatina.
Asch, Morris J.	Pennsylvania.	De Abscessu Frigido.
Ashcraft, John H.	Pennsylvania.	Intermittent Fever.
Banner, Constantine Lafayette	North Carolina.	Colitis.
Barham, R. G.	Virginia.	Acute Enteritis.
Barr, Richard Rice	North Carolina.	Physical Growth and Decay.
Barr, W. F.	Virginia.	{ Medical Properties and Uses of Veratrum Viride.
Bass, Robert E.	Virginia.	Acute Gastritis.
Bartolette, T. Miles	New Jersey.	Differential Diagnosis.
Beeler, Milton W.	Mississippi.	Childbed Fever.
Bell, Edwin R.	Ohio.	Ovarian Cysts and Tumors.
Bell, George G.	Alabama.	Puerperal Fever.
Bell, W. D.	Texas.	Pneumonia.
Bennett, Edmund	New Jersey.	Convulsions.
Besselleu, Wm. F.	South Carolina.	Endosmose and Exosmose.
Beveridge, John L.	Georgia.	{ Epidemic Dysentery in Georgia, in 1853 and 1854.
Blackford, Ben., Jr.	Virginia.	Importance of the Study of Anatomy.
Boies, Jeremiah S.	Delaware.	Vaccination.
Boswell, Lewis A.	Virginia.	Remittent Fever.
Bradford, Philander Spotswood	Virginia.	Opium.
Brandt, Eli Bainbridge	Pennsylvania.	Phenomena of the Diseases of Women.
Breed, Wm. M.	Pennsylvania.	{ Sulphate of Quinia Therapeutically considered.
Breitling, Joseph	Alabama.	Scarlatina.
Brown, Bedford (M. D.)	Maryland.	Practical Bandaging.
Brown, Martin L.	North Carolina.	Typhoid Fever.
Brubaker, J. C.	Pennsylvania.	Dentition.
Bryan, Cyrus	Virginia.	Remittent Bilious Fever.
Buchanan, G. W.	Missouri.	The Physician's Life.
Buckwell, Edward G.	Ohio.	Ovulation and Menstruation.
Buffington, John F.	Maryland.	Necroscopy.
Buffington, John N.	Virginia.	Epidemic Cholera.
Butler, A. B.	Ohio.	Properties of Wahoo.
Cahall, Lawrence M.	Delaware.	Croup.
Callaghan, Arthur	Missouri.	Phenomena of Inflammation.
Campbell, Westlee M.	North Carolina.	Oblique Inguinal Hernia.
Cato, James F.	Georgia.	Intermittent Fever.
Chew, Henry B., Jr.	Maryland.	Apoplexy.
Chrisman, Burke	Virginia.	Pneumonia.
Clark, Hobson	Virginia.	Stomachal Digestion.
Clark, Joseph M.	Massachusetts.	Insanity.
Cole, John P.	New Jersey.	Pleuritis.
Comfort, Wm. A.	Canada West.	Cause of the Heart's Action.
Cowan, George	Kentucky.	Relations of Psychology to Physiology.
Cowell, J. Geo.	Massachusetts.	Croup.
Craven, Edmund R.	Pennsylvania.	Neuralgia.
Crothers, Robert W.	Illinois.	Constipation accompanying Pregnancy.
Curd, John R.	Virginia.	Yellow Fever.
Dalton, George O.	Massachusetts.	Phthisis Pulmonalis.
Davies, William B.	Virginia.	Inguinal Hernia.
Davis, James S.	Mississippi.	Cholera Infantum.
De Barres, Ph.	Cuba.	Les Signes Physiques de la Pleurésie.
Dean, Samuel Henry	Georgia.	Puerperal Fever.
Denise, Jacob C.	Ohio.	Absorption.
Dickerson, Robert J.	Alabama.	Pathological Anatomy of Tubercle.
Dickson, Lycurgus A.	Tennessee.	Menstruation.
Dieffenbacher, Philip L.	Illinois.	Hope, a Hygienic and Therapeutic Agent.

NAME.	STATE.	SUBJECT OF THESIS.
Dobyns, R. L. H.	Virginia.	Typhoid Fever.
Donnelly, Charles Henry	Canada West.	Abortion.
Dreher, Jurias G.	Pennsylvania.	Scarlatina.
Dunham, Albright	New York.	Metro-peritonitis.
Failor, Benjamin M.	Ohio.	Intermittent Fever.
Fant, Samuel F.	South Carolina.	Typhus Fever.
Feay, John	Pennsylvania.	Cynanche Trachealis.
Finlay, Charles	Cuba.	Capillarity applied to Physiology.
Finlayson, William H.	North Carolina.	Peritonitis.
Finley, Calvin G.	South Carolina.	Bilious Remittent Fever.
Fischer, W. Emil	Prussia.	{ De Consuetudinis et Idiosyncrasie vi in Medicaminum Effectum Exhibita.
Fisler, Jacob T.	New Jersey.	Intermittent Fever.
Fleming, Andrew	Pennsylvania.	{ Action of the Alkaline Chlorides on Calomel.
Flournoy, David	Virginia.	Hæmoptysis.
Ford, George W.	Pennsylvania.	Etiology of Phthisis.
Freas, H. L.	Pennsylvania.	Rubeola.
Frederick, Edward J.	South Carolina.	Capillary Circulation.
Gabby, R. S.	Ohio.	Pleurisy.
Garnett, O. V.	Kentucky.	Hydrophobia.
Garnett, Thomas N.	Virginia.	Pneumonia.
Graham, Leonidas J.	Alabama.	Digestion.
Gray, Alexander R.	Pennsylvania.	The Protein Group.
Gray, John W.	Alabama.	Hernia Inguinalis.
Groom, Evan J.	Pennsylvania.	Yellow Fever.
Gross, Ferdinand H.	Pennsylvania.	Peculiar Management of a Case of Labor.
Guild, James, Jr.	Alabama.	Yellow Fever.
Gwin, Robert D.	Tennessee.	Respiration.
Hagenbuch, William A.	Ohio.	Intermittent Fever.
Hall, Geo. W.	Illinois.	{ Effects and Treatment of the Poison of the Rattlesnake.
Hall, R. F.	Georgia.	Inflammation of the Cervix Uteri.
Hamilton, John W.	Pennsylvania.	Pulmonary Tuberculosis.
Hamilton, Swithen N.	Virginia.	Dysentery.
Hanly, Michael A.	Pennsylvania.	Digestion.
Harding, Philander H.	Maine.	Inflammation.
Hardwick, J. R.	Georgia.	Scarlatina.
Harris, Wm. H.	Pennsylvania.	Concussion of the Brain.
Harriss, Samuel G.	Virginia.	Physiology of Digestion.
Harvey, William C. (M. D.)	Missouri.	Remittent Fever.
Haslett, John D. S.	Pennsylvania.	Scarlatina.
Haynie, James M.	Alabama.	{ Duties, Responsibilities, and Pleasures of a Physician.
Hebble, Joseph	Ohio.	Typhoid Fever.
Heddens, William I.	Kentucky.	Physical Education of Females.
Herbst, Wm. S., Jr.	Pennsylvania.	Pneumonitis.
Hill, Joseph	South Carolina.	Sleep.
Hinchman, B. (M. D.)	Pennsylvania.	Relation of Age to Disease.
Hitt, Willis M.	Indiana.	{ Benefits of Sickness and Pleasures of Con- valescence.
Holman, Henry W.	Virginia.	Typhoid Fever.
Hopkins, Benjamin C.	Delaware.	{ Alcohol, Chemically and Therapeutically Considered.
Hopkins, Benjamin F.	Virginia.	Pleuritis.
Hoskins, Wm.	Virginia.	{ Surgical Means Employed to Arrest Trau- matic Hemorrhage.
Houston, John	Pennsylvania.	Erysipelas.
Hudson, William M.	Connecticut.	Formation of Callus in Fracture.
Hume, Quintus R.	Virginia.	Fractures of the Femur.
Hunt, David G.	Georgia.	Fractures.
Hunt, William Henry	Kentucky.	Cynanche Trachealis.
Hunter, Samuel B.	Maine.	Typhoid Fever.
Ingram, S. L.	Virginia.	Entero-mesenteric Fever.
Irvin, George	Pennsylvania.	Erysipelas.
Irvin, William	Pennsylvania.	Puerperal Fever.
Jackson, John S.	Virginia.	Scarlatina.
Jennings, Robert B.	Virginia.	{ Cholera Infantum in the Valley of Virginia in the Summers of 1853 and '54.
Jernigan, Charles H.	Alabama.	Pleurisy.
Johnston, James	Pennsylvania.	Autumnal Remittent Fever.
Jones, Denwood T.	Maryland.	Amenorrhœa.

NAME.	STATE.	SUBJECT OF THESIS.
Jones, James Y.	Virginia.	Gunshot Wounds.
Jones, Joseph Addison,	Virginia.	Cholera Infantum.
Jones, R. Augustus	Alabama.	Yellow Fever.
Jones, William W.	Georgia.	Phthisis Pulmonalis.
Kane, John K., Jr.	Pennsylvania.	Albuminuria.
Kay, Isaac F.	Pennsylvania.	Traumatic Hemorrhage.
Kennedy, Maxwell	Pennsylvania.	Gout.
King, John F.	District Columbia.	Puerperal Convulsions.
Kuhn, Louis De Barth (M. D.)	Pennsylvania.	Fractures of the Radius.
Leitch, J. Lewis	Virginia.	Etiology.
Lewis, James E.	Virginia.	Dysentery.
Lewitt, Wm.	Michigan.	Delirium Tremens.
Lindsay, Andrew	Pennsylvania.	Hydrangea Arborescens.
Logan, Samuel	Pennsylvania.	Dysentery.
Lowman, Wm. G.	Pennsylvania.	Erysipelas.
Lumpkin, Samuel P.	Georgia.	Carbuncle.
Lynn, B. W.	Indiana.	Medical Superstition.
McClelland, Freeman	Pennsylvania.	Medical Psychology.
McCorkle, Wm. A.	Virginia.	Chronic Gastritis.
McDowell, Geo. M.	Georgia.	Acute Endogastritis.
McEwen, Christopher	Pennsylvania.	Typhoid Fever.
McKinney, J. W.	Illinois.	Colitis.
McLeod, John	Alabama.	Puerperal Fever.
McMullen, James	Pennsylvania.	Erysipelas.
McPherson, Geo. E.	Pennsylvania.	Pneumonia.
McPherson, John H.	Virginia.	Scarlatina.
Mackey, Alexander S.	Virginia.	Anthelmintics.
Maddox, John Z.	Georgia.	Typhoid Pneumonia.
Madill, Thomas F.	Pennsylvania.	Habitual Constipation.
Magill, Thomas	Pennsylvania.	Eclampsia Gravidarum et Parturientium.
Maris, Edward	Pennsylvania.	Mental Development.
Markle, John Gideon	Pennsylvania.	{ Remittent and Intermittent Fever of the
May, John R.	Virginia.	{ Susquehanna.
Miller, James S.	Pennsylvania.	Scarlet Fever.
Mills, Nathaniel J.	Kentucky.	Rubeola.
Milton, Harvey Oliver	Alabama.	Pneumonia.
Moody, Milus W.	Mississippi.	Yellow Fever.
Moore, Alexander P.	Arkansas.	Dysentery.
Moore, John A.	North Carolina.	Cystitis.
Morgan, James	Pennsylvania.	{ Acute Dysentery of 1854, in Orange
Morrow, Wm. L.	Pennsylvania.	{ County, N. C.
Moss, Wm.	Pennsylvania.	Scarlatina.
Murry, John Y.	Mississippi.	Intermittent Fever.
Myers, W. H.	Ohio.	Pulmonary Tubercles.
Nash, Miles H.	Florida.	Menstruation.
Newman, Wm. H.	Kentucky.	{ Excretions an Index to Organic Changes
Nichol, John	New York.	{ in the System.
Nicholson, John C.	Tennessee.	Menstruation.
Ogburn, John F.	Virginia.	Anæsthesia.
Ohl, Josiah G.	Pennsylvania.	Phthisis Pulmonalis.
Osgood, William	Pennsylvania.	Febris Remittens.
Overton, Wm. S.	Virginia.	Acute Dysentery.
Park, William H.	Ohio.	Hysteria.
Patton, Thomas	Ohio.	Le Moral sur le Physique.
Payzant, Elias N.	Nova Scotia.	Entero-mesenteric Fever.
Peeples, P. W.	Mississippi.	Dysentery.
Pinkard, H. M.	Virginia.	Dysentery.
Pinson, Washington S.	South Carolina.	Mortification by Cold.
Plaisted, E. Freeman	New Hampshire.	Secretion of the Bile.
Plummer, Gardiner H.	Indiana.	Physiology of Digestion.
Pope, Chandler M.	Georgia.	Bilious Remittent Fever.
Pope, Willis	Alabama.	Physical Signs of Phthisis.
Porter, Benjamin F.	Pennsylvania.	Homœopathia.
Price, Robert A.	Virginia.	Amaurosis.
Quinn, John H.	Ohio.	Inguinal Hernia.
Ramsay, Wilson C.	North Carolina.	Intermittent Fever.
Ramsey, William P.	Pennsylvania.	Gastric Digestion.
Ransberry, John, Jr.	Pennsylvania.	{ Action of Therapeutic Agents introduced
		{ into the System.
		Typhoid Fever.
		Menorrhagia.
		Scarlatina.

NAME.	STATE.	SUBJECT OF THESIS.
Ratliff, Charles C.	Mississippi.	Intermittent Fever.
Rawls, Elijah H.	Georgia.	Typhoid Fever.
Reeve, James T. (M. D.)	New York.	Acute Peritonitis.
Reese, Charles E.	Alabama.	Dyspepsia.
Riley, John G.	Georgia.	Yellow Fever.
Ringwalt, Samuel	Pennsylvania.	Typhoid Fever.
Rogers, James Henry	New York.	Medical Mutations.
Rowland, Wm. A.	Georgia.	Pleuritis.
Rucker, Wm. P.	Virginia.	Physiology of Digestion.
Runyon, Thomas H.	Kentucky.	Intermittent Fever.
Russell, Leonidas,	Indiana.	Elevation of the Medical Profession.
Rutledge, Jacob I.	Maryland.	Scarlatina.
Sarver, William	Pennsylvania.	Typhoid Fever.
Scott, Daniel S.	Arkansas.	Anatomy of the Human Liver.
Scott, Christopher H.	Mississippi.	Chronic Inflammation of the Uterus.
Selman, David	Mississippi.	Measles.
Shepherd, James B.	Virginia.	Physiology of Digestion.
Sherrod, John I.	Tennessee.	Typhoid Fever.
Shreve, J. Ridgway,	New Jersey.	Varicocele.
Simmons, W. A.	North Carolina.	Gonorrhœa.
Simms, H. C. (M. D.)	District Columbia.	Tetanus.
Sims, Joseph B.	Kentucky.	Typhoid Fever.
Smith, Chauncey M.	New York.	Variola.
Smith, Hosea H.	New Hampshire.	Variola.
Smith, Joseph F.	Mississippi.	{ Physical and Chemical Properties of the Blood.
Spears, Abraham Kellar	Kentucky.	{ Uses of Water in Medicine and Surgery.
Spencer, Cadwallader C.	Ohio.	Asiatic Cholera.
Sproul, Samuel M.	Missouri.	Ovarian Dropsy.
Stavely, Wm. R.	Pennsylvania.	Imperforate Anus.
Stewart, David P.	Pennsylvania.	Puerperal Fever.
Stewart, Samuel F.	Pennsylvania.	Dysentery.
Strachan, Joseph Blackwood	Virginia.	Inguinal Hernia.
Strother, Robert C.	Virginia.	Acute Dysentery.
Swift, D. D.	Pennsylvania.	Gunshot Wounds.
Tate, John M.	Virginia.	Opium.
Thomas, David B.	North Carolina.	{ Epidemic Dysentery in North Carolina in 1854.
Thomas, Robert Y. H.	South Carolina.	Fracture.
Thomson, Wm.	Pennsylvania.	Fracture of the Patella.
Thompson, Kimbro	Virginia.	Inflammation.
Todd, William C.	Pennsylvania.	Delirium Tremens.
Townsend, A. K. P.	Maine.	Dyspepsia.
Turner, Edward J.	Virginia.	Acute Gastritis.
Turner, Thomas	Virginia.	Intermittent Fever.
Upshaw, Wm. T.	Tennessee.	{ Difference between Animals and Vegeta- bles.
Vansant, John	Virginia.	Inanition.
Wallace R. S.	Pennsylvania.	Acute Dysentery.
Warden, Jacob B.	Virginia.	Colitis.
Washington, H. W. M.	Virginia.	Uterine Hemorrhage.
Weiser, Josiah S.	Pennsylvania.	Colo-rectitis.
Welch, Samuel M.	Kentucky.	Use and Abuse of Medicine.
Wells, J. Ralston	Pennsylvania.	Puerperal Fever.
Whalley, James G.	Georgia.	Femoral Hernia.
White, James Z.	Virginia.	Femoral Hernia.
White, Walter T.	Mississippi.	Measles.
Wilkerson, Wm. Washington	Alabama.	Oxalate of Lime as a Urinary Deposit.
Willcoxon, Levi J.	Georgia.	Typhoid Fever.
Willet, E. Miles	Kentucky.	Datura Stramonium.
Willis, George M.	Georgia.	Inflammation.
Winsborough, Jos. W.	Virginia.	Ergot and its Uses in Cases of Labor.
Wolfe, David E.	Delaware.	Dysentery.
Wood, Sidney S.	North Carolina.	Scarlatina.
Woodson, Philip T.	Georgia.	Typhous Pneumonia.
Woodward, Wm. W.	Missouri.	Necræmia.
Worthington, Thomas E.	Kentucky.	Catarrhal and Purulent Ophthalmia.
Young, Alex. H.	Tennessee.	Morbus Brightii.
Young, William P.	District Columbia.	Erysipelas.

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